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Proceedings

THE JOURNAL OF
The Institution of Electrical Engineers

ORIGINALLY

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FOUNDED 1871

INCORPORATED BY ROYAL CHARTER 1921

EDITED BY P. F. ROWELL, SECRETARY.

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The Institution of Electrical Engineers.

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(Corrected 1st February, 1921.)

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PRESIDENT 1920-1921

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Vol. 59. DECEMBER, 1920. No. 296.

INAUGURAL ADDRESS

By Llewelyn B. Atkinson, President.

(*Address delivered* 18 *November*, 1920.)

Synopsis.

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Introduction.

My first personal duty in taking this Chair is to express my appreciation and thanks to my fellow-members for the honour they have conferred upon me in appointing me as their President. In accepting the post of President of the Institution of Electrical Engineers I can assure you I do so in no light spirit, for I am under no illusion as to the responsibilities and heavy duties which it involves ; indeed, I shall later say something on this topic. The pleasure to me of work, of scientific and industrial study and effort, lies in the occupation itself, rather than in its material rewards, and the ambition to achieve public distinction for its own sake is, perhaps unfortunately, non-existent in my make-up. Consequently, in becoming your President I fulfil no personal ambition, but nevertheless it affords me great gratification for two reasons. First, because I rejoice to think that it is possible for the Institution to recognize that it may usefully place its destinies for a time in the guidance of one who is no outstanding figure in science or industry, like many of the greatly distinguished occupants of this chair in the past, but who, may I say, with ordinary abilities has long been a member of, and a consistent supporter of every step which seemed to tend to the betterment of, the great industry which this Institution represents. The second source of gratification to me is a personal one. As a young man I used to attend the meetings of the Institution of Civil Engineers and the Institution of Electrical Engineers with my father, himself a civil engineer, and one who attached the greatest importance to the work of such Institutions, and on more than one occasion he expressed the hope that some day I might be in the chair of one of them. That wish of his is to-night fulfilled.

Since I became the President elect, it has been a cause of some surprise to me to realize the importance that seems to be attached to the Presidential Address. "Have you written your Presidential Address?" I have been asked this question a thousand times. I cannot help drawing from this the conclusion that the work of the President of the Institution is much misunderstood. We have had, particularly of later years, presidential addresses of such importance and merit that in a way they become the keynote or *motif* of the whole presidential work, and if we look back to the earlier presidential addresses, we see how this function has grown in importance.

But the real work of the President does not lie in his Address. For the year of his office he is, as it were, the Chairman and Managing Director of, and the co-ordinating element in, this great Institution ; and the increasing weight of his duties, as I remarked earlier, demands consideration. The original function of the president of any of the Institutions and learned Societies was to take the chair at the General Meeting, held,

say, once a fortnight, and to preside at the Council Meetings, and little more.

In our Institution the meetings have increased in number with the formation of various Sections, some of which at least, in addition to the Ordinary Meetings, the President attends ; there are 12 Centres and Sub-Centres which it is his duty if possible to visit during his year of office ; there are Committees and Joint Committees on the greatest variety of subjects of importance to the profession and industry, and those who have of recent years had the honour of being your President have found that it is becoming for the year of office nearly a whole-time job.

Unless these tendencies can be modified, it will result that few members of the Institution will feel able to undertake properly the office of President, because they will be unable or unwilling to devote the time and administrative effort necessary to the office. I take this opportunity of recording very gratefully the action of those with whom I am associated in the industry, not only in urging that I should accept the office of President when invited to do so and in giving me a free hand as to necessary office assistance, but also by offers of personal help in the carrying on of some of my own work.

I. The Jubilee of the Institution of Electrical Engineers.

The year 1921 will see the Institution of Electrical Engineers attain its "jubilee," and whilst this is a purely arbitrary period to select, the human mind naturally marks off such a period of time for some sort of retrospect and stock-taking, and I propose to say something in regard to the foundation* of the Society of Telegraph Engineers from which our Institution has sprung, and to compare some of the problems then facing electrical engineers with those which it has to consider to-day.

The Minute Book of Council Meetings of the Society of Telegraph Engineers shows that on the 17th May, 1871, a meeting was held at No. 2, Westminster Chambers, Victoria-street, S.W., in the office of Major Frank Bolton, when the following gentlemen agreed to form the Society and appointed themselves as a provisional Committee :—

E. O. Wildman Whitehouse, Esq.
Captain P. H. Colomb, R.N.
Major R. H. Stotherd, R.E.
Louis Lœffler, Esq.
Captain C. E. Webber, R.E.
Captain E. D. Malcolm, R.E.
Robert Sabine, Esq.
Major Frank Bolton.

At the same meeting, the formal resolutions of Constitution being passed, 66 names were submitted and duly elected as members, and of those 66 I believe that only two are now living, viz. General E. D. Malcolm of Poltalloch, R.E., and Sir Herbert Jekyll, K.C.M.G.

* In the 8th Kelvin Lecture delivered by Dr. Alexander Russell there are some interesting notes in connection with the foundation of the Society of Telegraph Engineers (see *Journal I.E.E.*, 1917, vol. 55, p. 1). Mr. J. E. Kingsbury in an Address to the Students' Section in 1915 (*Journal I.E.E.*, 1916, vol. 54, p. 189) also refers to the foundation of the Society.

Prior to the formation of the Society of Telegraph Engineers, there was a Society called the Electrical Society of London, founded on the 16th May, 1837, William Sturgeon (b. 1783, d. 1850)* being in the chair. The object of the Society, like the Society of Telegraph Engineers, was the reading and discussion of papers on electrical matters, but this Society did not last long, for on 22nd April, 1843, it was resolved to wind it up, and the final winding-up was carried into effect on 31st May, 1845. The failure appears to have been due, first of all, to financial difficulties arising from the cost of printing the Proceedings, and secondly, after the first two years, from the apathy of the members, who neither attended the meetings nor invited fresh members to join, so that the Society was in a state of absolute stagnation. A full account of this Society was given in the Presidential Address of Mr. C. V. Walker, F.R.S., in 1876.†

The first General Meeting of the Society of Telegraph Engineers and Electricians was held on 28th February, 1872, when the first President, Dr. William Siemens, delivered an address dealing principally with the objects of the Society, viz. the reading of papers and their discussion by members, and generally an exchange of knowledge and opinions on electrical subjects. On that date it was made clear by Dr. Siemens that he, at least, regarded the Society as a specialist Society and regarded the Institution of Civil Engineers as the Institution of Engineers which would cover all departments of engineers.

Dr. Siemens, afterwards Sir William Siemens, was President again in 1878. He was a man whom as a youth I met not infrequently, being a close personal friend of my father who greatly admired his knowledge and skill, and I may say that it was under his guidance that my engineering education was planned, it being a further part of the plan that when my training was complete I should become one of his personal technical assistants, but, like many other such plans, the unfortunate death of Sir William Siemens, as a result of an accident in November 1883, put an end to it.

I have alluded to the first Presidential Address given by Sir William Siemens setting forth, as he did, his view of the limited functions and operations of the Society of Telegraph Engineers which in the year 1888 became the Institution of Electrical Engineers. Substantially, the Society and the Institution retained that point of view for the first 35 years of its existence ; by a process of evolution or growth, the Institution has, however, reached a position where it no longer stands only for a science, but stands for, and claims to represent, a science and an industry based upon that science. I am aware that there are those, particularly among some of our older members, who regret the change, but there can now be no turning back and the fact must be fully accepted. The Institution of Electrical Engineers is, indeed, the first of the great Institutions to have been forced by the circumstances of its industry into such a position. The Institution of Civil Engineers, for example, is definitely a body to protect the interests

* It is an open question whether Sturgeon or Faraday invented the electromagnet (*see* the late S. P. Thompson's "The Electromagnet," 2nd edition, page 412).

† *Journal of the Society of Telegraph Engineers*, 1876, vol. 5, p. 3.

of engineers as engineers, recognizing and certifying to their engineering training and knowledge. The Institution of Mechanical Engineers may be said, as a broad statement, to stand for the same view, but the Institution of Electrical Engineers, whilst certifying by its membership to a standard of engineering skill, goes further and endeavours by its activities to promote the interests of all its members, in directions which do not necessarily depend on their particular status or skill. This function of its activities has, to a considerable extent, been brought about by the fact that electrical engineering is largely concerned with public services, and hence the intrusion into its affairs of Governmental and municipal regulations in a degree quite unapproached in civil or mechanical engineering.

The increasing and broadening sympathy existing between the Institution of Electrical Engineers and all classes in the industry, may be traced to the time when definitely or implicitly this great difference between it and the older Institutions was recognized and the work of the Institution adapted to the needs of the industry.

The great extension of the activities of the Institution through its 12 Territorial Centres and Sub-Centres, whilst it has raised the Institution to the position of the most active engineering force in the country, has created its own special problems, and notably that of finance. In the past the leading engineering Institutions, including the Institution of Electrical Engineers, have maintained in London a home and have held regular meetings, generally at fortnightly intervals, and published the "Proceedings" thereof, and at that point they have been content. In our own case last session the number of meetings of all Centres and Sections at which papers were read or discussions took place was 105.

These meetings, however, involve considerable expense. The expenses of meetings are twofold; for meetings in our own building only extra lighting, cleaning, and some refreshments are added, but for provincial meetings there is, in addition, rent of meeting places. Above all, however, there is involved the extra printing of all the "Proceedings," and we may reach a point where we run the danger of getting into the position of the old London Electrical Society, which was submerged in the cost of printing its own "Proceedings."

The cost of printing and posting the *Journal* in 1914 was £2 000 per annum, and this has risen until in 1920 it has reached £6 000 per annum. There is little doubt in my mind that some form of drastic editorship of the discussions and "Proceedings" will have to be put in force if the present number of papers is to be published. The alternative is to reduce the number of papers published, a policy which I should be reluctant to advocate, as it would tend to the result that papers on electrical subjects might be taken elsewhere in order that they might be published. The single fact that members of the Institution have to grasp is this, that this Institution is giving more to its membership than any British Engineering Institution has ever attempted; and comparisons of the cost of the working of our own Institution and the necessary subscription thereto with those of other Institutions have no common basis.

Members have recently received notice of the proposals of the Council as to future subscriptions, formulated after the fullest consideration by the late Council and endorsed by the new Council. In these times of increased expenses in every direction the Council have been naturally very reluctant to increase the subscriptions, but it would have been necessary earlier if it were not that the Institution has been receiving a rent for the use of its building; but the work of the Institution and its usefulness to members has, in my opinion, been greatly hampered in consequence.

In 1917 the incoming President, my old fellow-student at King's College, Mr. C. H. Wordingham, dealt very exhaustively with some possibilities of activity for the Institution. Some of those such as the Informal Meetings, and I may add the Wireless Section, have been inaugurated; others, which I entirely concur with, have had to wait until we get back to our own building. That event I believe will occur now in the near future. The Council have consistently pressed the Office of Works to vacate the building, and have used every kind of pressure possible to reach this result. But, even when possession is obtained, the building will require entire redecoration before it can be fully used for the full work of the Institution. For the immediate future I feel that great advances in the Institution's activities having been so recently effected, a period of digestion is necessary before any further radical changes are made.

The question of registration of engineers was prominently before the Institution Council last year and the matter was referred to in the Council's Report. There was some criticism of the fact that the members were not kept advised of the course of the discussions with the Institution of Civil Engineers, but it was difficult to do much in this direction. The Council were not only in delicate discussions with the Civil Engineers but also with several other engineering and scientific Societies, the form of the Bill was changed at least twice, the interpretation put upon the wording of the Bill varied almost proportionately to the number of interpreters, and finally, when the course seemed clear to make a communication to the members, the Bill was dropped.

The suggestion made, that the Council contained a number of Members of the Civils who were considering the interest of this body first, is absolutely preposterous. There may be a division of opinion on the Council or in the membership as to whether engineers should be registered, but when the time comes to settle the principle the members will be invited to take a hand in settling it. In any case, whilst the Council are quite open to discuss the question, as our late President stated on several occasions publicly, it will be a *sine qua non* that this great Institution takes in any such scheme the position to which it is entitled.

Among the difficult problems which the Institution will have to face is the fact that for some four years the war stripped the colleges and training institutions of their students. Many of these students will never again take up or complete their student courses, and the result, not possibly seen at once, will be that in a certain number of years it will be found that there will be a

dearth of men coming forward with the necessary qualifications for full membership of the Institution.

II. The Electrical Progress of Half a Century.

Having thus dealt with the Institution and its immediate problems, I propose to take some of the principal departments of electrical engineering and to compare the position at the time of the founding of our Institution and the past problems which have already been solved, with the position to-day and those problems which are to-day awaiting solution. Principally shall I deal with the technical aspect, pointing out in each what seem to me to be the essential milestones which have been the achievements of a period and the start of a new period. It is true that each of these departments should be represented and spoken for by a specialist, but what I desire to do is to give a panoramic view; and this may be done, perhaps, even better by one whose tastes, training, and occupation have led to a broad outlook rather than to a specialization in any one branch.

(1) *Physical and electrical science.*—We may first briefly review the change in our ideas on electrical and physical science since the Institution was founded. It was in 1873, the second year of the life of the Society of Telegraph Engineers, that the first edition of Clerk Maxwell's "Treatise on Electricity" appeared. In this treatise, electric phenomena as developed experimentally by Faraday and others were presented in the form of a mechanical and dynamical science, dependent for their effects on strains in dielectric materials and inertia effects in a medium (ether), these effects being propagated, not instantaneously as had been believed till then, but progressing in time, that is, as a wave.

That treatise was the starting point of modern electrical theory. Up to that date the development by the great experimental and mathematical electrical workers had been along the line of discovering and calculating the effects of certain phenomena, each considered by itself, but this treatise at once placed the whole science on an entirely different footing. It is perhaps unfortunate that the work was not written in two parts: first, those chapters which describe, discuss, and generally summarize the argument; next, those chapters which set out the results in mathematical form and by analytical processes arrive at further results; and separated from, or as an introduction to these, the chapters descriptive of new analytical processes necessary or useful to be used in solving some of the problems arising. I say I think it is unfortunate this subdivision was not made because it is my experience that many engineers have never studied Maxwell's work, from which they might have derived not only great advantage but great pleasure, owing to their having been dissuaded from doing so by the formidable nature of much that occurs in analytical parts of the work, whilst the principles may be clearly understood with only a general knowledge of the mathematical methods adopted.

Whilst as a result of this treatise the physical and dynamical science of electricity appeared to be put on a firm basis, a good deal of experimental work had to be done before some of the deductions arising from this result could be verified. I refer particularly to all that is involved in the expression "The Electromagnetic Theory of Light." I need hardly say that at that date there was absolutely no knowledge of what actually constituted the electric current, but the work on electrical discharges in rarefied gases of Spottiswoode, De la Rue, Gordon and, above all, Crookes, and later of Lenard (1893), and Röntgen (1895), terminated by 1896 in the discovery of the electron and the part it plays in the phenomena of the electric current.

To-day the work thus started with Maxwell has been largely completed. Many writers, and notably Oliver Heaviside, followed up what Maxwell had commenced, and completed the working out of the problems arising from the dynamical solution which Maxwell had given us, and the elucidation of the effects arising from the transmission of electromagnetic stresses and strains propagated under a great variety of conditions as waves in an elastic medium. From the experimental side the results predicted received their full proofs with the work of Hertz in 1885–1887, which at once opened up the possibility of wireless telegraphy, and to-day our knowledge of the fundamental basis of electricity, viz. the electron, has reached a high degree of certainty and precision of measurement, leaving us with the unsolved problem which electrical science is doubtless called upon to solve, viz. the nature of the atom.

Tied up with this problem will lie the complete solution of the problem of temporary and permanent magnetism. We had last session a very able and suggestive contribution by Mr. Evershed to the latter discussion, and I think the suggestions put forward by him were a great stride forward. The discovery of radium and radioactivity in 1898, and the proofs that chemical atoms are not, as till then supposed, eternal, but are subject to change, radioactive atoms themselves breaking up into other chemical atoms and electrons, was no less than an absolute revolution in thought.

Investigations which have been made in connection with the electron have led to a revision of our ideas on some questions of mechanics.

Newton's fundamental definitions in mechanics involve that the acceleration of a body is measured by the ratio of the force acting on it to its inertia mass, and Newton treated the inertia mass as a property appertaining individually to every particle of matter, fixed and unchangeable, and the whole of the Newtonian mechanics is built up on this as the basis. Measurements on the motion of electrons when moving at a velocity comparable with the velocity of light, show that the ratio of the force to the acceleration is not constant under these conditions, or, in other words, what we call the inertia mass of an electron is not a constant quantity.

Following the investigations of Sir J. J. Thomson, published in 1881, on the quasi-inertia of a moving electric charge, we have come to realize that we cannot regard the electron as something by itself, having a fixed inertia mass. The electron cannot move without setting up movements in the ether involving the storage of energy therein, and the amount of those movements depends on the acceleration and also on the velocity

of the electron. The inertia mass of the electron therefore is in part or wholly an expression of a rate of storing energy in the ether. Experimental investigation seems to show that the whole mass, or apparent inertia mass, of an electron may be thus accounted for, and it is clear that at least some part, if not the whole, of the inertia mass of all matter arises from the energy effect of the ether disturbances which take place when matter is accelerated, and these disturbances being transmitted with a finite velocity the inertia mass of all matter in whole or part depends on the velocity with which it is moving.

It is quite clear, therefore, that we as engineers have to accept considerable changes in the views on which the scientific training of most of us has been based.

The experiments, if we may so call them, of astronomical movements seemed to have established long ago the certainty of Newton's conception of constant inertia mass and of gravitational forces proportional to this mass acting thereon, and yet, as I have explained, the experiments of the laboratory on the smallest masses of which we have any experience has shown discrepancies as to the constancy of mass of those masses.

If the inertia mass of matter depends on its velocity of motion, since all experiment so far has shown that the gravitational attraction is proportional to the inertia mass, the question arises: is the gravitational attraction dependent on the velocity? The possibility of experimentally proving this appears remote.

The early electrical writers based the theory of electricity on an isolated unit quantity of positive electricity as their standard for measurement and calculation, whereas it was subsequently shown that, whatever might be the unit of positive electricity, there never was a unit of positive electricity without a corresponding unit of negative electricity bound together and connected by an intervening electric field. It seems, therefore, that we have to alter our view of the nature and properties of matter. The Newtonian view that there are isolated units of matter, each with its own inertia and gravitational mass, must give way to the idea that probably there is no such inertia mass except as part of a complete system embodying all the masses in the universe and whatever medium lies between them, and that the inertia mass of any particular piece of matter depends on the relative acceleration and velocities of all the masses in the universe, and that matter as measured and expressed by inertia mass is possibly convertible into what we call energy, and vice versa.

Experiments on the velocity of light made under conditions where the observer and his apparatus move with the same velocity as the source of light, are consistent with the Fitzgerald-Lorentz theory that all material bodies are shortened in the direction of their motion through the ether; hence, strictly speaking, we cannot speak of a length as constant except in a restricted sense.

I will not proceed beyond mentioning that Einstein, and those who are working on the same subjects classed under the "Theory of Relativity," have arrived at certain general expressions for the laws of physics true for all systems of reference in whatever state of motion they may be. In the endeavour to give some quasi-physical interpretation to the formal equations, they do so in terms which involve Time as a variable; that is, something which depends on where we are and the state of motion we are in. Whilst the physical variability of inertia mass and of length in the circumstances given above do not violate our fundamental conceptions, this latter does so, and certainly engineers will agree with Sir Oliver Lodge, who said at the British Association meeting: "Equations should be expounded in terms of matter and ether and not in terms of space and time."

Whilst, as I have said, it is necessary that engineers should prepare their minds for great changes in their conceptions on these matters, this need give rise to no misgivings or uncertainties about the ordinary results of mechanics and physics as applied to engineering. We may perhaps regard it in this way, that a complete expression for the mass or the energy of matter involves a series of terms, terms involving the velocities and accelerations relative to one another of all the matter in the universe. Such a series involves terms some of which only become of importance for velocities comparable with the velocity of light, and others which at more moderate velocities produce disturbance not comparable with the terms that are of importance to us within the limits of ordinary experiment. It appears, for example, to be moderately certain that the kinetic energy of a mass of matter in motion is not to be expressed by the statement "Kinetic energy equals one-half the mass multiplied by the square of the velocity," the velocity in such cases being taken to be the absolute velocity with reference to a supposed stationary ether. It is probable that the true form of expression is a series in which there is a first term of great but unknown quantity dependent on the internal energy of the matter itself; the second term is the term already mentioned as the ordinary complete statement, but treating the velocity as the velocity relative to the earth; while other terms of smaller importance at moderate velocities would follow. But, as the first of these terms cannot be measured or investigated by us with our present knowledge, it is of no importance to us. The third and higher terms are of no importance to us because they are so small; and we are left just where we were for all ordinary purposes with the one term which we can measure and evaluate.

(2) *Generation of electric currents by mechanical power.*—In 1867 Wehrner Siemens and Sir Charles Wheatstone simultaneously published their respective discoveries or inventions of the shunt and series self-exciting direct-current dynamos, the approach through the permanent magnet generator and electromagnetic generator with permanent magnet exciter having led up gradually to these, and in 1870 the re-invention by Gramme of the closed-coil armature winding described to the French Academy of Science in the year this Institution was founded, gave a fresh impetus to the practical construction of what at that time was considered to be large machines (3 kW). The year 1873 saw the drum armature winding, and whilst from that time empirical and experimental progress was steady

the rational development of the dynamo waited till 1885, when Dr. John and Edward Hopkinson gave a complete solution of the determination of the magnetic flux in the nearly complete magnetic circuit of a dynamo where the path of the flux could be very closely defined.

Up till 1885 the direct-current machine had generally had the armature winding laid on a smooth core, and copper brushes for collecting the currents were used. The tramway motor driving through tooth gearing, however, necessitated a more robust construction ; the slotted armature which had been tried but abandoned owing to the sparking at the commutator, was revived and, by help of the carbon brush which since about 1900 has excluded all other types, became practicable. Commutating poles which also had been used in 1885, but which at that time were regarded as only necessary because the design was incorrect, returned in 1905 and are in general use to-day. The characteristic direct-current generator which has evolved is the multipolar type with commutating poles and carbon brushes.

Alongside of these developments came the invention of the transformer by Gaulard and Gibbs, first exhibited in London in 1883 ; and the possibility thus opened of distribution at high pressure and use at a lower pressure, led to a renewed interest in alternating-current generators. In 1883 Dr. John Hopkinson showed mathematically that, contrary to general opinion, alternators could be run in parallel, which was experimentally demonstrated by Professor Grylls Adams (1884) with machines at the South Foreland lighthouse, in which experiments I had the advantage of acting as his assistant. The year 1886 saw Tesla's two-phase alternating-current motors, and 1890 the three-phase or *drehstrom* motor and system of Dobrowolski, and in 1891 the demonstration of 100-kilometre power transmission at high pressure from the Lauffen falls on the Rhine to Frankfort, which I well remember visiting, completed the demonstration of long-distance transmission of light and power by high-pressure three-phase alternating currents.

The standard alternator to-day is the three-phase alternator ; not only the advantage of larger output for a given weight, but the absence of an alternating flux in the pole-pieces of the generator which arises with single-phase alternators, have contributed to this result. The use of closed circuits of high conductivity surrounding the pole-piece, called by Leblanc who first proposed them *amortisseurs* or dampers, have, however, enabled single-phase alternators of any size to be made in which this trouble does not arise. Multipolar machines with fixed generator coils and revolving poles are now the standard for low-speed engines, but for turbine-drive a rotor magnet with a single pair of poles and a winding distributed over the pole-faces, and a fixed armature or stator, a construction introduced about 1906, is the type which has become standardized.

The improvement of high-speed steam engines and their adaptation to the direct driving of electric generators have been largely accomplished by electrical engineers and members of this Institution. Two types in particular have contributed to the progress made, the high-speed single-action enclosed reciprocating engine associated so closely with the name of Willans, which sufficed till engines over 1 000 h.p. were required, and the steam turbine of Parsons first introduced in 1884, which has now become universal for everything over about 1 000 h.p.

Although the earlier turbine engines running at even 18 000 r.p.m. were coupled to direct-current dynamos having commutators of small diameter and few parts, yet as soon as large powers were required, the increased number of parts and diameter of the commutator raised insuperable difficulties, and the electric current is now always generated as an alternating current (as indeed happens primarily even in direct-current dynamos), and if direct current is required this is obtained by rotary converters, really a device for commutating all or part of the current at a lower revolution speed, or quite recently by making use of the rectifying property of the mercury arc. Arc rectifiers in metal containers up to 200 or 300 kW are now working quite satisfactorily.

The progress of late years in the generation of electricity has been in the direction of enlarging the units, with considerable increase in efficiency by so doing, with a diminution in the amount of attendance necessary, but it appears probable that we have now reached the present economical limit in this direction, as the cost of building very large units of plant and getting them on to the site begins to outweigh the other advantages from the use of such large units.

Of the strictly technical problems of the future, the first is the more complete realization of the value of our fuel. The change of the last seven years in the value of fuel, not only measured in terms of currency but measured in terms of human effort, has turned upside-down all previous considerations, and the immediate problem of electrical generation to-day appears to lie in the direction of more complete utilization of fuel by selective distillation with recovery of useful products, and in the use of gas and coke in the boiler furnaces or by complete conversion of the coke into gas for use in gas engines. This latter part of the problem has indeed progressed very slowly, and we can hardly be said at present to be within sight of gas engines in any way comparable in matters of cost, convenience and running expense, with the steam turbine of to-day.

The gas turbine has indeed made some progress, generally on the lines of multiple explosions discharging gases into a turbine wheel, but, speaking without detailed knowledge of the latest developments in this direction, I believe that the future gas turbine will work on a cycle of combustion at constant pressure with adiabatic expansion through nozzles giving the gas the necessary velocity for a turbine wheel, the constant pressure for combustion being obtained by pumping the air and gas to the turbine through regenerators heated by the waste heat. I am aware that this class of cycle has had consideration and been rejected largely on the losses incurred in the compression part of the cycle, but the inherent advantages of such a cycle for large units are so great that I cannot help thinking that sooner or later the difficulties of the compression part of the cycle will be overcome.

Much experimental work, however, remains to be done, particularly on the question of the form of the expansion nozzles and the action of gases expanding in them. Some experiments show that a large part of the work done in expansion is utilized in heating or rather maintaining the heat of the gas, which is an indication that the energy is first converted into internal rotational energy ; in other words the heat energy is not converted, as it should be, into the mass movement in the direction of the nozzle which is necessary for high efficiency in this type of turbine.

I might mention the efforts which are being made to give wider ranges of temperature of the working fluid in heat engines by the use of mercury vapour, by which it is claimed that 44 per cent greater output from a given weight of fuel has been obtained on 1 000 kW of plant, a notable advance, or by still higher pressure of steam, with boilers having revolving tubes to facilitate the release of the steam. I might mention improvements in condensers to maintain economically the highest vacua, but, after all, these cannot carry us much further.

The heat engine and dynamo as the elements for transforming the energy of fuel into electric currents have reached a stage of efficiency where there is little further to be hoped along the present lines, and in the direction of output for a given weight of material we have reached the point where, notwithstanding the small loss which is transformed into heat, the output is really limited only by the necessity of surfaces to dispose of this heat. Last session we had a very suggestive paper by Mr. Shepherd which, with the discussion pivoting on it, indicated that the next step will be the complete water cooling of the machinery, again enhancing the possible output.

There still remains the possibility of some completely new process, which will eliminate the heat engine and dynamo and substitute some other method of producing the electron stream that we call the electric current, by a process which does not involve the efficiency limits imposed by the second law of thermodynamics, as, for example, the possibility of electrically unlocking the stores of energy in the atom.

In this connection I cannot overlook the possibilities suggested by what is called the photo-electric effect. When light of certain wave-lengths falls on a metal the metal becomes positively charged, electrons being liberated. As far as we know, the energy which the liberated electrons possess is greater than that supplied by the light wave, if the energy of the wave is uniformly distributed. The " quantum " theory assumes that the energy of the wave is concentrated in finite portions, which are sufficient to account for the result ; but this seems to me a very forced conclusion, and one much less likely to be true than an explanation which is based on the idea that there are great stores of energy in the atoms, and that by the application of a small resonant effort we can release this energy at the expense of the internal energy of the atom. Photo-electric cells capable of generating small currents have been produced, but the whole subject is as yet waiting further research.

(3) *Supply from central stations and transmission of power.*—Electrical science and engineering found its earliest applications in the production of an effect at a distance, because it is in this direction that it produces effects that cannot be produced in any other way. In telegraphy and telephony the effects are small, and it is the rapidity of transmission which counts ; whereas in the distribution of heat, light and power it is the space traversed which is important. The possibility of producing all motive power in places or under conditions where it is cheap and convenient, and of distributing it with little loss had been an ideal long looked for. Clerk Maxwell, when asked shortly before his death what he considered to be the most important discovery recently made, replied " the reversibility of the Gramme machine." In 1878 when Sir William Siemens became President for the second time, he devoted a considerable part of his Address to the description of an experimental transmission of power over a distance, when 2·3 h.p. was delivered with an overall efficiency of 40 per cent.

Up to 1881 very confused ideas as to the possible efficiency of power transmission were current, and the first clear statement was made by Ayrton and Perry about that date, when they showed that the idea that the efficiency could not exceed 50 per cent was entirely fallacious. Once the idea was clear that the loss depended on the current and resistance of the line, and the power transmitted on the initial pressure, the course was clear ; and, after the demonstration at Frankfort in 1891, the electrical transmission of large amounts of power over long distances was only a question of gradually improving technique.

This country by lack of water power, and by the smallness of the distances, does not lend itself to striking examples of such transmissions, but, on the other hand, the density of population and the universal demand for light and power in an industrial country like Great Britain do lead to the development of power on a large scale and to its distribution.

The history of this development in Great Britain is curious. Although, after 1873, rapid progress was being made by electrical scientists and more empirical inventors, engineers in general and the public knew nothing of it, and hence the Paris Exhibition in 1881 when the electric light burst upon an astonished world had an extraordinary effect. The Crystal Palace Exhibition in 1882 and other exhibitions at which electric light and motive power were shown, followed one another rapidly and focused an immense engineering and financial attention on the subject. The first central station in England was that set up by the Edison Company on Holborn Viaduct in 1881, and the result of all this was the Electric Lighting Act of 1882. I need not comment on the result of this Act or the amending Act which followed ; what has been effected has been done in spite of and not by the help of these Acts.

The *Journal* of our Institution marks the progress made in theory and practice of central station design, among which I cannot overlook the influence of the Grosvenor Gallery supply station (1885). The work of our great constructing firms and of the unique school of British cable manufacturing has translated those

designs into practical accomplishments, the initiating foresight and financial ability of a few men have combined these elements into commercial propositions and immense public benefit, but, through it all, runs the political misunderstanding which bound up a technical instrument for annihilating space, with the narrow space limitations of the municipal and urban boundary. The war, the universal solvent, roused even the politician to the truth, and now we have, under the newly-appointed Electricity Commissioners, to begin again. The problem to be solved is : What is to be scrapped and what is worth preserving out of the medley of generating stations in existence, how are the best to be linked up, and what is to be added of new, and who is to initiate, finance, and control the new ? For 40 years we have worked in the wrong direction, many voices having declared it, but the pride of locality, and the fear of exploitation, have deafened the ears of those who were in control. The task is a most formidable one. Considerable progress is being made in the first part, viz. the organization of areas for linking up, but the second stage is the more difficult. What is to go—who is to control ? Every element of jealous humanity is there ; much diplomacy, much goodwill, and some force will be necessary for the solution. The third and last stage, the provision of new and additional generating stations, is that which will give rise to the greatest engineering and technical interest and discussion.

The scientific and directional staff of the supply industry.—The discussions and uncertainties surrounding the future programme of electrical generation and distribution have had and are having a very serious and detrimental psychological effect on the engineering and directional staff of the supply industry.

The position of these staffs, especially in the smaller municipal undertakings, has never been very happy. Men of considerable engineering ability, but perhaps not so well equipped as in the case of larger undertakings with commercial or diplomatic talents, have had to contend with an environment in which local and labour politics formed a dominating feature. While in many cases the difficulty was minimized by the help of a sympathetic committee, or chairman of committee, the result has been in a great many cases to produce a final condition of semi-apathy.

This has been greatly aggravated in the last year or two, when, in the belief that all the smaller stations were to be swallowed up, even the higher staffs of company-owned undertakings as well as the committees of municipal concerns have lost their interest, which in the latter case was often a personal or local enthusiasm, and have simply carried on with a minimum of exertion. Whilst it must be admitted that the appointment of the Electricity Commission and all it involves has been loyally accepted, largely due to the confidence which the personality of the Commissioners inspired, yet even this has led to an attitude of mind whereby the Commissioners have come to be regarded as the *Deus ex machina* who shall solve all problems and resolve all troubles.

Engineers of small undertakings are generally the commercial managers as well, but their work and pride are on the engineering side, the generating station and distribution system. The realization that sooner or later the generation will cease and the distribution only remain, and that salesmanship rather than engineering skill will be the feature looked for, is naturally not very agreeable to these men. This class of men are in fact going through what the craftsman goes through when machinery replaces his skill, and they feel very much as he does when he is assured that eventually machinery causes more employment, not less. We must face the fact that one reason why the larger generating stations will be more economical than the smaller ones, is the decrease in the total amount of skilled superintendence that will be necessary, and that this class of occupation will diminish. The compensation must be found in the increasing use of electric current, and the engineer's function must be diverted to all that these involve in finding new openings, designing equipment for new operations, and in stimulating consumption and the distribution therefor.

(4) *Cables for power and lighting.*—A survey of the changes which have occurred in the lifetime of the Institution would not be complete without reference to insulated conductors, although there has been perhaps less apparent change here than elsewhere. In 1871, telegraphy was the application of electricity needing insulated conductors, and here gutta-percha coverings were widely used. High insulation resistance under conditions where excessive heat was not likely made it a very useful and suitable substance ; rubber-insulated cables were also used. Naturally, when electric light and power appeared, the same cables were adapted, but the heating of such wires made gutta-percha quite unsuitable. The rubber-insulated cable, and rubber mixed with or treated with " ozokerite " at one time had a considerable vogue. About 1885 came the early bitumen-insulated cables which suffered like gutta-percha from softening and decentralization of the conductor with heat, until the process of compounding bitumen with resin and other oils and sometimes with rubber and the vulcanization of the mixture was perfected. Systems such as the Crompton, employing bare conductors in iron or earthenware troughs with porcelain insulators, had at one time a quite extensive use, but it has been found difficult to prevent electrolytic actions, and several serious explosions due to electrolytic gases or coal gas leaking into the conduits prevented its extension. In 1890, Ferranti produced the paper-insulated high-pressure main for use in connection with the Deptford station, this being a concentric conductor with rigid internal rod or tube and external tube made in lengths and screwed together. Twenty-five miles of this conductor working at 10 000 volts were running in connection with the Deptford station in 1891. Before, however, any considerable length of this conductor had been made the oil-impregnated paper-insulated stranded-conductor cable with lead covering was developed and the manufacture commenced by the British Insulated Wire Company, and a definite and permanent type established.

The results of the trials and experiments on a great many types of cables have led to the result that to-day

it may be said that the vulcanized rubber-insulated cable holds the field for internal work, that for underground mains at low and medium pressures vulcanized bitumen cables or paper-insulated lead-covered cables are standard practice, whilst for high pressures paper-insulated lead-covered cables are used exclusively. The early paper lead-covered cables did not aim higher than 11 000 volts, and to-day the British Engineering Standard tables do not go further, but cables of 20 000 and 30 000 volts are extensively used, cables of 50 000 or 60 000 volts can be obtained from responsible manufacturers, and were there a real necessity cables of 100 000 volts would be forthcoming commercially, either of ordinary types, perhaps with graded dielectric, or with pressure-distributing metallic layers forming part of the dielectric enclosure.

Whilst the types of cables enumerated, which were established long ago, have by a process of elimination become standardized, it must not be supposed they have not developed. Constant research work and experiments have been devoted not only to the materials employed but to the method of manufacture, and above all to the improvement of methods of installation of jointing and protection, a large part of the trouble with cables being with these accessories.

Though not strictly a cable question let me refer to the pioneer work of Price, developed on the North-East Coast, of ring-main high-pressure feeders with discriminating protective devices. With various detail modification this method gives, with a minimum of mains, an almost absolute immunity from breakdown for the individual consumer. This system is now attracting considerable attention abroad, where so far no such security of supply has been realized. In most cases special forms of cable are being used, but efforts are being made to adapt the principle to ordinary cable systems.

The future progress seems to lie in the direction of the improvement of the material in two directions. The researches of Evershed on characteristics of insulation and of Addenbroke on dielectrics, which have been published through our Institution, have shown that the limits of dielectric strength and the losses of power in dielectrics under alternating pressure are due to the presence of moisture. The organic cellular structure of paper leads to its being very hygroscopic, and the greatest difficulty is found in drying out all the moisture and maintaining the dielectric moisture-free at all stages of manufacture. There are organic threads which are practically non-hygroscopic, but they cannot be made into papers or woven for the very reason that their surfaces are so smooth, and so it would appear as if either papers must be chemically altered or that a new method of applying felted fibrous materials and then securing in place must be devised to use such fibres.

In addition to such lines of research in any case the constant improvement of quality of paper in the sense of elimination of weak spots is taking place. The dielectric strength of paper as used for cables is, say, 500 volts per mil, but the dielectric is only operated in cables at, say, 50 volts per mil, a wide margin being necessary to cover the inequalities of the material and the irregular and unexpected rises of pressure to which they are subjected. The great fall in the price of rubber, from 11s. 6d. per lb. in 1911 to 1s. 6d. per lb. in 1920, if permanent, may have a bearing on future types of extra-high-pressure cables.

(5) *Telegraphy by land and sea.*

Land telegraphy.—At the foundation of the Institution the electric telegraph was well established and communication carried on in the case of small offices or railways by single-needle instruments, in the case of more important services by Morse ink-writers, and for heavy services by the Wheatstone automatic machine which operated by perforated ribbons, and enabled the messages prepared by several operators to be transmitted over one line and printed in Morse code on tapes at the receiving end.

In 1853 the principle of duplex working, that is, sending a message along a wire in two opposite directions, had been invented, and in 1877 the diplex or sending of two messages in the same direction at the same time which had already been theoretically foreseen, was superimposed, thus forming the quadruplex system.

With these additions the working capacity of most lines was sufficient for the requirements, even with hand signalling at about 40 words per minute for each operator. But, nevertheless, on certain lines the traffic is heavy enough to justify special methods, considered generally as high-speed telegraphy.

Two broad principles have been worked upon. They both depend upon the principle that the number of signals that can be transmitted over the wire is greater than a hand operator can produce or a sounder reader can write, and so we have automatic systems like the Wheatstone where several operators prepare messages, these being sent over the lines by an automatic sender and received by an automatic printer of some kind. In contradistinction the hand-sending remains, but the line is given in turn to several operators, the receiving sets being coupled in synchronism, all the sending and receiving sets working at the same time, and all operators having the line for some part of the time taken to give one signal by hand. This is called multiplex telegraphy. Built up on each of these are methods of producing the signals by type levers as in typewriting, and of decoding and printing the signal in typed characters on slips or sheets. In practically all these systems a letter is composed of five signals instead of any number between one and four as in the Morse code, a return to a system used in early needle instruments, but one which for the purpose in view has the great advantage of equal times for all signals.

In 1914 the Postmaster General appointed a committee to consider the question of high-speed telegraphy, and they reported in 1916. The definite conclusion reached was that for this country the multiplex system is superior to the automatic, that a special apparatus named in the report included all the features considered desirable, viz. a free keyboard perforator, five-unit code, and printing direct in page form. Such an apparatus working duplex with quadruple sending and receiving sets at each end would despatch from 40 to 50 telegrams per hour per operator, that is 160 to 200

messages each way per hour. One such apparatus dealt with two-thirds of the whole Manchester–London traffic, and very few circuits could keep one apparatus occupied. Hence we may certainly say that, even for crowded traffic, invention is really ahead of requirements. The use of apparatus like high-speed telegraphic apparatus is, under Post Office conditions, complicated by staff questions, and the practice in the Civil Service of selecting the higher grades by seniority and not by technical or temperamental merit, thus making it difficult to ensure that the overlooking of a staff is done by one specially skilled in working and adjusting a number of circuits.

Probably the most needed apparatus to-day is a cheap and simple transmitter worked from a typewriter keyboard, and a cheap and simple printing receiver to replace the hand-operated sounder instruments of smaller offices, for the reason that there is an ample supply of people able to use typewriters, and the printing receiver obviates clerks skilled in reading Morse code. For the smaller offices, the telephone has replaced the single-needle instrument which was used.

As is well known, the speed of signalling at telegraphic frequencies depends inversely on the square root of the product of the resistance and the capacity, so that as these depend on the length of the circuit, greater speed is obtained by dividing the line into sections, and inserting repeaters to start the signal afresh. The latest form of repeater is the thermionic valve tube, so arranged that a small incoming disturbance causes an amplified local current to work an ordinary electromagnetic relay. The effect of this with similar valve relays on the receiving instrument is to enable much smaller currents to be used, so that it is contemplated to use conductors of hard-drawn copper weighing only 20 lb. per mile for all telegraphic circuits, instead of conductors weighing as at present, say, 200 lb. per mile, with such repeaters, say, 100 miles apart.

Submarine telegraphy, like land telegraphy, had reached a high degree of advancement when our Institution was founded. The principle of the design of cables had been well worked out. The Atlantic cable had been successfully laid in 1865, and it may be said that no new design of cable (except loaded cables, which I shall refer to later) has been introduced, and that the change in this section has been the continued extension of cable routes all over the world.

It is a source of gratification that nearly the whole of these cables are due to British initiative, and are of British ownership, whilst in the construction of long deep-sea cables at the present time no country other than England is manufacturing such—though it may be expected that Germany will re-enter the field, and Japan and U.S.A. are considering doing so.

In 1871 the mirror galvanometer and siphon recorder were the reading instruments used, and with them a speed on the Atlantic cable of 15 words or, say, 75 letters per minute was obtained.

Heavier conductors, improved methods of working, the use of inductance shunts, and the use of very sensitive relays such as the Heurtly and the Brown, have brought this speed up to 300 letters per minute under duplex conditions of working, and permit of Morse printing instruments being used, whilst the ubiquitous thermionic amplifying valve is producing a further increase due to the possibility of operating it with a smaller current than any other relay, and so enabling a smaller fraction of the incoming and increasing current at the receiving end to be utilized; in other words each signal can be shorter.

(6) *Telephony.*—At the founding of this Institution the telephone did not exist. We know that in 1862 P. Reis had invented a telephone that we, with present knowledge, can make talk, but there is no evidence, which is generally accepted, that it ever was used by him for transmitting speech. In the year 1876, Sir William Thomson, described at the British Association meeting the Graham Bell telephone he had seen in America, and in that year I was present when the father of one of my school-fellows completed a pair of magnetic telephones as described by Thomson, with parchment diaphragms and a small ferrotype centre, and established communication between different rooms in the house. The first scientific paper I ever read was on the telephone, and that was when I was still at school. In 1877 Bell himself gave an account of his researches and inventions to the Institution.

The most remarkable thing to my mind about the telephone is, that despite countless experiments on all types of receiver the receiver remains practically as Bell introduced it, and the transmitter has varied very little for the past 20 years, though the proportions of both have been very carefully studied and improved.

The first fully equipped telephone exchange for general service was opened for business in January 1878 at New Haven, Connecticut, and at 1st January, 1914, nearly 15 million telephone connections to exchanges were in existence throughout the world.

Quite early the necessity of complete metallic circuits for telephone work was recognized, and it was thought that the cost of this would seriously affect the spread of telephony, particularly on long trunk lines; but the invention of Jacob in 1882 of using two complete metallic circuits as the metallic circuits for a further telephone, known as superposed or phantom working, largely obviated the expense, allowing of three telephones on two complete circuits, though the use of this principle did not come into use till later, and its application to artificially loaded cables, either land or submarine, has only been worked out within the last few years.

In 1887 Heaviside had shown the conditions giving rise to attenuation and distortion of telephonic transmitted impulses, and showed that distributed capacity and inductance in certain proportions minimize both, and in 1899 Pupin analytically and experimentally established the conditions necessary to embody these ideas practically, by inductances placed at intervals on the line, and the first application in practice was in 1902. The year 1891 also saw the introduction of cables for underground use with dry paper insulation giving very low capacity.

The adaptation of inductance coils to overhead land lines in England was a matter rather of permitting less copper to be used than an absolute necessity, and it was so used by the National Telephone Company

and the Post Office, but the necessity arose when it was decided to put the trunk telephone lines underground. In 1915 the Post Office had connected London and Birmingham by underground cable, with loaded cables with inductance coils at intervals of $2\frac{1}{2}$ miles.

The application of inductance to submarine cables was first made in a different form. Continuous loading by surrounding the wire with an iron covering was adopted and the first loaded cable laid in 1906 under Lake Constance ; other cables on a similar principle are the French cross-channel cable (1911) and the Vancouver–Victoria cable (1913).

The construction of telephone lines or cables of any length and under any conditions may be said now to be only a matter of expense, but in view of the extension of wireless telephoning—to be referred to later—it is improbable that the expense will ever be incurred for long-distance transmission by submarine cables. The first submarine telephone cable with inductance coils at intervals was the cross-channel cable laid by the British Government in 1910.

What are the problems now waiting for solution ? These are perhaps as follows :—

More research is required on the nature and production of vocal sounds, so as to lead to some method of production and measurement which will enable accurate tests to be made of telephone transmission and instruments independent of human estimation.

To-day we use as a unit the standard line, a loop line of copper 20 lb. per mile (0·036 inch diameter) having per mile a resistance of 88 ohms, a distributed capacity of 0·054 microfarad, a distributed inductance of 0·001 henry, and a distributed leakance of 1 micromho.

Tests over other lines are made by a trained speaking operative and a trained listening operative, by direct substitution of the line to be tested and the standard line, till we can say the line being tested is equal to so many miles of standard line. It has proved a useful method, but we ought not to feel satisfied till we have some more absolute measure than this gives. Progress is being made with research on these subjects.

The subject of multiplex telephony, apart from the limited superposed telephony already mentioned, is also one of the coming problems. Some success has been achieved by a method which has been called " wired wireless telephony." It is a bad name. The idea involved is the old idea of multiplex telegraphy dependent on the passage over one line of a number of separate signals each sent into the wire by oscillating current impulses of definite and different frequencies, and the passage of all these currents through all the receiving instruments, and the selection by each instrument of the impulses to which it is tuned. But the method, as it applies to telephony, involves a further idea borrowed from wireless methods which I may thus explain.

As a telephone necessarily responds to all frequencies within its range, the currents transmitting the messages must be of frequencies which will not affect any telephone, such frequencies as 10 000, 20 000, or 30 000 per second, compared with 800 taken as representative of the human voice.

The telephonic variation from a given transmitter is superimposed on this oscillating current, and up to this point will affect none of the connected receiving telephones. If now one of the many telephones is included in a circuit tuned to one of the selected frequencies, the oscillating current will be collected by that circuit and, if a rectifier is inserted in the circuit, the superposed telephonic variation would be effective in the receiving telephone.

I believe it has so far been possible to operate four or five circuits over a telephone line in addition to the ordinary direct circuit carried by the line. Clearly the method is of importance from town to town rather than for local line operations.

In addition there are problems rather financial and administrative than technical. We often see comparisons made, to our own detriment, as to the number of telephones per head of the population in this and other countries, but probably if we had a comparison of the messages per telephone, or even the total number of messages per head of the population, a different story would be told. It is clear that, in a sparsely populated district, communication by telephone attains an importance which it does not reach where all necessities and markets are near the door. But, at least in densely populated areas, an economic factor arises which differentiates the telephone from nearly every other business.

In most businesses the cost of an individual service becomes less as the number of services increases. In the case of a telephone exchange the operating cost of each connection increases as the total number of connections increases, for the simple reason that each individual can make and does make more calls. It is for this reason that comparisons of the annual cost of a telephone are quite fallacious unless the number of calls is referred to, and why any system of flat-rate charging is inequitable.

The problem for telephonists therefore becomes the technical one of keeping all main and junction lines working fully, this being a question of the proper distribution of centres and sub-centres, together with the proper system of charging so that every user pays for what his service costs. In this connection the system whereby the Post Office records the charges and renders an account at intervals, which there is no means of checking or disputing, is most unsatisfactory, and sooner or later some method of rapidly agreeing a call as registerable, so that it can be questioned there and then, must be put in force. The automatic system lends itself to a development of such a method of record, one on which practically no dispute could arise, except as to inaccurate mechanism.

(7) *Wireless signalling or space telegraphy.*—At the founding of the Institution in 1871 the idea had been in many minds to signal without actual telegraph wires, the general idea being the use of current transmitted through the earth or water, between suitably arranged plates some distance apart, and the picking up of a part of the current thus passing by other plates placed along the line of the current. The results were not very encouraging until after 1876 when the invention of the telephone, an instrument far more sensitive than

the minor galvanometer, provided a means of picking up exceedingly small currents.

From 1880 Professor John Trowbridge of Harvard University commenced systematic experiments on wireless methods, by conduction through the earth, by electromagnetic induction (that is by coils or parallel wires), and by static induction ; but all his experiments led him to a negative result, and it is only so far as indicating that some new method was necessary that they were valuable.

The late Sir William Preece was a veritable pioneer ; he tried every practicable method brought to him, and it is not too much to say that the advance of space telegraphy was helped by no single individual more than by him. He repeated many of Professor Trowbridge's experiments on a large scale and with better means, and established a certain number of working examples. But in the years 1887–9 Hertz published the results of the investigations by which he at once crowned by proof the Maxwell electromagnetic theory of light or radiation, and pointed the way to a means of space telegraphy. Numerous physicists and inventors saw the way, and many experiments were carried out during the years 1892–6, and in 1896 Marconi adapting and improving and adding to the various results up to date, and by the discovery of the vertical earthed antenna produced a workable apparatus.

But, above all, Marconi discovered Sir William Preece, and in May 1897 experiments were made between Lovernock and the Flatholme in the Bristol Channel. I remember it well ; I was not present at the experiments, but I found among my late father's papers a letter (I was then living near Lovernock) which ran : " Preece has asked me to dinner ; I am to meet a chap named Marconi, who claims to be able to telegraph without wires."

Sir Oliver Lodge, who had long been working in this direction, was the first to point out and demonstrate the desirability of syntony or tuned sending and receiving apparatus, both for ensuring privacy and for getting the maximium effect from a limited impulse, and also the desirability of long trains of waves set up by oscillating circuits.

From this point the improvements have been largely in detail, and in the increasing application of the principles for which Sir Oliver Lodge contended, namely, tuned circuits and continuous trains of waves. The discovery by Duddell in 1900 of the fact that by connecting the terminals of an electric arc to a condenser and an inductance in series oscillations could be set up, led to the investigation of this method of producing continuous trains of waves, and Poulsen by combining the arc between a hot and cold electrode with an atmosphere of hydrocarbon and a magnetic extinguisher enabled oscillations of wireless frequency of great power to be maintained.

At this point one may stop and note why the wave method has proved possible when the electric current, electrostatic, and electromagnetic induction methods failed. The answer lies in the amount of energy which can be transmitted, and in the limitation of this energy to a path of moderate dimensions. In the case of the electric current the current that would have to be passed between plates, on any reasonable base line, would be enormous, bearing in mind that it would probably be distributed in three dimensions through the whole bulk of the earth of 8 000 miles diameter. In the case of the old electrostatic condenser method the energy available for the receiving end was one single charge of the condenser of which the plates were widely separated. A similar remark substituting currents for electrostatic potentials holds good of the electromagnetic method, but the wave method substitutes the production not only of a single ether impulse, but of an enormous number of such impulses following one another, each in itself of great power because produced through a moderate space, that is between the antenna and the earth, which impulses, moving off, extend only in a vertical direction to a height of about 30 miles, where the researches of Tesla seem to show the existence of a conducting layer. These waves of energy following one another with frequencies of, say, 300 000 per second can all be collected into what are practically single impulses, giving effects therefore enormously larger than those producible by the earlier proposals.

But the greatest discovery was to come. Long ago Edison had observed that an incandescent filament allowed a current to pass between it and an independent electrode in a lamp bulb, the Edison effect it was called, but Dr. J. A. Fleming in 1913 discovered that the current would only pass in one direction ; the apparatus was in fact a rectifier, and also a sensitive detector of small electric disturbances. It was further shown that the current is due to an actual stream of electrons passing from the hot electrode. This Fleming tube acted as a valve or rectifier, but was not very sensitive. The improvement effected by Lee de Forest, however, in adding a grid or spiral placed between the hot cathode and the cold anode, and which could be connected to a separate circuit the variation of whose potential even if small produced great variations in the electron stream, opened the way to the most remarkable series of results that the history of electric science has to offer. By suitably ringing the changes we have in this apparatus a wireless receiver and detector, an exceedingly sensitive relay, a current magnifier or amplifier, and a generator of oscillations. Thus the thermionic valve is finding applications in almost every branch of electricity. Its development came during the war ; we all became gradually accustomed to it. I often think if only it had been introduced complete to one of our meetings, like the telephone, the microphone, or some of the earlier electric discoveries, what a furore would have been created.

With the apparatus thus provided producing continuous wave trains, means of receiving, rectifying and amplifying the received wave trains, a steady flow of energy which could be received as a unidirectional current operating a magnetic telephone or as an alternating current operating an electro-dynamic or two-coil telephone, it only remained to vary the intensity of the wave train by voice modulations in order to accomplish wireless telephony. This has been done. In doing it, the search made about 1878 for loud-speaking telephone transmitters, that is transmitters capable of handling

large currents, has recommenced and with notable success, and forms an interesting example of the greater inventive power which the training and selection of individuals and the accumulation of knowledge has brought about in the last 50 years.

The future of wireless signalling is hardly to be estimated. The immediate future will probably see all other methods of generating oscillations give way to the thermionic tube method. In detail rather than principle does this differ from the arc method where the discharge of electron from the hot carbon fulfils the same purpose as the discharge of electrons from the hot wire of the tube, but the advantage in steadiness of temperature and resistance lies with the tube, enabling the wave-length to be held more constant. Already demonstrations with tubes up to 20 kW have been given.

The recent developments of automatic transmission by high-speed senders with perforated tapes, and reception with telephones and dictaphone recorders to be read off at a lower speed—or by automatic keyboard senders with reception by decoding and printing telegraph (Creed)—hold out singular promise. First, as to the element of secrecy, as only those possessing similar machines can receive and decode the messages, and secondly, by promise of speeds only limited by the capabilities of transmitting and receiving machines.

Now that wireless transmissions have achieved technical success in a high degree, attention must be paid to efficiency. The losses in ohmic resistance and dielectric losses in present antenna constructions are very large compared with the radiated energy. I am told 10 times as great. The same results hold good at the receiving end. Here then is a field either for much greater total radiation, or for the same radiation with much lower input of energy.

III. Problems needing Study and Solution.

(1) *Electricity in agriculture.*—The position of the problem of electricity in agriculture is not to my mind one reflecting great credit on our science.

Sir Oliver Lodge, in the Fifth Kelvin Lecture,* in 1914, gave the result of experiments he and others had carried out, and, for some years past, reports are from time to time circulated of the great increases in crops of various sorts which have been obtained by the use of electrical discharges from wires suspended a few feet from the ground. But the results have been confused and contradictory, and certainly have not reached a position of any degree of scientific certainty. The difficulties and uncertainties of crop experiment on a field scale are hardly realized by those who have not experienced them.

The effect of past history on any particular piece of land is astonishing. I know of a field where every year one strip stands out with a different growth from the rest. Inquiring as to the cause, I found out that 20 years ago the farmer had chalk applied to that strip.

I have seen a field where for several years a small area never had a decent crop, and learned it had been ploughed with snow on it and the snow ploughed in, and the result was a continuing one.

Every cultivator knows the difference in soil, particularly in alluvial soils, that occurs even in the same field, whilst the passage of a harrow or a roller at the right time over part of a field may easily add 20 per cent to the crop of that part. I have a field this year in which was barley and over that clover seed sown, but owing to the rain there was about 10 days' difference in the sowing of the clover on half the field, and the later-sown half has a good plant, the first half very little. In a field of swedes, again owing to the weather, one part was sown a fortnight later than the other, but the later sown is a good crop, the earlier a poor crop.

And so where tests are being made of treated and untreated seeds there may be a difference of time in sowing one or the other; where electrification tests are being made there may be a slight difference in the tillage due to pole and wires, and hence it is only in the case of tests by really reliable experts who understand the effect of small causes that the results are of any value. And it is just here that the tests by reliable experts have so far shown the least effect of the electrical method.

My own view is that careful pot and plant laboratory experiment offers the greatest opportunity of determining the causes of the effects that have been observed.

It will be remembered that during the war a representative committee under the Chairmanship of Sir John Snell was formed, and has since issued an interim report (dated 2nd April, 1919) describing certain preliminary field, small plot, and pot experiments carried out. The results of these experiments were by no means conclusive, as is stated in the report; nevertheless, one or two definite results emerged :—

(1) That, with currents of the order 10×10^{-9} ampere per plant passed from a charged wire through the air to the plant, the results of an electric current are clearly injurious.
(2) That, in the case of young plants at least, the vegetative growth of plants was accelerated by currents of the order $0{\cdot}3 \times 10^{-9}$ ampere.

The committee is still sitting and I believe has reached some rather more encouraging conclusions than indicated by the first report, but the problem is certainly one which the industry should study. One of the difficulties at the outset is that few horticulturists or agriculturists understand electrical phenomena, and few electrical engineers understand the scientific basis of horticulture or agriculture. It may therefore serve a useful purpose to say a few words which will help to make clear the problem to be solved.

The growth of plants consists in the building up of the cells of the plant under the influence of the radiation of sunlight, the raw materials required being, in the main, carbon, nitrogen, potash, lime, phosphorus, and small quantities of silica, soda, and other salts and water.

The carbon is obtained from the carbonic acid of the air, the energy for this endothermic process being supplied by the sunlight. The nitrogen is obtained

* *Journal I.E.E.*, 1914, vol. 52, p. 333.

from nitrates in the soil or from manures containing nitrates or ammonia. The potash is taken from clays in the soil or from manures. The phosphorus can be obtained from manures only. The lime may be taken from the soil or manures, the remaining salts from the soil, and the water from the rain.

The amounts of these elementary substances which are required per acre by average crops are as follows, taking, for example, wheat and potatoes:—

	Carbon, lb.	Nitrogen, lb.	Potash, lb.	Phosphoric Acid, lb.	Lime, lb.	Water, tons
Wheat, 30 bushels	2 005	50	29	21	9·2	1 200
Potatoes, 8 tons	2 069	89	103	32	34·5	1 300

But the production may be raised with unlimited quantities of the necessary plant foods and sunlight to, say,

	Carbon, lb.	Nitrogen, lb.	Potash, lb.	Phosphoric Acid, lb.	Lime, lb.	Water, tons
Wheat, 80 bushels	5 350	134	77	56	24·5	3 120
Potatoes, 20 tons	5 120	223	256	89	87·0	3 380

Apparently at or about these latter figures biological factors come into play, questions of the rate of transfer of the fluids, of the shading from sunshine of the plants one by the other, and so on, and these limit the growth, apart from the question of the supply of essential foods. So it is between these limits that we have to work.

If plants were grown in sterile sand and all the foods had to be provided by artificial manures, the cost to-day of the quantities in the first table per acre would be as follows:—

Wheat.

	lb.		lb.		£	s.	d.
Nitrogen	50	from	286	Nitrate of soda, £24 5s. per ton	3	2	0
Potash	29	,,	210	Kainit, £8 per ton	0	15	0
Phosphoric acid	21	,,	78	Superphosphate of lime, £9 2s. 6d. per ton	0	6	3
Lime	9·2	,,	10	Ground lime, £2 12s. per ton	0	0	3
					£4	3	6

Potatoes.

	lb.		lb.		£	s.	d.
Nitrogen	89	from	500	Nitrate of soda	5	12	0
Potash	103	,,	750	Kainit	2	13	0
Phosphoric acid	32	,,	119	Superphosphate of lime	0	9	6
Lime	34·5	,,	35	Ground lime	0	0	8
					£8	15	2

The majority of soils, however, contain the bulk of the substances necessary, and thus by living on our capital we may replace or find part only of the substances required: but by this process we gradually exhaust the soil, since on the British system of sewage disposal the final products of agriculture consumed by human beings are consigned to the sea.

I have referred to the supply of the necessary plant foods by manures, that is, either by returning part of the substance taken from the soil, these being fed to animals who pass through their systems much more of these substances than they retain, the balance or a large part remaining in the farm-yard manure, or by the use of mineral manures; but one important section of plant foods, the nitrates, is also produced in the soil by groups of bacteria, which, some working in conjunction with the roots of certain plants on which they feed, and others feeding on organic matter in the soil, are able to absorb and fix the nitrogen of the air.

Agriculture therefore adopts rotations or successions of crops, in the course of which are utilized leguminous plant crops with which the allied bacteria fix nitrogen, or alternatively soil fallows or rest periods are used, during which bacteria fix nitrogen which is accumulated for a succeeding crop. A crop of clover will thus fix and add to the soil about 30 lb. of nitrogen per acre, besides supplying itself with the necessary nitrogen.

In a rotation of four crops such as is commonly used in a large part of England, it is necessary to supply in addition about 160 lb. of nitrogen per acre during the four years if all the crops are sold off the farm, and about 75 lb. if the usual part is fed to animals, of which 50 per cent may reach the soil again in the manure.

At low levels of production no nitrogen other than that provided by the bacterial action is given.

The bearing of all this on electro-culture is as follows: Whilst there is nearly always an air of mystery about the discussions of electro-culture, the questions to be answered are simple. Does any system of electrification promise to increase the quantities of the availability of any of the primary food necessities of the plant, or to augment the rate of or total absorption of the plant foods, and if so at what cost?

We may at once say that the presence of a highly charged conductor with a corona discharge therefrom cannot augment the quantities of any of the food materials necessary in the soil except the nitrogen.

It is conceivable, but improbable, that it will increase the rate of solution of the natural soil elements.

What are the possibilities ?

(1) Direct stimulation of growth processes, replacing, in fact, sunlight.
(2) Increase of transpiration of water.
(3) Decrease of evaporation from the soil, equivalent to providing more moisture.
(4) Production of nitrates.
(5) Ionization of nitrogen or carbonic acid so as to be more readily absorbed by the soil bacteria or the plant.
(6) Sterilization of the soil.

Of these (1), (2), (3) are purely electrical if they exist ; they are direct actions and would occur in the region immediately under the wires.

Many experiments have shown, however, that the effect, whatever it is, does not occur necessarily directly in the near neighbourhood of the wires used to maintain the electric stress, or to give the electric discharge, but that the effect may be to one side or the other of the electrified plot, and it appears to me that this result can only be interpreted as indicating that the result is not strictly electrical at all, but due to something produced in the neighbourhood of the wire, which is heavier than air, but can be diverted by the prevailing wind.

If this is the case we are thrown back on to the causes (4), (5), (6).

The researches of Earle Warner and Jakob Kunz at the University of Illinois have shown that the corona discharge in air causes ionization of nitrogen, oxygen and carbonic acid ; the researches of Professor The Hon. J. Strutt and Dr. Lowry upon the action of the silent discharge have shown that active forms of nitrogen are the result. The discharge in carbonic acid produces oxygen ; in air, with water, ammonium nitrate. The fact then that chemical active ions and compounds are produced which would move with the wind, points to these being the cause of the observed results. If the results were due to the production of nitric acid one would expect the increase to be greater the greater the current, but, on the contrary, the results were better with smaller currents. A similar agreement would seem to hold in the case of the ionization of carbonic acid.

The results obtainable in increased crops by the partial sterilization of the soil are very remarkable. The soil is normally inhabited by bacteria which, living on the organic matter of the soil, assimilate atmospheric nitrogen and, in dying, leave behind a nitrogen compound, in other words enrich the soil with nitrogen. These bacteria are, however, held in check by other forms of soil life, and a partial sterilization which will destroy these other forms gives enhanced opportunity to the nitrogen-fixing bacteria. This partial sterilization is carried out by steam-heating soil where it is to be used in limited quantities, or by chemical agents such as formaldehyde, dilute carbolic acid, etc., on a larger scale.

Recent researches have shown that one of the most powerful sterilizing agents known is oxide of nitrogen ; one part in 50 000 parts of air is sufficient to kill any germ with which it comes in contact. It seems to me, therefore, that we have here the possible solution of the effect of electrical discharges over plants.

A crucial experiment would be to compare the action on plants growing in soil, and in water with the plant foods dissolved therein. If the water-culture plants were unaffected and the soil-grown plants benefited, then I think the case for soil sterilization would be made out ; if the reverse it would point to nitrate being the cause.

It would appear as if the different effect of strong discharges and weak discharges may be in this, that strong discharges produce products, other than the oxides of nitrogen which have a sterilizing effect, perhaps nitric acid, whilst weak discharges produce the sterilizing effect described.

The Wohler process of so-called electrification of seeds has also been boomed by the papers. The process consists in placing the seeds in a solution of $2\frac{1}{2}$ to 5 per cent household salt and passing an electric current through the solution, then drying the seeds and sowing them. Even assuming the solution to be effective, it is difficult to see what result would accrue from the electric current, unless it was a possible deeper or more rapid penetration by electro-osmosis. Careful experiments however by Dr. Hall at Rothampstead led him to conclude as follows :—" The writer is not prepared on present evidence to say that the process never succeeds, but the risk of failure seems so great that the farmer should look upon it as an adventure which may or may not prove profitable."

Mr. Martin Sutton, of Reading, after exhaustive trials arrived at what was practically a negative result. It is not necessary to question the veracity of the observers, the causes of variation in crops which I have mentioned may have contributed, but in addition it has been shown that kiln drying of cereal seeds contributes to a higher percentage and more rapid germination. In this part of the process the benefit in certain cases may lie, as has already been suggested by Dr. Hall.

(2) *Electric vision.*—One of the problems which awaits solution by electrical engineers is that of electric vision or, in newspaper language, " seeing by electricity." The broad principles are understood and have been for many years. I believe they would yield to an organized research but, perhaps because there does not seem to be a wide market, no one is particularly interested.

The transmission of photographs by electric current, which from its usefulness to newspapers has received attention, may be referred to. The principle involved is in all cases the splitting up of the image into a number of lines, and each line and therefore the whole image into a number of dots. These dots are to be translated into a series of current impulses which are to be recorded as equivalent dots at the receiving end. This involves synchronizing the movement of the sending picture and the receiving blank sheet.

The method of Professor Korn is to allow light from each dot forming the picture to be projected by a lens and fall on a selenium cell sensitive to light, so that

approximately the current varies proportionately to the illumination of the projected part of the image. At the receiving end, by a sensitive galvanometer with mirror or other light-varying attachment, the intensity of a point of light is varied. This point of light falls on a sensitive photographic plate, affecting each portion of the picture in turn and reproducing when developed the intensity of the sending portion. By synchronizing the positions of the dot photo images with those of the sending dot images, the picture is completed.

By such apparatus photo copies have been transmitted 700 miles. With other apparatus on similar principles Thorne Baker has sent pictures from Manchester to London.

To make the picture immediately and constantly visible, however, requires something more. Either the whole operation must take place and be repeated sufficiently often for the image to impress the retina continuously, that is to say, 16 times a second, or the image must fall on a phosphorescent luminous surface which will store the image for a longer period but not so long a period that will prevent movement being appreciated.

The number of impulses required for the first of these methods is very great. If we take an image made up of 50 dots to an inch, then one square inch of image would require 50^2 or 2 500 impulses, and at 30 repetitions per second this would be 40 000 impulses per second, which may be compared with 800 per second usually considered to represent telephonic frequencies. It appears clear that wire transmission can hardly be considered; wireless, however, remains. Some work in wireless transmission by the photographic method has already been done by Thorne Baker and M. J. Martin.

The use of selenium for the purpose of a transmitter, even with the compensations effected by Professor Korn, seems out of the question, the charge of resistance and the recovery is too slow, though it has been used for other purposes at telephonic frequencies. So research requires to elucidate two separate questions:

(1) Some action of light which acts instantaneously and enables a current variation to be set up. (2) Some massless method of illuminating or darkening a surface, which can be varied by an electric current.

For the first (transmitter) it suggests itself that the photo-electric effect might be used. Kemp records a measurable current obtained with a photo-electric cell illuminated with the equivalent of a candle at $2\frac{3}{4}$ miles distance, showing great sensitiveness. For the second (receiver) a vacuum tube with a beam of cathode rays diverted by the current from or on to a phosphorescent point after the manner of the Braun cathode tube oscillograph suggests itself. If the method of producing an image on a phosphorescing surface were adopted, it is probable that one cycle of operations per second over the whole surface might suffice. There is a great range of substances phosphorescing after exposure to light from small fractions of a second up to hours, so that this part is purely experimental research.

The intermediate mechanism of wireless transmission I need not enter upon here. When I was about 19 years of age I commenced crude experiments on this subject, using interference methods in monochromatic light to obtain the light and dark points, with a very small movement of what was equivalent to a telephone plate, but I have never had time personally to pursue it further. I am convinced, however, that all the elements for the purpose are now at hand, needing a definite research to complete the task.*

(3) *The changing basis of productive industry.*—At the founding of our Institution there was not, strictly speaking, an industrial problem. The manufacturing era which commenced with the perfection of the steam engine by Watt in 1774, and the factory system which followed, were looked upon as the beginning and end, as a system of industrial production. It may be that it will be thought that the devotion of any attention to such a subject is anyhow outside the range of an Institution like ours. But we cannot get away from this question. Our retiring President gave us in his address an exhaustive examination of the question of electric traction, but his year of office was mostly filled with attendances at Industrial Courts and trade union negotiations. We pride ourselves on the engineering efficiency of our great generating stations and on the safeguards to avoid breakdown and stoppage, yet the greatest risk of stoppage is the unlubricated working of the human element. Of what avail is it to continue to perfect the machinery of material civilization if, in the end, that civilization is wrecked because of the dissatisfaction of the people for whose satisfaction the perfecting is assumed to proceed. We must think about these things, we must help to form opinion thereon, and we must help one another to conclusions by discussion. What is it that really lies at the basis of social and industrial unrest? You will be told it is irritation at unequal and, presumably, unjust distribution of the products of industrial work, or the unequal distribution of leisure, amusement, house-room and food. I am of opinion that in the main it is that people do not like their work; they are always trying to escape it, to forget it.

It may be summed up in the monotony of machine production or machine operation, at high pressure, with enough education partially to awaken the reason, but an insufficient education to appreciate the beauty and the triumph of what is happening. An engineer can watch a cotton spinning machine; to him it is more than a mass of wheels to be fed at intervals and relieved of its burden of thread as it is produced; his mind sees in the machine the adaptation of means to an end, the triumph of producing continuously a product so even that no change from point to point can be determined. He may think how the product can be increased or improved. Not so the cotton mill operative, to him or her it is only so many hours of noise and moist air, something to be endured and then forgotten.

Why is there among young and old this ceaseless hunt for something to occupy the mind and attention? The situation has been aggravated by the idle and irregular life of the Army, through which so large a

* Since this Address was delivered Mr. A. Campbell Swinton has called my attention to an Address delivered by him in 1911 to the Röntgen Society dealing with the subject of Electric Vision and putting forward almost identical suggestions to those outlined above, except as regards wireless transmission.

proportion of the male population has recently passed. Why do book-makers, organizers of vast football and boxing entertainments, theatres, cinemas, motor charabancs, dance halls, and many other forms of entertainment, find always an ever-growing and insatiable multitude of supporters? Because, if I am right, they provide something to think about, something to look for besides the mechanical routine of work. It seems to me sometimes as if we are operating a cycle of work, not unlike the cycle of some engines, where a large power production on a working stroke is counterbalanced by a large power absorption on a compressor stroke, with very little net result.

If I am right, that lack of interest in work under modern methods is at the base of industrial unrest, then efforts to allay and overcome unrest must contain some element which will overcome this lack of interest. Whatever may be the case in unskilled work, in semi-skilled work, in machine work, and in skilled work, I believe it is possible to stimulate interest in the work by explanatory teaching, and by discussions with and among the workpeople. I believe that sooner or later it will be wise to place part of the direct responsibility of management and production in the hands of the operative workers, and that we have to find a basis where the capital required is paid for at a fixed rate, high where risks are high, low where risks are low, and that the whole product beyond this is divided between the people who are managing and directing, and the people who are operative producers.

In industry on a large scale under modern conditions, the initiating and directing classes are not the actual shareholders or capitalists, but are the managing directors, managers, and technical staffs, and no class suffers more from the continuing harass and fret of industrial strife than these classes. The idea that enterprise would stop under such conditions is largely fallacious. If enterprise means the running of great risks in the hope of great rewards, then the company promoter who runs little risk of his own, but induces others to run great risk by hope of great reward, is our greatest asset—but this is not so; in the majority of cases where persons put money into new and risky enterprise they lose it, and no one is the better. The German industrial system was largely founded on the system of individuals supplying capital at a fixed rate to industrial banking corporations, who after examining most carefully, under trained advisers, the prospect of a new enterprise, embarked on it, if they did, with a real assurance of success, and, receiving a higher rate of interest than the individual capitalist, obtained thereby a cover for their risk and a payment for their skill and organization.

To work on such lines utilizes individual skill and enthusiasm; it removes the objection on the part of the workers that surplus production benefits someone other than the producer. Coupled with a share, and an understanding of the objects of management, it stimulates and interests, and, in my view, if some such ideas do not fructify, the pressure for nationalization, or business controlled by politics, will, I fear, tend to its adoption, with all the great evils and ruinous financial results that will follow. Every experience shows that public ownership of an industry giving a lesser incentive to the individual than private ownership produces a lesser product at a greater cost; but all the arguments for private ownership centring on interest in a given work and on self interest apply to every person occupied in an industry, and it should be our effort to utilize those efforts to the fullest.

(4) *Application of scientific methods in commerce and industry.*—It is a matter of common knowledge that British industry has been for the most part initiated and controlled by men who have not as a rule had a scientific or engineering training and whose outlook is, broadly speaking, unscientific. Such men claim to be practical, and it is a matter even of pride to them that their achievements are reached by intuition or, as they would say, common sense.

Discarding intuition, in matters which are subject to causes and effects, as a dream, we may analyse common sense to mean either reasoning from simple knowledge to form a judgment on more complicated matters, or so rapid an adjustment of forces to circumstances as they arise, as to have the appearance of working with a full knowledge of the relation of effects and causes. But whilst with the British temperament, strongly individualist, adventurous, self-confident, pertinacious, this basis for commerce and industry has in the past been successful, the last 30 years have seen its gradual eclipse by other methods and, in so far as British industry and commerce have not followed them, so they have lost ground. The broad characteristics which go to make a successful commercial and industrial leader are in most cases incompatible with the mental attitude of a scientific worker, and vice versa; but if some scientific training were universal in our education system, there would be an understanding by the industrial leaders of the power of the scientific method, which would lead them to surround themselves and maintain contact with men of specialized training on all subjects, and to consult with them and rely on their guidance.

In speaking of scientific methods I am not especially or only referring to physical sciences; I include the science of economics, including statistics, the science of hygiene, of ethnology, of physiology and psychology. Science is organized knowledge. Why should every industrial or commercial operation, unless it be a repetition of an old operation (in a rut!), be a new experiment going over ground which has probably been already covered, or for which the data already exist? What would be thought of an engineer who went into the boiler trade, and began by empirical methods to make a boiler and gradually, by a series of explosion experiments, to ascertain the properties of water and steam and the relation of temperature and pressures and strengths of material? Yet, ridiculous as it appears, that is the situation in many industrial and commercial operations.

It is true that the total of organized knowledge to-day is so large that it is impossible for small undertakings to command it. Particularly is this the case when, owing to lack of trade organization in Associations, most of the time of a business owner is taken up in formulating attacks on his competitors or repelling

their attack. The law of *diminishing returns*, which applies to agriculture and the production of raw materials, does not apply to manufacturing or commerce, being replaced by the law of *increasing returns*, that is, the firms with the largest turnover can always undersell the others. Hence there is a natural instability in manufacturing and commerce between rival firms, an instability which, whilst it benefits or ruins individuals, does no good to the consumer, or the total of manufacturers or commercial men.

The solution for us lies in Associations representing the separate branches of the industry. These, by various methods of damping the oscillations which arise in all unstable conditions, enable a total industry to devote itself to the overcoming of external competition, to the cultivation of overseas trade, to the development of improved processes, all these with the possible benefit of increased application of scientific knowledge. This may be provided under co-operative principles for all concerned in an industry, though the progress in this last direction is slow.

Nevertheless I am pleased to say that the British Electrical and Allied Industries Research Association, founded by the B.E.A.M.A. and with the help and support of the Institution, is at last successfully launched and has taken over the research work initiated by the Institution. This at once puts into action a sum of £16 000 per annum for joint electrical research, a sum small enough for a great industry, but it is hoped that as the first results appear, the usefulness of the effort will be appreciated and that groups in the industry will bring forward their problems and achieve a common knowledge and advancement.

Whilst for races like the German race, which is naturally adapted to the regimental or team spirit of working, the integration or combination of commerce and industry under a limited leadership is suitable, and permits of the highest adaptation of scientific organization in industry, the British temperament leads to a better result in a greater number of leaders; but to overcome the otherwise inevitable unstable economic result the Association must intervene. Twenty years or more ago, when the Cable Makers' Association was founded, its founders saw these effects in a dim way and without conscious analysis. But since then the Association movement has spread, and I venture to think that it saved British industry from eclipse.

Curiously enough, however, though this movement started in the electrical industry, that industry was the last to benefit from it, except in one department. I cannot here refrain from referring to what has been done by Mr. Dunlop, one of our Members of Council, and some associated with him, in the face of the greatest apathy and discouragement, successfully to bring about this result for the electrical industry. In no direction is Association more needed than in the publicity of our industry towards its customer, the public. In the past it has been left to each manufacturer to push his wares individually to the consumer of his product. The engine and dynamo builder has pushed his goods with the consulting engineer and the company or municipal undertaking; the electric motor manufacturer has educated individual trades and factory owners to adopt the electric drive; the contractor has sought individual installations by personally canvassing and convincing customers of the usefulness of electric light and so on; always we have worked with a plenum or pressure "pushing" our wares.

Now we begin to see widely what some have long seen, that it may be cheaper to create a vacuum at the far end, into which the goods will follow. In other words, educate the general public, enlighten the immense possible market, in fact work to create a universal demand for electric current; the call for goods will follow automatically. This is the function of the British Electrical Development Association, in the founding of which the Institution has lent most useful countenance and assistance.

I contend that these cases of Associated effort are examples of more scientific methods of conducting business which, whilst utilizing individual effort in every direction, nevertheless by organized method use the resources of accumulated knowledge to take the place of wasteful unorganized effort.

IV. The Industrial Outlook in the Electrical Industry.

Speaking of the manufacturing side of the electrical industry, we may look back to the situation in 1871. There were a few firms established at that date whose business was the manufacture of telegraph apparatus, and cable-manufacturing firms who were producing telegraph and deep-sea cables, but beyond this there was no electrical manufacturing industry. The year 1878 saw the commencement of the manufacture of dynamos by a few firms, and by 1885 there were established almost a dozen firms making dynamos up to 200 kW.

Since then there has been the great increase in the number of firms entirely occupied with electrical manufacturing, and in the number of firms who are partly engaged on the production of electrical machinery and products.

Unfortunately, it must be admitted that the manufacturing side has not prospered as it should have done. The localized condition as regards electricity supply brought about by the legislative policy previously referred to, resulted in each generating station embodying the special ideas of a designing engineer, with little regard to standardized plant, and this led further to a close subdivision of tendering, thus discouraging the placing of contracts for the whole supply undertaking in the hands of a single firm, this being the opposite of the procedure on which the electrical industries of America and Germany have grown. This reflected itself on the foreign market, where the conditions necessitated the taking of whole contracts, and, although several attempts in the past have been made to form groups of firms capable of supplying complete installations on a large scale to operate in foreign markets, they have not been found very successful, the internal co-ordination being insufficient. The result of this absence of foreign markets and the operation of a large number of comparatively small firms in free competition, led to a senseless competition in

price for the home market, which kept the industry in a state of poverty where it could exist and no more.

Only in one branch of the industry did sounder methods prevail, and here, curiously enough, because the absence of standardization and competition was so dangerous to the entire industry that it was earlier perceived. Whereas in the case of electrical machinery the visible nature of the product and the ease of conducting tests maintained some standard of quality to which the prices had necessarily to correspond, in the case of cables it was always possible to maintain some margin of profit at almost any level of price by a sacrifice of quality, which in the absence of definite life-tests was not obvious to the buyer until too late.

So clearly was the danger of such a process realized, that by 1889 the British cable makers had been forced to a consideration of the situation, and to arrive at agreement as to standards of products and, for the most ordinary forms of small cables purchased by non-technical buyers, a standardization of minimum prices which at least allowed these classes of cables to be produced on an economic basis.

Furthermore, the necessity of close supervision and responsibility in the matter of laying and installing cables, led to the system of single contracts and the building up of competent staffs which could be drawn on to form the skeleton units for cable-installing all over the world. Thus, alongside of a machinery and accessories manufacture in a deplorable commercial and financial condition, we have seen a cable manufacture dominating the world market and, speaking generally, in a satisfactory financial position.

Apart from social, political, and labour unrest the manufacturing situation now looks more favourable. There has been a movement of integration, joining up under single control of sufficient firms to enable complete contracts to be undertaken in any part of the world. The close of the era of constructing small localized generating plants will permit and necessitate standardization, and the increasing size of units will limit the number of new-comers into the industry who, with experience lacking, and having the necessity of forcing their way into the market, were a constant depressing influence.

The experience of the war production of standard designs, and the increased amount of automatic and semi-automatic machinery as a result of the war, will tend to specialization on the part of those making small machines and accessory apparatus, thus to a certain extent eliminating competition, but, as the result of the increased production of a limited range of standard articles, diminishing the cost below what the previous competitive cost would have been. An example of this effect is seen in the past history of the clock and watch trade. Clocks and watches were never so cheap as they became when, by standardized machine production, practically all competition was eliminated. With the proviso that political and industrial stability is maintained, and that the supply industry is left free to adapt itself to the new code of area unification, the general outlook appears to indicate a time of great activity for electrical manufacturers, with a more satisfactory financial result than in the past.

Conclusion.

In what has been said, I have endeavoured to give a picture of the technical achievements within the sphere of the electrical industry, which 50 years of effort has produced, and I take this opportunity of saying how grateful I am to many of my friends for information and help they have given me in this direction. In doing so, I have necessarily given my own individual point of view; only to a limited extent have I been able to mention by name that army of workers to whose ceaseless efforts these results are due, because for the picture it is the result, not the individual, that must be seen.

I have ventured to mention a few questions which, again, strike me personally as worthy to receive our further efforts. Sometimes I think that the age of our great scientific workers, the age when a few great men stood out from all the rest, has gone, and one asks oneself, why? But I think the best cure for such thoughts is to take up such evidence as is given by the last issue of our *Journal*, to look at the quantity, and the wonderful quality of the papers put forward, and to compare these with the papers in our *Journal* when the giants were about. It is the number of great workers to-day that leads one to suppose there are none.

There is one matter in connection with technical papers to which I should like to refer here. The vast amount of technical literature which appears in the "Proceedings" of learned societies and the technical journals of the day makes it impossible for any one person to read everything that is published even in his own particular branch of work. I therefore take this opportunity of appealing to all those concerned with the publication of technical papers to preface every paper so published with a brief note indicating the contents of the paper, the ground covered, and the main conclusions arrived at or results obtained. A note of this kind would enable the reader to decide at once whether or not he would be justified in giving the time to read the paper and assimilate its contents. Such notes would also be of great assistance to abstractors of papers in preparing the material for *Science Abstracts*, the valuable publication of the Institution in co-operation with the Physical Society of London and the American Physical Society.

This Institution contains within its membership men and minds capable of dealing with any scientific or industrial problem; it is the duty and privilege of the Institution to be the centre for their work. I urge on the membership to use it more and more for such a purpose. The Institution is led and guided by a Council, in touch with the Committees of the Territorial Centres, and in the past the Council has been freely criticized and has no doubt benefited by the criticism, but, let me say, criticism is good, but constructive suggestion is better. It may be that the methods in our present constitution are not the best for obtaining on the Council the true representation of the wishes of the members, but no constructive alternative has been put forward, and I am sure that,

if it is, it will receive most careful and sympathetic consideration. It should be the conviction of every member that he has a voice in the work of our Institution, that the Institution exists to fill his needs, and that where the Institution cannot speak with one voice because of conflicting interests, it should use its power and its influence to obtain the best possible hearing for those interests, individually, to expound their own views.

The Institution exists for its members, but its members owe it something. Three hundred years ago Bacon wrote:—

> "I hold every man a debtor to his profession, from the which as men of course do seek to receive countenance and profit, so ought they of duty to endeavour themselves by way of amends to be a help and ornament thereto."

I commend those words to you all.

SOUTH MIDLAND CENTRE: CHAIRMAN'S ADDRESS

By F. Forrest, Member.

(*Address delivered at* Birmingham, 3 *November*, 1920.)

I appreciate very highly the honour you have done me by electing me your Chairman for the ensuing year. I earnestly trust that during my term of office the post of Chairman of this important Centre will lose nothing either in influence or dignity.

The essential difference between a paper read before this Institution and a Chairman's address is that the former should be a thesis on one particular subject, whereas in the case of an address a certain discursiveness is considered desirable, and one is permitted to touch on a variety of matters of general interest to any gathering of electrical engineers. In my short address this evening I propose to refer briefly to several such matters, and to deal in greater detail with one or two which may be of special interest to those engaged in the electric supply industry.

From a purely engineering point of view the Electricity (Supply) Act 1919 wisely administered is likely to prove an important means towards national industrial progress, and is certain to bring about a great improvement in the efficiency of electrical power supply. As members are aware, under this Act the country will be divided up into a small number of electricity districts, in each of which will be erected, as and when required, generating stations which will ultimately supply the whole of the electricity required in that district. These large generating stations will be situated in positions chosen with due regard to the best facilities for obtaining abundant cheap coal, ample supplies of water for the condensers, and the shortest distance to the centre of gravity of the loads to be supplied. This concentration of the generating plant required for each district into one or two stations will mean that the largest and most economical units obtainable will be installed and, as the load factor of the system will be high, due to the diversity of the load over the large area, the resultant economy will go far towards counteracting the effect of the recent heavy increases in the price of fuel and labour upon the selling price of electricity.

Provision is also made under this Act for the utilization, where found possible or expedient, of waste heat from other manufacturing processes and of water power, so as to conserve as far as possible our rapidly diminishing reserves of coal. As an indication of what may be found possible in this direction it is interesting to refer to a paper * read before the Institution recently by Mr. S. H. Fowles, in which it was stated that the amount of blast-furnace gas which is at present being either entirely wasted or used in a very wasteful manner in this country has a heat value equivalent to 750 000 kW. Furthermore, the Water Power Resources Committee of the Board of Trade in a recent Interim Report on the water power available in Scotland showed that, although only nine sources of power had been investigated up to the present, these were calculated to yield a total of 183 000 kW, whilst a good case can be made out for the immediate development of a further 300 000 kW.

The Act is also unique in its way, in so far as it definitely authorizes the Electricity Commissioners, either by themselves or by some other competent body, to conduct experiments for the improvement of the methods of electric supply or of the utilization of fuel or water power, subject only to the approval of the Board of Trade to the necessary expenditure being incurred. This clause of the Act is one which must give peculiar satisfaction to members of this Institution who have long been aware how very far we are behind America and Germany in the matter of experimental and research work. Fortunately, this country has never lacked great original thinkers and men with an aptitude for research inherent in their nature who, with the increased facilities to be provided by this Act, will add greatly to our present stock of knowledge and help to throw light upon some of our most difficult problems. Our Institution, in particular, numbers among its members many such men who are eminently fitted for this work by their specialized education and training. In saying this I do not use the term " education " in any restricted sense as applying to a course of study at school or college, but by it I mean all those experiences, circumstances and studies which go to mould a man's character and increase his knowledge, and so fit him to do those things best of which he is most capable.

We see, therefore, that the new Act, by delimiting the proposed electricity districts with a single view to obtaining the best all-round efficiency, by providing for the utilization of waste heat and water power where found commercially expedient, so as to conserve coal, and by providing for research work so vitally necessary in the case of an industry based upon an ever-advancing science, is a landmark in our industrial history, and the beginning of a new and better era for electrical engineering.

Although, owing to the present difficult financial situation, the full development of the national scheme as outlined in the Act may be considerably retarded, it will not be inappropriate just now to consider some of the engineering questions to which the Act will direct special attention.

The type of prime mover chosen for use in the generating stations will be that which under normal working conditions gives a maximum of economy,

* *Journal I.E.E.*, 1920 vol. 58, p. 431.

reliability and convenience for a minimum investment of capital. As far as our present knowledge goes, the high-pressure steam turbine fulfils these conditions best and is therefore likely to be adopted. The size of each individual unit is not likely to exceed 40 000 kW for single generators, owing partly to difficulties of transport, but partly to the fact that the increased economy with larger sets will be very small, and this is more than offset by the additional risks incurred due to a smaller number of units being installed. A safe rule to adopt would be that the size of the largest generator installed should not exceed in capacity 20 per cent of the maximum load on the system it supplies, and spare plant equal in capacity to the largest set installed must be provided and kept available for immediate service. The first and chief consideration must be continuity of supply and, unless some such limit is imposed on the size of the unit, too great a striving after the lowest possible costs may lead to grave embarrassment in case of breakdown. The trend of modern engineering in its search after economy is towards still higher steam pressures and increased superheat. Some of our largest new stations are to run with steam pressures as high as 350 lb. per square inch, with a total temperature of about 700° F., whilst at one large station now approaching completion it is proposed to re-heat the steam between the high-pressure and low-pressure cylinders of the turbines. The present is, as it were, a transition period in the design of the modern power house, and the experience gained with these high pressures and with re-heating will go far towards deciding the type and arrangement of the main turbo-generators for future stations.

Although the gas-firing of the boilers is at first sight an exceedingly attractive proposition, a sufficiently good case cannot yet be made out for it, and our present method of burning low-grade fuels direct under the boilers is likely to be continued. It cannot be too clearly stated that every process of coal distillation for boiler firing involves considerable heat losses and requires us to consume larger quantities of coal than if the coal were burnt directly under the boilers. The extra heat lost must be compensated for by the production of by-products or substances of greater value than the heat so lost. Unfortunately, this is not the case, even with the present high price obtainable for these by-products, so that gas-firing with its promise of higher boiler-house efficiency is not yet a financially sound proposal.

The efficient burning of low-grade fuels has been made possible by improvements in the design and arrangement of the mechanical stokers and furnace brickwork, and also by the gradual introduction into our boiler houses of men of superior intelligence supervised by expert steam engineers. These men are now greatly assisted in the carrying out of their work by accurate instruments for indicating the amount of steam delivered by each boiler, gas temperature indicators, draught gauges, and CO_2 indicators. We are hopeful that shortly it will be possible to replace the CO_2 indicator with an instrument for indicating accurately the excess of oxygen in the flue gas, which is what we really want to know. The temperature of the gases discharged from a modern water-tube boiler with its economizer is so low that mechanical draught is a necessity, but this, however, gives us a better means of controlling the duty of each boiler during periods when they may be supplied with fuels of widely varying qualities.

Further elasticity in the working of the boiler house might be obtained by sub-dividing the overhead coal bunkers into two unequal parts, the larger part holding fuel of average quality for use during periods of light or moderate loads, and the smaller part holding fuel of better quality for use during periods of heavy load or in case of emergency.

Even with the most economical plant the overall thermodynamic efficiency of the big generating stations we are considering is not likely to exceed 20 per cent, when running with a load factor of 40 per cent. This is a great advance on what is being accomplished to-day in our smaller and older stations, and it will be found very difficult in everyday practice greatly to improve this efficiency whilst steam is used as our working substance. Our friends in the gas industry have often quoted to our disadvantage the superior efficiency of gas production as compared with the generation of electricity and, if I may be permitted to digress a little from the main theme of my address, it is as well to point out that in the average public supply gas-works the gas in the holder only contains 25 per cent of the heat energy of the coal from which it is obtained. Whilst electricity can be utilized for power production at a constant efficiency of about 90 per cent, gas can only be utilized for the same purpose at an efficiency of, say, 25 per cent when the gas engine is new, and at very much less than this when it is old.

In comparing the relative thermal efficiencies of gas and electricity for the production of power, we see that in the case of electricity the principal loss is incurred in the generating station and is borne by the supply authority, whereas in the case of gas the principal loss is incurred in the gas engine and is borne by the consumer. Perhaps it is partly on this account that (according to Sir Dugald Clerk) the whole of the public gas undertakings in this country only supply about 448 000 b.h.p. of gas engines, which is of course a mere fraction of the total horse-power of motors connected to the mains of our public electric supply undertakings.

The outstanding features of the design and arrangement of the controlling switchgear which will be installed in the large stations of the future will, I venture to predict, be safety and absolute simplicity. Among other means likely to be adopted for increasing the safe working of the main switchgear, will be the use of current-limiting reactances of such capacity and so arranged that every main switch in the station will be capable of satisfactorily breaking the maximum short-circuit current of the circuit which it controls. The prejudice against the use of reactances for the main switchgear which has undoubtedly existed hitherto, is giving place to a better appreciation of the benefits accruing from its use, and it is now considered to be as sound engineering to insert current-limiting reactances for the prevention of destructive short-circuit currents as it is to mount a 6-inch gun on a non-rigid carriage.

Both safety and simplicity are increased by keeping the number of circuits connected to the main busbars down to the absolute minimum. For instance, if there are six generators installed in the station, then the number of circuits connected to the main busbars should not generally exceed 18, viz. 6 generator circuits and 12 group feeder circuits, each of the latter supplying through current-limiting reactances a sub-busbar to which a group of feeders would be connected.

With the increased size of the new electricity districts, feeder and transmission-line voltages are likely to be much higher than the standard pressure of 33 000 volts adopted by Manchester and Birmingham for their new stations. For pressures over this amount the three-phase trunk feeders will probably each consist of three single-core lead-sheathed cables, the copper conductors being stranded over a hemp core to increase their diameter with a view to reducing the potential gradient. The high dielectric loss when pressures exceeding 30 000 volts are used, and the induced voltage in the lead sheathing if single-phase cables are adopted, make the whole subject of cable design for these very high voltages a highly complex problem, but we may be sure that our British cable manufacturers, who have been experimenting for years with extra-high-voltage cables, will be ready to supply such cables when required, and will guarantee their satisfactory working. I am assuming, as I hope will be the case, that all main transmission feeders will be placed underground (full advantage being taken of the compulsory wayleaves provided by the Act) and only a few transmission lines of secondary importance placed overhead.

Although probably not more than 10 per cent of the output of the big stations we have been considering will be used for purely domestic supplies, I want to refer particularly to a means for giving this class of supply from substations connected to the main E.H.T. distributing system.

It will generally be conceded that for small low-tension domestic supplies direct current is on the whole more satisfactory than alternating current, apart altogether from the larger question of power factor and the fact that for equal declared pressures alternating current is at least 41 per cent more dangerous to touch or handle than direct current. The latter point is of special importance now that so much portable apparatus is being introduced into the modern household. Low-tension alternating current can, however, at present be supplied to small consumers more cheaply and economically than direct current, because, with the former, step-down static transformers having an efficiency of about 98 per cent are employed, and these require no regular attention, whereas, with direct current, rotary converters must be used having an efficiency of about 92 per cent, and these do require constant skilled attendance. A number of entirely automatic rotary-converter substations have been working with considerable success for some years in the United States, where labour costs are even higher than ours, but I cannot believe that this method is likely to meet with much favour in this country. The ideal towards which we are working, and which a few years ago seemed almost an impossibility, is a completely automatic static substation for converting E.H.T. three-phase alternating current into low-tension direct current, with an overall efficiency approaching that of the modern transformer.

The remarkable development during the last few years of the mercury-vapour rectifier for substation work in sizes up to 500 kW each, has brought us much nearer to our ideal, and I believe it is apparatus of this character which will be adopted for small traction and lighting substations in the future. The working principle of the rectifier and a complete description of the design and construction of rectifiers of large output as built by Messrs. Brown Boveri have already been published in the technical Press, and as an indication of their successful operation I am able to tell you that rectifiers having an aggregate capacity of 55 000 kW have already been supplied or are under construction in Switzerland. I hope a 250-kW set will be working this winter in Birmingham. I have seen these large rectifiers in course of manufacture at Baden and have witnessed them in operation on both traction and lighting systems, working alone or in parallel with rotary converters, and I was impressed by their noiselessness, the small space occupied, and the fact that they require no regular attention. They are insensible to variations in frequency, their regulation or voltage-rise between no load and full load can be adjusted between 5 and 17 per cent, and their overall efficiency, including the step-down transformer, is about 93 per cent at 500 volts and is practically constant at all loads. In first cost they are cheaper than rotary converters and can be installed in a less expensive building.

One disadvantage with them is that the arc (and therefore the direct-current supply) is not self-restoring after an interruption of the alternating-current supply, but has to be re-started by means of a small ignition arc temporarily set up between an electrically-controlled steel rod plunger and the mercury-bath cathode. It is not, however, difficult to foresee that when a fully automatic substation is required, some simple instrument of the relay type will be devised to restart a rectifier and connect it to the busbars after the alternating-current supply has been restored.

These rectifiers, the working principle of which was discovered in 1902 after years of patient research by an American (Cooper Hewitt), and which have been brought to their present state of practical utility by the ingenious Swiss, seem to hold in themselves great promise for the future of electric supply in suburban or more isolated districts and, if they do in time partially replace that wonderful machine the modern rotary converter, it will be one further instance of science emancipating men from the monotonous repetition work of machine attendance to enable them to use their natural abilities in a wider and a better sphere.

I have now described quite briefly a few of the engineering matters which are to-day engaging the close attention of those responsible for our large electric supply undertakings, and I have endeavoured to indicate how some of these questions may ultimately be dealt with. The whole subject is one of absorbing interest, and one to which many of us turn with relief and satisfaction in this time of economic fallacies and industrial unrest.

The work of electrical engineers is expanding daily as new discoveries are made, or as more efficient methods are devised. It is a splendid tradition of our profession, which we must all faithfully observe, that particulars of any new knowledge gained shall be published and made known for the benefit of all, and the proper channel for the dissemination of such knowledge is through this Institution to which we all have the honour to belong.

WESTERN CENTRE: CHAIRMAN'S ADDRESS

By A. J. Newman, Member.

(*Address delivered at* Bristol, 8 *November*, 1920.)

(ABSTRACT.)

Engineering Institutions.

Each year finds the Institution, as a whole, growing in membership, utility and prestige, and, largely as a result of this, the greater importance of engineering to the general community is becoming more publicly appreciated. Compared with the Civil and Mechanical Institutions, the Institution to which we belong is young in years, but its membership is already the largest of the three. The membership of just over 100 when it was founded in 1871 compares to-day with that of nearly 10 000, figures which testify to the romance of electrical development. Indeed, the activities of our modern Institution are so far-reaching that I submit no member is loyal, even to his own personal interests, if he fails to embrace the opportunities and benefits which at present it affords.

As to the future, it now appears practically certain that Technical Institutions, as a whole, will take an increasing part in the more general welfare of engineers. If it does not fall to the lot of our Institution, I feel sure that, ultimately, there will be other bodies recognized as authoritative in defining status and in forming a register of engineers. It seems to me that there is ample scope for all organizations which will work sincerely towards our greater public recognition.

Equally, nowadays, the necessity for the highest engineering skill and conduct can, indeed, hardly be exaggerated. The difficulty is, however, adequately to organize any engineering body such as those which look after the interests of the established legal and medical Societies, one reason being that there is a very much greater number of types of mind required for engineering. This goal is, therefore, still before us, doubtless largely owing to our own lack of combined effort. Unfortunately, the average engineer is left with merely a minimum of time for the study of economic and social questions, but he cannot disregard modern developments which may affect appreciably his personal liberty and position. The engineering mind is all for natural progress, and if, in these changing times, he can bring his creative well-balanced faculties and influence to assist in national and local government affairs, it will be all to the good.

Labour and Modern Prices.

Such new action seems especially necessary if, as is felt in some quarters, Labour is out for the ultimate control or dictation of industry. I am referring to this subject in view of the serious aspect of these modern industrial stoppages, such as the present coal strike. As an engineer chiefly interested in electricity supply, I may be safely credited with abnormal opportunity in which to deplore the ever-increasing charges demanded by manufacturers and workers alike. As for the more enlightened manual workers, one still endeavours to believe that they want neither charity nor revolutionary humbug, but prefer fair dealing, sound sense and sound progress. All general prosperity depends, of course, upon the prosperity of our sound public industrial undertakings, from which our workmen have the right to expect their fair share of profits.

It is, unfortunately, the unthinking section of Labour that causes most of our industrial stoppages, the section which wrongly contends that manual labour produces *all* wealth, and should therefore enjoy *all* wealth. If the claim were that all wealth is produced by *work*, meaning the work of hands plus brains, it would be logical. We know, for example, that one firm will succeed and another fail, though both employ the same class and quality of trade-union labour. The difference is, of course, in the management, as good direction, wise buying and selling will cause that firm to prosper. Yet the claim is repeatedly put forward, that the wealth made by the successful firm is made mainly through the medium or efforts of the manual workers, though they will never admit that the failure of the other firm is, in any way, attributable to any fault on the part of such workers.

Industrial and financial conditions are, unfortunately, so turbulent and precarious to-day that it seems incumbent upon all officials and thinkers to reason patiently with the workmen whenever opportunity for discussion

is afforded. In the past we have neglected this important aspect of verbal influence and mental co-operation, and, partly because of such failure, we are now reaping the consequences in this national strike. It should be preached, on every hand, that there is only the one certain road of escape from this crushing burden of rising prices and wages, and that is, not by further increasing them, but by working the instruments of production at their maximum pressure, and so enlarging as rapidly as possible the mass of commodities available. Indeed, the real and sole solution can still be given in the two words " Increase production."

At present we are making among ourselves an economic war which may well prove, in its way, as burdensome as the real war. One sincerely hopes, therefore, that all brain workers and officials will, in future, consider it part of their duty to preach the dual gospel of industrial peace (to steady trade and restore confidence) and maximum production from man and machine. In fact we are scrambling for the biggest share of a poor harvest, instead of increasing the harvest until there is plenty for all.

Electricity Supply.

Passing from the important factor of man power in industry to that of motive power, one turns, almost instinctively, to the problem of an ample and reliable supply of cheap electrical power. As a nation we cannot indefinitely distribute more than we produce, and, when all want more money and shorter working hours, we must solve the problem by greater provision of efficient machinery of production, of which power is certainly the most important.

The outcome of such vital considerations was the conception of the national scheme of electricity supply, and the passing of the Electricity (Supply) Act, 1919. With this Act members are doubtless generally familiar, though I might perhaps, with advantage, briefly review certain of its proposals as they appear to date. In the Parliamentary Bill, which resulted in the Act, provision was made for the appointment of either Joint Electricity Authorities or District Boards. The difference between a Joint Electricity Authority and a District Board is as follows :—The Joint Electricity Authority must be representative of existing authorized electrical undertakers (presumably all of them) within the district, although it is left optional whether it has also to be representative of county councils, local authorities who may not be authorized undertakers, consumers of electricity, and other interests. In the case of a District Board, however, representation must be given to all the interests I have just enumerated. With this exception, there is little difference between the Joint Electricity Authority and the District Board.

The Bill, upon which the Act was formed, provided that generating stations might be purchased compulsorily. Under the Act, however, the whole of the operations are voluntary, the acquisition of such undertakings being by arrangement. A further notable discrepancy between the Act and the Bill, is the entire absence of financial clauses.

The Act provides, in Section 18, that the Ministry in question " may construct works or acquire land." Although the term " land " is legally understood to include both the land itself and anything that stands upon it, it has been held that the Section in question does not provide, or allow for, the acquisition of works which are already constructed.

Under the Act it is not intended that any Joint Authority shall make any profits. The results of economies are all to inure to consumers. Such being the case, it at once appears that companies forming part of a Joint Authority would only profit indirectly through added facilities and an expansion of their business. Presumably, however, if such companies give financial assistance or guarantees, they might reasonably anticipate receiving some favour, as compared with other consumers who may not give such support. Similar remarks may also be applied to other authorized undertakers, local authorities and county councils.

Seeing that the acquisition of works is not compulsory, it may be asked what happens in the case of any particular undertaking not voluntarily falling into line with the proposals. The reply is that, since no new generating stations may be constructed, and no existing station extended, without the authority of the Electricity Commissioners, it is reasonable to suppose that no such works will be respectively constructed or extended if a supply can be given from the central authority at a figure at least as good as that at which the particular undertaking in question could itself furnish the supply. Further, one has always to bear in mind that, in all probability, those who support the scheme will be justified in asking for rather more favourable terms of supply than others. It may therefore be reasonably anticipated that, in most districts throughout the country, Joint Authorities will be formed, although there may be difficulty in amicably settling or balancing up all the interests of the various undertakers in each area.

The rate of progress which, however, might have been anticipated a year or two ago, will now probably be less rapid. Delay may result, in view of the present state of the money market, as the capital charges per kilowatt of plant installed are now probably as much as three times those which pertained before the war. Obviously, the new plant to be installed in a capital station would have to be compared with the cost of the existing cheaper plant belonging to present undertakers.

Under the Act, at the present time, money would have to be raised by the Joint Authority, on the security of the works taken over or owned by such authority. As money is so dear, and there are now so many sound investments giving a good return, one has seriously to consider if investors would be readily found to loan money to Joint Authorities on this security alone. I am of opinion that little could be accomplished in this respect, and that it is absolutely imperative that financial clauses, such as were contained in the Bill, should be passed. Lenders may then be given the security of the rates of county councils and local authorities, as well as the financial backing of companies within the area.

Whilst it is necessary to have the backing of the local rates, etc., it may be pointed out that, in giving such

backing, the local authority does not really incur any risk, because the Joint Authorities must be self-supporting, and there is no reason at all why they should not prove to be so. This assumes, of course, that their constitution is so drawn up that those who have voting power also carry financial responsibility proportionate thereto, such financial responsibility being borne either directly or through them by the authorities whom they represent, whether local authorities or otherwise. Owing to the present state of the financial market, it may be necessary, or at any rate wise, that Joint Authorities be formed covering districts, although there is no immediate prospect of giving a supply in such districts from a central source. It is most important, at the outset, that there should be unity of idea and purpose, and that efforts should be made for standardization throughout the district with this ultimate object in view.

General Design and Construction.

Under this heading I propose to review, in a general way, modern practice and tendencies as regards engineering equipment chiefly for power supply, confining my remarks mainly to electrical gear. It is gratifying to learn that our organizations, both from the manufacturing and technical aspect, are able to equip satisfactorily the biggest stations yet proposed, and that experts are available for their successful operation.

Referring to the actual lay-out and construction of a super-station, the main initial problems to be solved may be stated as follows:—(*a*) An adequate supply of water, and (*b*) the maintenance of a continuous supply of fuel in case of external disturbance. In connection with (*b*), the problems of coal and ash-handling plant have also to be faced, and in the case of these large power stations the capital outlay involved by such auxiliaries, with the necessary railway sidings, represents no inconsiderable portion of the total expenditure.

In order to minimize these difficulties in a new way, a *floating* power station has been proposed. Such a floating station, constructed entirely with its complete equipment, say on a huge raft, would seem to present certain definite advantages. If oil fuel were employed, the tankers conveying the fuel from the oil fields could be moored alongside the station, and the oil pumped direct from the vessel into the storage tanks. Thus railway transport charges would be entirely eliminated, as well as the risk of interruption to supply due to breakdown in railway facilities, strikes, etc. In addition, an adequate supply of cold water for condensing purposes would always be available, and the head on the circulating pumps would be a constant minimum. Such a new arrangement would also appear to have the advantage that, if the station were composed of separate units, any of these units might be transported without dismantling to another site, should necessity arise.

The construction of such a floating station would, of course, present certain difficulties. The raft might be of cellular construction, thus lending itself readily to commonly-accepted power-station design, in that certain of the cells could be utilized as condenser basement, oil and water storage tanks, etc.

It is not easy to conceive any situation providing adequate water facilities except on tidal water, and large estuaries generally offer the greatest advantage, as they allow of the super-station being placed more centrally with respect to the area of distribution. Effective mooring devices would have to be arranged, but, as such devices are provided already in the case of floating docks, it need not be anticipated that special difficulty would arise from this complication.

As regards the construction of the raft itself, that item would of course be expensive, but against this must be offset the cost of a suitable site and foundations for a power house erected on land, which, even though the site chosen may be waterlogged or otherwise unsuitable for other purposes, is very considerable. Moreover, no coal sidings, cooling towers, or culverts, and only a minimum of coal and ash-handling gear or water piping would be required, and, in addition, the mobile cold-water supply would ensure maximum efficiency.

As an alternative, such a station could, of course, be built by the sea-shore, and a wharf provided immediately adjacent for the accommodation of oil tankers, a floating pontoon being arranged to provide a constant suction head for the circulating pumps. The relative merits of the two alternatives would, of course, depend on the local conditions which prevail.

I might now refer to present manufacture of generating and auxiliary electrical apparatus for power stations. The enormous increase in cost, of both labour and material, and the enhanced value of money, are forcing manufacturers to consider every means for reducing the dimensions, and therefore the price, of their apparatus. Apart from minor refinements and improvements in design, the possibility of such reduction in cost lies along one of three main lines, i.e. higher speeds, higher temperatures, and better cooling facilities.

As regards turbo-generator sets, the present tendency is to increase the size of unit and operate it at the maximum possible speed for any particular frequency. Up to a certain point, the cost of the electrical generators decreases with the speed. On the other hand, a steam turbine for 3 000 r.p.m. is lighter, cheaper, and more economical than one of the same output for 1 500 r.p.m. Another tendency is towards increased economy. Owing to the difficulty of efficiently dealing with the much greater volume of steam met with in the latter expansions of such large steam turbines, it has been proposed to by-pass a quantity of this steam, up to say 5 or 6 per cent, and use it for heating the condensate. By this means, I understand that 4 to 5 per cent better steam consumption can be obtained, even when due allowance is made for reduced efficiency of the boiler-house plant owing to increased chimney losses.

The tendency in this country is now to use mica insulation on large machinery to a much greater extent, thus taking advantage of higher temperatures. Also all modern turbo-generators are now arranged for forced cooling, and, to protect the windings from dust and dirt, air filters are usually provided. Few people realize the enormous quantity of dust which is carried, either on or through the windings of a modern alternator. One can illustrate this point by the fact that the air surrounding a generating station may have as much

as 0·0006 lb. of dust per 1 000 cubic feet, and that a 5 000-kW alternator may be so designed as to require, say, 27 500 cubic feet of air per minute for cooling purposes. Even if the machine is only in operation for 50 per cent of its time, as much as 2 tons of dust per annum, under such conditions, has to be dealt with by the filtering arrangements employed.

The wet air filter has been extensively used during the last few years, but the air cooler seems now to be coming into more general favour. This cooler consists of a number of tubes, as in condenser practice, through which cold water is circulated. The hot air from the alternator forms a closed circuit, the cooler being placed somewhere in this circuit. With this arrangement there is no danger of water getting into the generator, the amount of dust carried by the air is negligible, and the danger from fire in case of breakdown is also minimized, because the amount of oxygen in circulation in the closed air system is necessarily very small.

Referring to the main lines of modern switchgear design, there appear to be two schools of opinion. One advocates totally enclosed or ironclad gear, and the other favours installations in masonry or other fireproof structures. The final decision will probably be based on the reliability or otherwise of the totally enclosed type, and its efficiency in continuity of service. In this respect it is important to note that, during the past year, totally enclosed ironclad main gear has been installed in the Dalmarnock station at Glasgow. Such gear has of course proved very successful for substation work, but it has not previously been tested as main power-station gear.

With regard to future switchgear progress, it is expected that construction on large gear is likely to become more and more robust. Switches are, in fact, already built for capacities up to 2 000 000 kVA. On air-break switchgear, wear must of course be reduced to a minimum, and, for oil-break gear, the problem is to provide against explosions likely to occur in breaking heavy currents. Outdoor weatherproof switchgear is also being developed, and it will be interesting to see what attitude the Home Office will adopt towards its extended use, especially as supply engineers are continually being faced with the difficulty of capital cost when called upon to give a small supply of power from distribution lines which may be in close proximity to the prospective consumer.

The question of metering the energy supplied has recently been receiving more technical consideration. Certain large supply authorities have been keen to realize the advisability of employing a competent meter engineer conversant with three-phase power measurement. The adoption of two, or three, meters in series for large consumers has proved a wise investment. Means of keeping a reliable check on the power factor has, so far, not been largely adopted. I think, however, the employment of a sine meter in conjunction with a watt-hour meter has been found to be an arrangement as satisfactory as any yet proposed for the purpose of ascertaining the average power factor over a given period.

Passing to the subject of converting plant, the main recent development here, on rotary converters, seems to be the gradual adoption of higher speeds. Refinements in recent practice also embrace improved design of field and armature windings, so as to eliminate "hot spots," and better damper windings giving increased stability and freedom from hunting. Substitution of a larger number of smaller brushes appears also to result in more satisfactory operation. Improved methods have also been developed for seasoning commutators at higher speeds and temperatures than are likely to be reached in practice. Special care taken in this part of manufacture is amply repaid by the greater elimination of commutator faults, such as high bars, etc., which often develop when a machine has been in commercial use for some time.

Much work has also been done in improving mercury-vapour rectifiers for large outputs, but it still seems difficult to estimate the limit of their practical use. In order to obtain parallel operation between different rectifiers, or between a rectifier and a rotary converter, I believe it is necessary to adopt a number of somewhat complicated devices

As regards modern transformer practice, considerable progress has been made in the development of types for high voltage and large output. For large sizes forced oil cooling is usually adopted, the oil being circulated through a cooler and back through the transformer tank. For one super-station scheme, six three-phase transformers, each for 20 000 kVA, with a ratio of 33 000 to 6 000 volts, are proposed. If such three-phase units are adopted, instead of single-phase, they will be the largest so far built in this country. The biggest sizes so far actually constructed appear to be those at Glasgow Corporation Dalmarnock station, viz. 7 800 kVA, single-phase oil-insulated forced-cooled, with a ratio of 20 000 to 6 000 volts, at 25 periods, three of which form a three-phase group of 23 400 kVA, which is relatively 45 000 kVA at 50 periods. Five such groups are in operation or being built. The largest three-phase self-cooled transformers built, so far, in this country as one unit, are, I understand, 4 000 kVA, with a ratio of 6 000 to 12 000 volts, at 50 periods. High-voltage types for 50 000 volts, 2 000 kVA, are at present being constructed for export to Spain.

Present necessity for economy in capital outlay is being met to some extent by the use of weatherproof transformers. One firm is at present building 12 000-kVA weatherproof groups for 66 000 volts, and 5 000-kVA groups for 33 000 volts, three single-phase units forming one three-phase group. Another tendency is towards the use of dry type air-blast transformers for voltages up to 11 000, due partially to the present shortage and price of transformer oil.

Considerable progress has been made in the linking-up of power stations, which necessitates, apart from the transformers, voltage-regulating equipment to compensate for line loss. Such regulators are of the step-by-step or induction type. The design which appears to be gaining most favour is that in which a step-by-step boost, positive or negative, is obtained by means of a pair of small subsidiary transformers. The regulation is procured through a step-by-step controller connected in the low-tension secondaries of these trans-

formers, there being only magnetic, and no electrical, connection between this controller and the boosted main. Consequently, the main circuit, having no stops or switches in it, is not interrupted, the regulation being done in perfect safety on the low-tension controller. For large power schemes there is now a tendency to design the alternators and main transformers with fairly high internal reactance, thus obviating the need for installing current-limiting reactances. If, however, external reactances are required, they are usually of the ironclad air-core type, though development is proceeding on the air-core concrete type.

An effort is also being made, on all large new schemes, to standardize high-tension voltages. A generating voltage of 6 600 and transmission voltages of 11 000 and 33 000 are being advocated. These pressures seem to represent the highest voltage for which alternators can be wound consistent with safety, and the most economical for the transmission distances at present being considered. Higher transmission pressures will doubtless be proposed when general linking-up is contemplated.

It is gratifying to know that a 33 000-volt cable is now a commercial product, and cable makers appear willing to go to considerably higher pressures. One of their difficulties has been pressure gradients, and, on this account, smaller sections than 0·075 square inch will probably not be made for voltages higher than 33 000.

Jointing troubles may be said to be overcome by the elimination of hygroscopic tapes, and at present cable makers are sacrificing high-resistance compounds in order to obtain a filling which will consistently contract down to freezing point. Much of the trouble in the past has arisen from contraction, holes being left in the compound which are under partial vacuum, thus allowing easy paths for high-pressure discharges.

I am inclined to suggest that the laying of cables direct in the ground will become more popular than the use of troughing and bitumen filling. I also favour transmission lines constructed on the split-conductor system, as it seems a pity to expend large amounts of capital on catenary-suspended pilot cables necessary for other systems of protection. In comparing the two alternatives, one must not lose sight of the difficulty and expense of maintaining overhead construction, which expense has to-day an appreciable capital value. Each transmission scheme will have to be considered on its merits, as such factors as contour of ground, saving of route length by avoiding detours, wayleaves, clearings, etc., may turn the scale in favour of either system.

Turning for a moment to consumers' machinery, I will only refer to the larger motors now installed. Some very large reversing rolling mills have recently been equipped, and still larger ones are adopting electric drive. Some of these individual motors take peak loads of approximately 20 000 horse-power. Direct-current motors are invariably used for reversing mills, in conjunction with Ward-Leonard motor-generator sets carrying heavy flywheels. For colliery winding, the perfection of the gear drive has given a great impetus to the high-speed alternating-current motor, and a great majority of winders are now being fitted with gears and such motors.

In order to correct for power factor, the use of synchronous motors is now being more considered. The synchronous motor can be arranged to start from the A.C. side with approximately 40 per cent of full-load torque. When this is not deemed sufficient, however, the synchronous induction motor may be adopted, which starts as an induction motor with heavy torque, and, when up to speed, is changed over to a synchronous motor, whilst it can be run at unity or leading power factor.

Touching upon the electrification of railways, it appears that the 1 500-volt direct-current system may be standardized, but a definite view as to this is not possible until the Electric Railway Commission has made its report.

A recent advance, in America, is the use of an automatic substation equipment for electric railways. The automatic gear can be set to start up and switch into circuit converting machinery at certain predetermined times, or upon a drop in voltage, and alternatively to shut down again such machinery when the load falls to a predetermined value. The conditions of operation in this country do not seem quite so favourable for the adoption of such substations here, but there appear to be many cases where they could be used to advantage.

Standardization of Apparatus.

Unfortunately, it is not, even yet, generally appreciated how serious is the extra cost of electrical apparatus due to the lack of standardized requirements and constructions. It would be in the interests of all concerned for consumers to adopt when possible, as their standard, apparatus conforming to the specification of some independent authority of repute, such as the British Engineering Standards Association, and leave unessential details as far as possible to the manufacturers.

It is also anticipated that ultimately some such authority may co-operate with makers in even standardizing general designs, with a view to limiting the number of types or patterns now installed. For instance, the various makes of motors now offered are exceedingly numerous, even important general dimensions, such as length or width between feet, and height of centres, being invariably so divergent that only minimum progress appears as yet apparent in standardizing such details, which of course are often vital when a user wishes to replace urgently any machine by that of another maker. The modern large industrial amalgamations by manufacturers have, of course, resulted in certain progress in this direction. It may be hoped, however, that in future makers will somehow agree among themselves to manufacture as far as possible to particular designs which are approved by some authority as the best or most suitable. The advantage of this, if carried out over the widest range of engineering products, would seem enormous, and would ensure the maximum mass production which is so vital to greater and cheaper output.

Scientific Management.

I will just note one or two advances under this heading affecting present manufacturing conditions. The principle of labour representation, in the control or direction

of industry is now carried out by means of works' committees and industrial councils. The growing complexity of technical manufacture and materials necessitates more intelligent if perhaps less skilled labour, and this intelligence is being developed by education. Such education is desirable so that workers may better comprehend industrial considerations and requirements, and take wider and less prejudiced views regarding changes which have to be made. This education is, in general, being undertaken in works' schools, and embraces also pure education, such as instruction in English and other cultural subjects, physical culture, etc., as well as instruction in practical shop operations. In some cases, lectures are given to foremen and workers by experts from outside the works. This is, of course, apart from the wider tuition which is now given by manufacturers to their professional apprentices or students. The factory is also rendered more efficient and comfortable by the provision of proper canteens, ventilation, lighting, etc. One hopes that the future tendency towards wider scientific management will not embrace the application of so-called laws worked out abroad and suitable only for abroad. In this country it can safely aim at the fullest endeavour to win the co-operation of every worker by fair dealing, encouraging each employee to appreciate his or her co-operative functions, and the necessity for eliminating waste in all directions such as waiting for materials, defective tools, unnecessary lifting and handling of material, bad transport, etc. Scientific management is essential to modern conditions, and its wider or sounder adoption will doubtless be only a matter of time.

Research.

Modern scientific management may also be said equally to involve the utmost use of research as a principle in fostering the natural growth of engineering development. Modern research is, of course, of two kinds, namely pure, which is done chiefly at the universities and existing national institutions, and applied, done chiefly at works or in industrial laboratories. The former deals with problems common to our industry as a whole, and the latter with problems peculiar to individual works' undertakings.

The findings of research are of the utmost importance to electricity supply engineers, and it is generally agreed that the powers conferred by the Electricity (Supply) Act, 1919, will go far to assist or supplement the research undertaken by manufacturing and institutional workers. The supply engineer is chiefly interested in the problems of transmission, distribution and application of electricity, and here his views and facilities will be especially helpful.

It is highly satisfactory to observe that numerous new bodies are now enthusiastically engaging upon research work, as the problems yet to be solved are far-reaching. There is, for instance, the more economical utilization of coal, the use of powdered and briquetted fuels, and low-grade fuels of the lignite type, and the fuller and better use of heavy and light oils. In the field of prime movers there is pure science research in thermo-dynamics and heat transmission, etc., and applied research on existing prime movers and their auxiliaries, such as steam and water turbines, the reciprocating engine, internal combustion engines for gas and oil, the gas turbine, combined internal combustion and steam engines, and the two-fluid turbine, etc. As regards electrical plant, the scope is equally important and complex and concerns improvements of existing constructions, and the development of new apparatus for all electric applications, which the national scheme of supply will do much to make possible.

I trust that the importance of attracting, especially to research work, highly suitable young men from the universities and colleges, will not be under-estimated. This indeed is essential, as our future progress as a leading nation, long after the present question of high prices and increased output is settled, must assuredly be proportionate to our technical and scientific advancement.

In conclusion, I may add that I am very optimistic, especially as regards the outcome of these great problems of research. Nor am I, in other directions, subject to any sustained pessimism. At present our great nation may seem almost hopelessly divided, or in tight circumstances, but there is no doubt that in the past it has bravely negotiated rougher tracks which appeared to lead nowhere. Prevailing unrest and strife may, to some extent, be due to what one might possibly term humanistic research, out of which good may come. In spite of formidable difficulties during the past year or two, much has been accomplished. Given peace, conscientious effort, honesty, and fair-dealing between all parties, I shall prefer to remain an optimist.

NORTH-EASTERN CENTRE: CHAIRMAN'S ADDRESS

By J. R. Beard, M.Sc., Member.

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POST-WAR CONDITIONS AND DEVELOPMENTS, WITH PARTICULAR REFERENCE TO THE ELECTRICITY SUPPLY INDUSTRY.

(ABSTRACT.)

I. General Considerations.

The terrible strain of more than four years of war under the intensified conditions of the modern world of science and industry, the alternations of failure and success, the continual crises and the final almost unexpectedly complete triumph, appeared at the time to have absorbed the whole emotional capacity of the nation.

The ordinary man assumed, often unconsciously, that the nations as a whole would gladly return to peaceful pursuits and that the principles and benefits of national, and even international co-operation, learnt during the war, would continue to be applied in peace, thus not only balancing the economic losses of the war, but permitting of an increased standard of living among the poorer clasess. If nations could be run as business concerns, this is what might have happened; but, when the unifying influence of immediate danger was removed, distrust, jealousy, ambition and greed rapidly sprang up between nations and classes and individuals alike, and now, two years after the armistice, we are in the midst of an aftermath of conflict and disturbance, which is as world wide and as perilous as, and in many ways more complicated than, the war itself.

A careful study of English history a century ago shows several remarkable resemblances to the present day. As we all know, it was a time of international strife, as severe, relatively to the conditions of the age, as the recent wars, but it is not so generally remembered that it was also a period of acute industrial strife. Between 1760 and 1832 occurred the industrial revolution, and the history of the time reads like a history of civil war. It was the period of the invention and introduction of machinery, and the fight centred upon who should get the benefits of the resulting increased production and wealth. The present generation has seen the discovery of mass production and of the reduction of costs by combination and the elimination of competition. Once again the struggle centres upon who shall reap the benefits, and once again it seems obvious to the impartial observer that they should be spread over the entire community. That this was, on the whole, the final outcome of the former bitter struggle is, to some extent, a ground for hope that the present unrest may work itself out to a similar basis of general mutual contentment.

During the past half century the masses have gained their intellectual freedom, and now the vital need is for them to be taught how to make the best use of it. They are much in the position of the boy verging on manhood who feels his training is over when it is just beginning, and who realizes his powers without feeling his responsibilities. It is a critical period in every boy's life; he must not be bullied or driven, but yet he must be directed, which involves restraint in some directions and encouragement in others. It appears to me that modern industrial life is at a similar critical stage, and moral as well as material help will be needed if the nation is to recover. A higher aim in life than having more money than one's neighbour, a truer appreciation of the psychological bases of happiness, a more varied and intelligent use of leisure hours, and a greater sense of individual responsibility in communal affairs are essential to all classes alike. They would make the richer classes realize the need for remedying the glaring material inequalities of the present structure of society and would show the poorer classes that progress, to be sure, must be gradual, while both would learn that collective effort accomplishes more than individual effort, whether the units be individuals or sections of the community.

If the national activities could be organized like the team work in the football field and the same sporting competitive spirit encouraged between nations as exists in our leisure occupations, there would be some hope for the world. Bitterly do the employers of to-day realize how the selfish individualistic cutting of piece rates in the past has recoiled on their heads to-day; equally bitterly will employees in the future regret the modern tendency to the limitation of individual output. The greatest need of the day is for both to reach a mutual understanding and build up the national team on the two cardinal principles that maximum individual effort coupled with maximum co-ordination produce the maximum result, and that each member has individual responsibility for the success of the whole.

Two other features of modern industrial life which I can only touch on here are the general reduction of hours of work coupled with an increased standard of living, and the over-concentration of the population in manufacturing centres.

Hours can only be shortened without decreasing production and wealth, if each individual produces

more in the same time ; and if, in addition, the standard of living is to increase, production per hour must increase at a greater rate than the reduction of hours. This can only be accomplished by a more extended use of machinery, and, hence, engineers must do more work in order that the mass of the population may do less.

Over-concentration is again mainly a problem for the engineer and is becoming more urgent each year. Our towns have passed their economic limit in density of population, with the result that there is a great waste of time and labour in unnecessary transport of both people and materials, while the value of land in such congested centres is such that it is not economically possible to provide sufficient fresh air per inhabitant. One proof of this is that, although rents are highest in densely populated areas, such areas are usually saddled with the highest rates.

II. Electric Power as an Aid to National Recovery.

When national recovery is viewed from the more material side, it is apparent, both from a study of economics and also from the experience of nations in the past, that the most important factor in quickly raising the burden of taxation and restoring the national wealth is some special development of applied science. The general application of steam power to industry was the main source of England's speedy recovery from the Napoleonic Wars. In more recent times the burden on the community of the American Civil War, which was waged on a scale still scarcely appreciated in this country, was forgotten in 10 to 15 years owing to the discovery and development of the enormous mineral wealth in copper, iron and coal lying ready to hand in the unexplored west. To what are we to look for similar assistance to-day ? Beyond the bare possibility of the discovery of the key to the almost unlimited energy locked up in the atom, only two main sources are in sight, scientific organization of production and the closely allied question of the general application of electric power. The first is too wide a subject to be dealt with here, but the second is the immediate concern of us all as electrical engineers, and we must, therefore, exert ourselves to the utmost to be worthy of our place of honour in the national team.

Broadly speaking, the steam turbine was not such a radical innovation as was the reciprocating steam engine and, similarly, electric power as hitherto applied to industry is more a refinement than a new departure ; but as our most promising means of increasing production it is of the greatest importance to the nation at the present time. It is a striking fact that in the whole of the United States the consumption of electricity per annum per head of the population in 1919 was 368 units, while even on the North-East Coast, which is admittedly the most electrically developed part of our country, the corresponding figure was about 260 units.

In other ways, however, electric power will produce more radical changes in the life of the community. Due to the ease and cheapness with which it can be distributed and the efficiency with which it can be utilized in the smallest quantities, electric power promises a revolution in reducing and simplifying the labour of the household in a way which can be approached by no other agency. Progress in this field has lagged behind progress in the industrial field, with the result that, generally speaking, the conditions under which the men of to-day are working show a relatively greater improvement than those of the women. In this country a power-driven washing machine or vacuum cleaner is looked upon as a luxury. In Chicago the monthly sales of the supply undertaking, i.e. excluding sales through contractors, are 650 washing machines and 1 200 vacuum cleaners. There are now 82 makers of washing machines in the States, and the various electrical firms are making preparations to manufacture fractional horse-power motors for this and similar domestic purposes by the million.

Electricity will also be a most important factor in dealing with the question of over-concentration, both of industry and of population ; the latter through cheaper and more efficient transport, the former through the ease with which relatively small works concentrating on a few lines of specialized production can be spread over the country without loss in efficiency of the application of power and with consequent economy due to lower rates, cheaper land and cheap well-lighted single-story buildings covering a relatively large area. An equally important share in facilitating decentralization will be taken by another branch of this Institution, viz. our friends the telephone engineers, and I hope that they will bear this in mind in the expenditure of the £10 000 000 which can be raised for telephone and telegraph developments under the recent Act.

There is no doubt that in the international electrification of industry this country holds a natural advantage due to the low cost of distribution resulting from our dense population and our relatively small area, added to which we are not faced with expensive long-distance transmission, since our long coast line, frequent rivers, and widely distributed coal-fields make it economical for us to generate separately in each of the much discussed 16 areas, the main requirements of the particular area without serious need for high-capacity interconnection between them. This is clearly shown in the area covered by our Territorial Centre. It is traversed by three large rivers and several smaller ones, it has coal-fields in the north and the centre, and it is bounded on all three land sides by very sparsely inhabited country, such as the foothills of the Cheviots and the Pennines and the Yorkshire moors.

The steady pioneer work in the electric power supply industry during the last 20 years bore most astonishing fruit during the abnormal growth of certain of our industries in the war period. It is estimated that 95 per cent of the new mechanical power was applied through electric motors, while in 557 undertakings the average increase in plant capacity was 101 per cent. Such striking figures caught the imagination of both politicians and business men, and for the time we had the stimulating, if in some ways rather alarming, experience of forming a plank in a political platform and of appearing in the speeches of the Prime Minister.

Generally speaking, the momentum of this wave of enthusiasm has been useful in removing many long-standing obstacles to the proper technical development of electricity supply, fortunately without, so far, carrying the changes to undesirable extremes. The reorganization of the Government's control of the industry by the Electricity (Supply) Act of last year, while still amenable to improvement, is much more thorough and satisfactory than any electrical engineer would have ventured to hope for previous to the war.

During this year the appointments have been made of the Electricity Commissioners who are to carry into effect the provisions of the Act, and these appointments have not only proved acceptable to all the diverse interests in the industry but have been such as to give confidence to the industry as a whole. The Commissioners have already made considerable progress with the necessary preliminary work, such as delimiting areas and initiating schemes for unified systems of power distribution centring on the more densely settled industrial areas of the country, but it would be unfair to place the whole responsibility for this reorganization upon them alone. Their main duty is to lay down the correct lines of policy, and, to the extent of their powers, to guide developments accordingly. Without the constant and energetic support of the industry as a whole the work of the Commissioners will be greatly handicapped, and it is, therefore, to be hoped that supply undertakings and their managers will succeed in setting a high standard of team work for other classes of the community to live up to. This is so vital to proper development that at a later stage I propose to refer to it a little more fully.

III. Problems Confronting the Electricity Supply Industry.

It is now seven years since our past chairman, Mr. C. Vernier, in one of the most inspiring Addresses delivered before this Centre, dealt in a very comprehensive manner with the problems then affecting the electricity supply industry. In some respects considerable advances have been made, but in many more we still seem little further forward, and that Address, together with Dr. Ferranti's still earlier Address as President, are well worth re-reading to-day. While I do not propose to attempt to deal so completely with the same subject, there are a number of matters of immediate interest to which I believe attention can usefully be directed. These appear to me to fall under six main headings:—

(1) Adjustment to post-war conditions.
(2) Technical advancements towards economy in
  (*a*) Generation.
  (*b*) High-pressure transmission and distribution.
  (*c*) Detail distribution.
(3) Tariffs.
(4) Provision of capital.
(5) Relations with employees.
(6) Co-operation between undertakings.

(1) *Adjustment to post-war conditions.*—I have placed this first for two reasons; none of us has yet fully awakened to the fact that the true value of money is not its amount in pounds sterling but the commodities for which it can be exchanged, and, secondly, both the actual and relative values of various kinds of material and labour have appreciably changed. Mr. Frank Walker has dealt with the general subject very lucidly in an article in the *Beama Journal* * which I would commend to the notice of members.

Fig. 1 reproduced from this article shows the rate at which the cost of an average combination of labour and material has increased, and for comparison I give a corresponding curve for similar costs in the United States. The rate of increase of the latter would be greater if expressed in British currency, due to the declining rate of exchange.

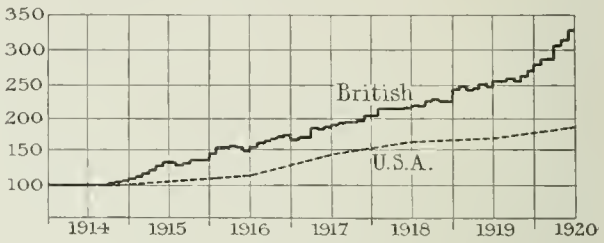


Fig. 1.—Rate of increase in cost of an average engineering combination of labour and material.

To the figures given by Mr. Walker, I may add the following approximate figures which are of direct interest to those engaged in electricity supply.

Table 1.

	Relative Cost July 1914	Relative Cost September 1920
Foundation work and civil engineering	100	295
Steel-framed buildings	100	280
Boiler plant	100	360
Turbo-alternators and condensing plant	100	325
Substation buildings	100	300
Switchgear	100	240
Transformers	100	330
Rotary converters	100	315
11 000-volt † cable without excavation	100	180
Low-pressure cable without excavation	100	215
Excavation and reinstatement for cables	100	505
Creosoted wood poles	100	375
Line insulators	100	350
Bare copper strand	100	195
Insulating oil	100	240
Motors	100	330
Metal filament lamps ‡	100	100
Lighting fittings	100	200
Disposal of ashes	100	380
Manual labour in power stations §	100	390
Salaries of technical staffs §	100	220¶

* "Engineering Costs, July 1914 to June 1920," 1920, vol. 7, p. 9.
† During the period considered the thickness of insulation has been reduced.
‡ Vacuums have gone up, but gas-filled have gone down and are now used very extensively, so that the average is about as shown.
§ The figures allow for the reduction of hours which has taken place between the periods considered.
¶ This figure will vary between wide limits for different undertakings, due to the fact that in pre-war times employees engaged in similar work received very different remuneration. It will also vary widely between the different grades of staff.

TABLE 1—*continued.*

		Relative Cost July 1914	Relative Cost September 1920
Typical power-station coal:			
London District	Per Ton	100	249
	Per B.Th.U.	100	309
West of Scotland	Per Ton	100	377
	Per B.Th. U	100	384
North-East Coast	Per Ton	100	380
	Per B.Th.U.	100	354
Return on capital invested at the time in:			
2½ % Consols	Actual	3·3%	5·45%
	Relative	100	165
Typical Corpn. Stock	Actual	3·7%	5·9%
	Relative	100	159
Typical Industrial Pref. Share	Actual	4·8%	7·1%
	Relative	100	148

Electricity supply:—

	1913–14	1919–20
Number of undertakings examined	283	304
Total Working Expenses / Total Revenue	46·5%	64·3%
Gross Profit / Total Working Expenses	115%	55·6%
Gross Profit / Capital Expenditure	7·57%	7·76%

Gas Supply:—

Number of undertakings examined	52	50
Total Working Expenses / Total Revenue	80·2%	87·5%
Gross Profit / Total Working Expenses	24·6%	14·3%
Gross Profit / Capital Expenditure	5·06%	5·74%

From Mr. Walker's figures and the foregoing table the following general conclusions may be drawn:—

(1) Electrical transmission and distribution are relatively favourably situated as regards materials, due to the comparatively low increases in copper and lead.

(2) The electricity supply industry benefits from the small amount of unskilled labour employed.

(3) It has been affected by the changed conditions no more than the gas industry which is its rival in the domestic field.

(4) The relative proportions of capital costs, labour and fuel are approximately the same as they were before the war, but, due to increased rates of interest, capital charges are relatively greater.

(5) The revenue has not increased by anything like the same proportion as the working costs, due to a large proportion of the capital employed having been expended in pre-war times. This difference must, however, gradually become smaller since the pre-war plant is steadily wearing out, and all repairs, renewals, and replacements must be carried out at post-war values.

(2) *Technical advancements towards economy.*—In dealing with this part of the subject we, in this Centre, are fortunate in our association with an area of the country which, due to various reasons, but primarily to the initiative, courage and foresight of a few local engineers, has been an experimental ground for most of the advances which have taken place during the past 20 years. In addition, I have personally had the good fortune to have recently paid a short visit to the United States under conditions which brought me into close touch with several of the large supply undertakings and the chief electrical manufacturing works. Since my return we have also had the pleasure of a visit from two leading American engineers which has been very encouraging to many of us in that, although they undoubtedly have much to teach us, we have in turn many developments which have proved of interest to them, even though most of our operations are necessarily on a somewhat smaller scale.

Before considering the sub-headings into which I have divided this section, it is very necessary to emphasize the importance of always bearing in mind that it is the true overall economy which is the ideal. Not low steam consumption or even low B.Th.U.'s per unit in the power stations; not maximum carrying capacity in the distribution system, but minimum overall costs taking into account capital charges, labour, materials and continuity of supply.

(*a*) *Generation.*—Economy in generation can be sought in at least eight directions:

(1) Increased use of plant.
(2) Reduction in proportion of spare plant for overhaul and stand-by.
(3) Utilization of all available forms of waste energy.
(4) Reduction in capital costs per kilowatt.
(5) Reduction of operating expenses.
(6) Improvement in thermal efficiency.
(7) Utilization of low-grade fuel.
(8) Combination, or co-operation with other allied industries.

Although the war has accustomed us to the economics of mass production, it is significant that the first six headings are at once seen to depend upon supplying the requirements of each industrial area from a single unified system, while on the most cursory examination the same can be said of the remaining two.

The day is, fortunately, past when the disadvantages of the parochial character of the majority of our undertakings required special emphasis before a technical audience, and the more obstinate non-technical difficulties are considered later under the heading of "co-operation."

The first requirement of such a unified system is a general network of mains, of the same frequency but not necessarily the same voltage, covering the entire industrial area and so much of the agricultural fringe as can conveniently be incorporated with it. With such a network full advantage can be taken of the diversity between the periods at which individual consumers and the separate industries require their maximum supplies, spare plant can be pooled, and surplus energy from certain industrial processes absorbed

into the general system, or shortage of such energy for local requirements made up from the system. The unified network is, therefore, sufficient in itself to deal with the first three items, and experience shows that in this way alone the capital expenditure on generating plant can be halved, or, to put it in another way, each kilowatt of plant can produce annually twice the number of units.

The natural development from the unified network is the concentration of generation on the most suitable sites using the largest units of plant, or to adopt the phraseology of the day, the "super power station." Advantages immediately accrue under items (4), (5) and (6), due to the mere increase in size, while further advantages can be obtained under items (6), (7) and (8), from the more skilled and specialized design and supervision which can be afforded by a large undertaking. These six ways of improving economy are interdependent and, in consequence, the design of such a station is one of the most interesting problems in modern engineering, involving, as it does, a most intricate balancing up of numberless alternatives requiring a high degree of technical skill and judgment on the part of the designing engineer. Perhaps the most fundamental problem is to form the correct balances between capital charges and running expenses on the various sections of the plant, and by far the most important balance is that involving the thermal efficiency. The general principles were laid down in 1904* by Messrs. C. H. Merz and W. McLellan in their classic paper before this Institution on "Power Station Design," but in view of recent changes in relative values it is a suitable occasion to consider them afresh. This is done in a somewhat modified form in Fig. 2, which shows the additional capital expenditure it is economical to spend in order to save one-tenth of a pound of coal per unit, taking into account load factor, cost of coal, and rate of capital charges. For typical present-day figures of 40 shillings per ton for coal, and 12½ per cent for interest and depreciation, the maximum additional capital per kilowatt is £6·3 at 100 per cent load factor and, of course, half this, or £3·15 at 50 per cent load factor. The diagram only gives approximate results, as it is based on the assumption that to save coal it is only necessary to spend more capital. In practice various other items such as repairs, labour, electricity for driving auxiliaries, oil, water, etc., enter into the question to some extent, but it is not easy to deal graphically with these, and in an accurate determination the total annual costs of these items must be separately determined and deducted from the annual value of coal saved (on the middle ordinate) before carrying the horizontal line across to the capital charges line.

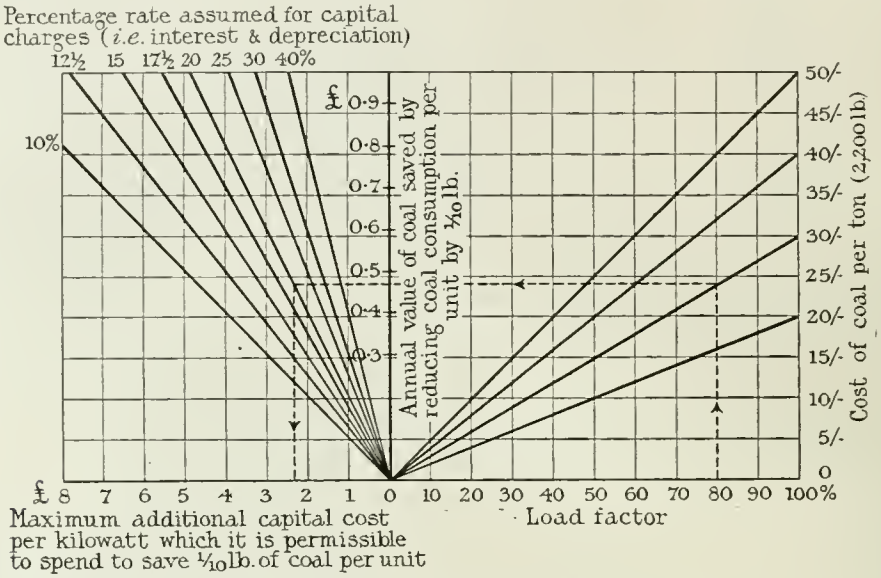


Fig. 2.—Curves showing additional economical capital expenditure to save one-tenth of a pound of coal per unit, taking into account load factor, cost of coal, and rate of capital charges.

The diagram makes clear a fact which was pointed out nearly 18 years ago by Mr. C. Turnbull, viz. that in a large undertaking the plant should preferably vary in fuel consumption according to its duty. For instance, if the total load curve be of the type illustrated in Fig. 3 (*a*), this can be replotted in the form shown in Fig. 3 (*b*). It is then apparent that to obtain the best results 10 per cent of the plant should be designed on the basis of 80 to 100 per cent load factor, 30 per cent on the basis of, say, 60 to 80 per cent load factor, and so on. Naturally such a scheme cannot be applied in a very rigid manner, as we are not dealing with a subject which has technically reached its full development. Consequently, it is generally safe to assume that new plant will operate at a very high load factor and will, therefore, justify considerable expenditure on fuel economy, while the more antiquated and uneco-

* *Journal I.E.E.*, 1904, vol. 33, p. 696.

nomical plant will be reserved for meeting the purely peak demands.

The advantage of burning low-grade fuels is, of course, their relatively low cost, which is due to the difficulty of burning them without special appliances and to the higher transport charges resulting from their greater weight per B.Th.U. Electric power generation is the ideal form of use for them, as it minimizes both these disadvantages. Except in special circumstances and at very high load factors, however, it is not economical to burn them immediately adjacent to their source, but each case needs consideration on its merits, balancing not only transport of fuel against transmission of electricity but also taking into account the overall cost of generation in the two alternatives. Wherever it is burnt, Fig. 2 shows that it is not economical to aim at as high thermal efficiency with low-priced fuels as it would be with good coal and, further, since cost of transport is equivalent to an increase in cost of the fuel, the thermal efficiency of the power

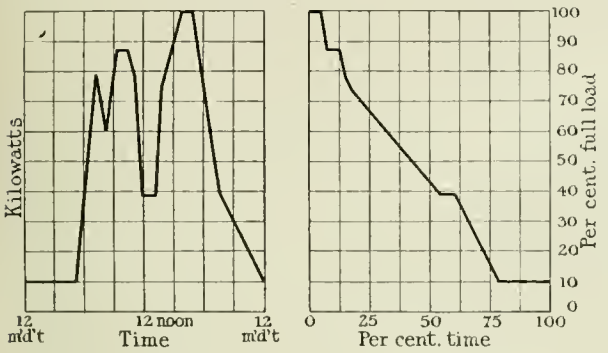


Fig. 3 (*a*). Fig. 3 (*b*).

Fig. 3 (*a*).—Typical load curve.
Fig. 3 (*b*).—Same typical load curve in percentage form.

station should be lower when the fuel is burnt at its source than when it is transported. Another general point to bear in mind in connection with low-grade fuels is that the plant required to utilize them is, naturally, more expensive to provide and maintain than plant for dealing with normal coal; hence it is desirable to make the fullest use of the more expensive plant, i.e. to run it at the maximum load factors. In fact, it is often only by running on a high load factor that it is economically practicable to burn these low-grade fuels at all, so that it is almost essential to combine such a plant with an extensive general-supply network.

A somewhat analogous problem arises in some industrial areas where there are no sites in the areas themselves at which sufficient cooling water is available for condensing purposes. The cost of transmission from suitable sites outside the areas is then equivalent to an addition to the capital costs of the outside stations and, if the distance is sufficiently great, this may easily exceed the additional capital costs of the cooling towers and sites for the inside stations. In such a case there is usually a certain limiting load factor below which the less-efficient cooling-tower station inside the area has the net advantage. If the demand is sufficiently large it will, therefore, pay to divide the generation into two sections: one outside the area dealing with all that portion of the load which is above the limiting load factor, and one inside the area, adjacent to the load, to take the remaining low-load-factor portion. To arrive at this division the whole demand of the area will have to be analysed in the form shown in Fig. 3 (*b*).

There is one important direction in which improved economy can be obtained without capital expense, and that is by concentrating the attention of the station operating-staff on the boiler house and by increasing the technical skill of those responsible for supervising the burning of the fuel. To some extent this is already appreciated by many undertakings but, on the whole, our American friends are much more alive to the point than we are. They have found in their boiler houses that increased supervision, and also supervision which is individually more highly skilled and therefore more highly paid, is a paying proposition. In fact, the present tendency is to rate the boiler-house superintendent, if anything, rather above than below the engine-house superintendent, while the chief charge engineer is encouraged to spend the greater part of his time in the boiler house.

Similar principles in balancing capital charges against running charges should be applied also to those items of the power-station equipment which do not directly affect fuel economy, such as coal and ash-handling plant. For instance, it is justifiable to spend much more capital in handling the continuous stream of coal into the boiler houses than on the infrequent handling of coal into and out of the store-heaps which most power stations provide for tiding over emergency conditions.

The last item—combination or co-operation with other allied industries—is one on which much thought and experimental work is being expended at the present time. Most of this is in connection with low-temperature carbonization of the coal previous to its being burnt under the boilers, the object being to obtain the valuable by-products, such as motor spirit, which are produced by this process. In other directions mutual economy is technically possible by interworking; with the gas industry—as is done in Chicago—with electric vehicle companies, such as that recently formed in this district, with certain electro-technical and electro-metallurgical industries and with schemes for public supplies of hot water.

(*b*) *High-pressure transmission and distribution.*—In dealing with generation, efficiency has been one of the most important items, but, once the power station is left behind, much the most vital consideration is low capital cost. This is not to say that efficiency should be ignored, and in a paper before this Institution* I have already dealt very fully with the correct balance between capital charges and cost of losses in the case of mains, but, once a reasonable amount of copper is provided, the most important feature in design is to obtain the minimum capital charges for interest and

* *Journal I.E.E.*, 1916, vol. 54, p. 125. In applying this to post-war conditions it should be borne in mind that, due to the relatively greater importance of capital charges as compared with running charges, the tendency will be towards somewhat higher current densities. On the other hand, the relatively low price of copper tends towards lower current densities.

depreciation* consistent with security of supply and the maximum safety of both employees and the general public, and it is on this basis that I now propose to consider the question.†

In this country there has hitherto been little in the way of transmission, as with our cheap coal and relatively poor water-power resources it has been economical to generate in proximity to the load. While loads are small it is unnecessary to use pressures in excess of those which can be taken direct into industrial substations, since, due to physical limitations, neither overhead lines nor cables can be economically designed for low carrying-capacities at very high voltages. In some areas this stage is already passed and in time, as electricity supply develops, this will apply to many other areas. Considerable attention has therefore been recently directed to the use of pressures above 20 000 volts, and several schemes are already under construction in which a voltage of 33 000 has been adopted, while in other cases still higher pressures are in contemplation. With such pressures the cost of mains and switchgear is too high per unit of plant to permit of their use for general distribution and, accordingly, practice seems to be crystallizing into the adoption, for such areas, of a transmission system at very high pressure covering the whole area and feeding, at a relatively few selected points, a high-pressure distribution network (operating at what is still officially called E.H.P.) which supplies direct to the larger consumers and to substations for transforming to low pressure for detail distribution.

In considering our British transmission problem it is essential to distinguish clearly our special conditions from those obtaining abroad. Except in a few special circumstances we do not require to use pressures much above 20 000 volts from the point of view of pressure drop. We require them in order to enable us to reduce the cost of transmission per mile. Further, although the wayleave clause of the new Act greatly facilitates the use of overhead lines, our country is so densely populated, and even the rural portions are so broken up with villages, farms and plantations, that it would be impracticable to adopt a standard pressure which involved overhead construction throughout. Accordingly, a considerable responsibility devolves upon our cable manufacturers, as the maximum voltage for which they can build a satisfactory 3-core cable should undoubtedly be the standard transmission voltage. Considerable progress has been made during the last few years, and to-day I understand that most makers are prepared to supply cables for a working pressure of 44 000 volts, while 55 000-volt cables are being made commercially in small quantities, and there is general expectation of carrying the maximum pressure successfully to 66 000 volts. In order to keep the cable diameter and the weight of the drum lengths within reasonable limits, this progress has only been possible by improvements in manufacture which permit of greatly increased stresses in the insulation, accompanied by reduced dielectric losses. A certain amount of caution must be used in applying test results to actual practice, and some uncertainty exists as to the behaviour over extended periods of cables operating at these high stresses. It may therefore be of interest to mention that, by co-operation with the cable makers, several special lengths of single-core cable with from 0·1 to 0·15 inch of insulation have been for over a year in successful commercial operation on each phase of a heavily loaded 20 000-volt system with the neutral earthed through a limiting resistance.

The adoption of a sufficiently high transmission pressure resembles the planting of a forest, in that it calls for a long view of ultimate gain to be obtained. Given sufficient load it would appear that the most economical pressure is at least 50 000 to 55 000 volts, but if the load per feeder is limited the lower pressures have the advantage. With many undertakings the load in the next few years is not likely to reach the amounts which would justify the highest pressure and hence, at present, there seems some danger of the extended adoption of pressures which are too low for maximum ultimate transmission economy and too high for general distribution purposes.

The co-ordination of railway electrification with general power supply will have a great influence in facilitating the development of these transmission systems. When the railways were built they naturally followed the industrial channels as far as possible, and since they were built new industries have naturally developed along the railways, so that transmission lines following the general run of the railways (though not necessarily directly on the railways themselves) are in a position to pick up both railway and power loads. This combination means increased loading per feeder and hence more economical transmission, and at the same time a higher pressure for the maximum economy. From this standpoint the recent unanimous recommendation of the Electrification of Railways Advisory Committee appointed by the Minister of Transport, that the power requirements of the railways should be taken at the same frequency as the standard frequency for the corresponding power supply area, is of the greatest importance.

The adoption of a general high-pressure distribution network fed at several points from a superimposed transmission system at a much higher pressure introduces some new problems. How frequently shall the two systems be connected, and what pressure shall be adopted for the distribution network? These are to a certain extent interdependent, and the problem is complicated by the great variations in load density and in the magnitude of individual loads which are met with in practice. My investigations indicate, however, that the curves of overall costs, using different alternatives, are relatively flat over a considerable range. This is a fortunate property of many such curves which are met with in examining engineering problems, and it is particularly fortunate in the case under consideration where the variations are almost entirely outside our control. The greater the load density and the larger the individual load, the higher the economical

* Low depreciation is assumed to involve automatically in most instances a low cost for routine maintenance and repairs.

† An exception to this is the case of conversion to direct current, but it is now generally agreed that the balance of advantage is in favour of distributing alternating current to the industrial or domestic consumer, so that the question only crops up in the case of traction or special supplies, such as electrochemical works.

distribution pressure, and, as the individual loads tend to be greater on the average in the less dense areas, the net result tends towards an evening-up of the theoretically desirable pressures. The most universally suitable pressure appears to be from 10 000 to 11 000 volts, and from the point of view of standardization I consider this should be encouraged for all new work, but the variation in economy due to using 5 000 to 6 000 volts in town areas, and 20 000 volts in colliery areas, is not sufficient to justify any wholesale change-overs of existing systems. The number of feeding points into the distribution network will, naturally, vary to some extent with the pressure adopted for transmission. The higher the latter, the greater the initial cost of tapping it, and hence the larger the area which can economically be dealt with from each tapping. While it is very difficult to give definite figures, it may be useful to give certain indications of the order of the problem, and I think it may be taken that each main distribution centre on the transmission network should deal with, say, 15 square miles in very dense areas and, say, twice this in areas of medium density. In general, too, it may be assumed that the capacity of each main distribution centre will be greater, the denser the area.

When we come to consider how the capital charges on the distribution network may be reduced, a number of methods of attack present themselves which may be summarized under seven headings:—

(1) Revision of regulations and of antiquated safety precautions.
(2) Increased cost of non-duplicate or semi-duplicate supplies.
(3) Increased use of overhead lines.
(4) Reduction of unduly high factors of safety of cables and overhead lines.
(5) Maximum use of the carrying capacity of the mains.
(6) Reduction in the amount of switchgear.
(7) Saving on buildings.

I do not include in this list the large savings which expenditure on proper feeder protective gear can effect by permitting an extensive use of ring mains and general interconnection, as I have already dealt with this very fully in the paper to which I have previously referred.* In any case I think this is quite generally appreciated in this country at the present time. It is, however, of interest to note, in passing, that this essentially British development with which this Centre has been so intimately connected is at present arousing considerable interest abroad, both in America and on the Continent.

(1) *Revision of regulations and of antiquated safety precautions.*—While for our more highly settled conditions American methods may err on the side of taking undue risks, we on the other hand, err from too great caution. As a new and somewhat mysterious agent, electricity was hemmed round in its infancy by numerous Government regulations. Now that electricity has reached years of discretion it is time that these regulations were thoroughly revised, and I understand that steps are already being taken by the Institution, at the invitation of the Electricity Commissioners, to consider what changes are desirable. This is such an important matter to the industry that it should receive the widest and most detailed consideration, and I suggest that before any final conclusions are reached an opportunity should be afforded to each Territorial Centre to arrange for a local discussion on the existing regulations which could form the basis of a report to the Council from the Committee of the Centre.

(2) *Increased use of non-duplicate or semi-duplicate supplies.*—During the early days of public electricity supply for industrial purposes a good deal of prejudice existed, and consumers were inclined to treat as a serious breach of faith by the supply authority short-period interruptions to the supply which they would have thought relatively little of if they had occurred on their own private generating plant. In consequence it became customary to provide a complete duplicate supply, resulting in considerably increased capital cost which was naturally reflected in the price which had to be charged to the consumer. Although any prejudice against taking a public supply is, fortunately, to-day a thing of the past, the tendency to unnecessary duplication still survives, so that, for instance, when a mill driven by a single engine is electrically equipped one often finds two cables and two transformers each of the full capacity. Each individual case must necessarily be considered on its merits, so that no general rules can be laid down, but in view of the present high capital charges the question of using a single cable, a single transformer, or both, is worthy of the most careful attention, as it has perhaps a greater effect on the capital expenditure than any of the other items. To-day a single cable is more easily justified than a single transformer, and one of the greatest needs of the day is an improvement in the reliability of the static transformer.

(3) *Increased use of overhead lines.*—This calls for no detailed comment here, except that it is highly desirable that the methods adopted for protecting the network should be equally suitable for underground cable or overhead line, so that there need be no difficulty in using the latter on any sections of the network where it is otherwise permissible. Incidentally, a further necessary feature of the protective schemes is that they should be suitable for tee connections in view of the increasing use of non-duplicate supplies.

It would appear that the time is fast approaching when standardization of overhead lines for pressures up to 11 000 volts might be carefully considered.

(4) *Reduction of unduly high factors of safety of cables and overhead lines.*—Largely due to the efforts of two of our past chairmen, some progress has been made in reducing the standard thickness of dielectric on high-pressure cables, and further reduction is probably permissible as methods of manufacture improve. While progress must necessarily be somewhat slow, so as to be sure, it seems desirable to arrange for existing standards to be automatically reviewed from time to time, say every five years. The existing factors of safety of overhead lines are not only unduly high in many cases but are also inconsistent, and the Report of the Committee of the Institution which is considering this matter is awaited with great interest. It is to be hoped

* *Journal I.E.E.*, 1916, vol. 54, p. 125.

that they will give due weight to modern improvements in protective gear, as a much lower factor of safety is permissible if the immediate disconnection of a broken conductor can be assured.

(5) *Maximum use of the carrying capacity of the mains.*—We all know the effect of a " bottle-neck " in reducing the capacity of a railway or tramway. Similar cases occur on our mains, and extra heating at localized places, such as substation leading-in ducts, grouped feeders near a power station, short lengths of cable in series with overhead lines, etc., may seriously limit the carrying capacity of long and valuable lengths of main. If this point is kept carefully in mind when designing the system, it is not a difficult matter to use short lengths of increased section at such special places so that the main as a whole may be loaded to its full capacity.

Due to the initiative of this Centre, the Institution some time ago set up a Research Committee to consider the carrying capacity of the armoured three-phase cables which are now generally used for high-pressure work. This Committee, and a similar Committee

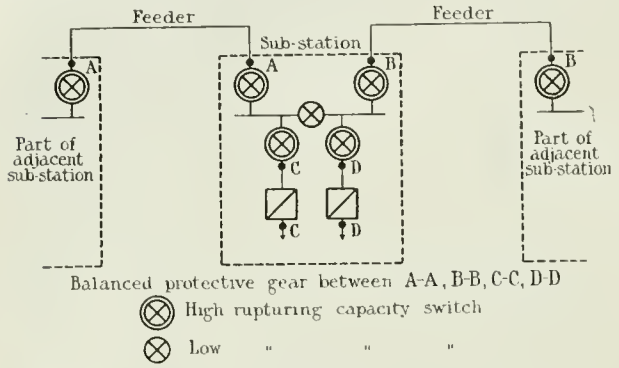


Fig. 4.—Usual lay-out of switchgear for duplicate-supply substations.

dealing with unarmoured cables, have since been absorbed in the newly formed British Electrical and Allied Industries Research Association. In spite of the difficulties due to the war and to the reorganization of the control of research, a considerable amount of useful work has been done at the N.P.L., at Liverpool, and in Newcastle. An account of this will be published and discussed at an Institution meeting during the present session.

(6) *Reduction in the amount of switchgear.*—It is surprising to what an extent unnecessary switchgear is often installed. To take a single instance, it is quite usual, when a non-duplicate supply is given, to find a switch on both the high- and low-pressure sides of the transformer. Obviously, a switch on the high-pressure side is sufficient in itself to protect the transformer and to enable the whole apparatus in the substation to be made dead when required, so that there can be no objection to coupling the low-pressure side of the transformer direct to the busbars of the distributing switchgear. In designing a substation it is well worth while to take each switch in turn and consider carefully whether it performs any useful function.

When we come to duplicate-supply substations for important consumers such as large collieries, the cost of switchgear is a very serious item. Fig. 4 shows the usual arrangement comprising two incoming feeder panels, two transformer panels and a non-automatic busbar sectioning switch to enable supply to be maintained in the event of trouble on the switchgear, and to facilitate maintenance work. This involves four switches of high rupturing capacity, and one switch of lighter construction. The arrangement permits of either feeder being out of commission simultaneously with either transformer, but the need for this is very remote and I would suggest that the simplified arrangement shown in Fig. 5 is ample for most requirements. This reduces the number of switches from five to three, and it lends itself to an arrangement of balanced protective gear which includes the whole of the switchgear, whereas with the former arrangement the switches and busbars cannot be protected without considerable complication. As a result of the protective arrangements the new scheme permits the use of lighter and cheaper switches for controlling the transformers,

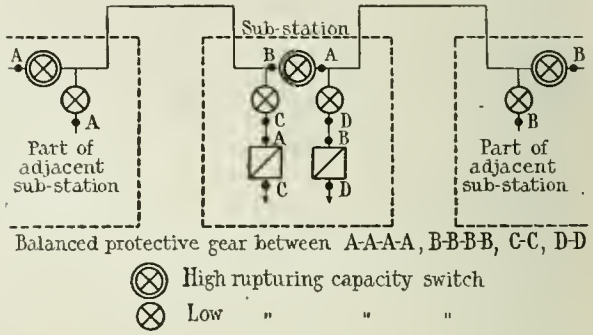


Fig. 5.—Simplified lay-out of switchgear for duplicate-supply substations.

since if, due to a specially severe fault, they should fail to clear, the fault will be no more serious from the system point of view than the failure of a cable, and will be cleared by the high rupturing capacity switches on either side. Due to the reduction both in number of switches and in the average rupturing capacity, the overall cost of the switchgear should be reduced to practically half.

(7) *Saving in buildings.*—While it is very convenient to have all apparatus under cover, the present cost of building is so high that we should seriously consider the adoption, in whole or in part, of the American practice of outdoor substations with the necessary modifications to suit our climate and special conditions. It is doubtful whether outdoor high-pressure switchgear is suited to our climate, but in any case American experience shows that it is not economical at the pressures we use in this country. The transformer can, however, be readily adapted for outside use at small expense, and by the use of lead-covered cable connections it can be made safe enough to satisfy our somewhat high standard. Substations of this type are being installed in this district.

(8) *Detail distribution.*—Table 2 shows a rough

analysis of the incidence of the capital costs which would be incurred at the present time in giving domestic alternating-current supplies to a typical garden village. Three cases have been considered corresponding to various degrees of domestic electrification.

The high capital cost of the detail distribution in cases (1) and (2) will repay very careful study, as it is obvious that its reduction would be one of the most important aids to a rapid extension of domestic supply. If one turns for guidance to American practice it will be found that conditions are much more standardized than they are in this country. For the dense down-town areas in the big cities direct-current three-wire distribution with 230 volts across the outers is used. This involves greater capital expenditure than any of our networks, but I believe it was economically justified by the fact that in the early days the supply undertakings maintained the lamps, and the savings in lamp costs and renewals at 115 volts more than balanced the additional network costs. As a result of this low lamp voltage they were practically forced into using a high-pressure network for general distribution in the smaller towns and on the outskirts of the big cities, with small closely spaced distribution transformers feeding small groups of consumers. It is almost universal to use 4 000-volt three-phase four-wire overhead construction with 2 300/230-volt pole-type transformers connected from one phase to neutral on the primary side and with a centre tapping for 115 volts three-wire on the secondary. For lighting, single transformers are used, and for power three transformers, connected in star on the primaries and in mesh on the secondaries. Occasionally the higher secondary pressure of 460 volts is used for power. The high- and low-pressure wires are run on the same poles and the high-pressure circuits are worked on while alive. It appears that up to 4 000 volts ordinary simple precautions against shock, such as rubber gloves, are satisfactory, but at higher pressures these give trouble. In this way a scheme of general distribution has been semi-consciously evolved, which is more flexible and cheaper than the British 400/500 single-pressure underground systems, although it appears to us somewhat unsightly and dangerous. From statistics which I have seen, one fatal accident and five shocks to employees and the public combined occur annually per 1 000 miles of high-pressure mains. These figures are much lower than might be expected and, if compared with similar figures for tramways, motor cars and omnibuses, etc., are relatively negligible; but public opinion in this country demands a specially high standard from the electrical industry, and it is difficult to justify risks to human life even if they are very small. At the same time one cannot help contrasting the requirements made upon us with similar requirements in, say, railway work where, for instance, the abolition of level crossings would undoubtedly save many lives.

It is only of recent years that alternating-current

TABLE 2.

Nature of Load	(1) Lighting only	(2) Lighting with occasional heating and cooking	(3) Lighting, cooking and all heating, with the exception of a coal fire in the living room, which also provides the hot water
Type of Network	Overhead	Overhead	Underground
Apparatus	Capital Cost per kW of Consumers' Maximum Demand, assuming a Diversity Factor of 30 per cent for all Classes of Demand		
	£	£	£
Power station	5·3	5·3	5·3
High-pressure Distribution and Transformation *	4·8	4·8	4·8
Low-pressure Network	24·4	4·4	1·4
Service and Meter	20·8	3·0	1·0
Total	£55·3	£17·5	£12·5
Assumed Connected Load of Average Consumer — Lighting kW	0·4	0·4	0·4
Assumed Connected Load of Average Consumer — Heating and Cooking kW	—	1·7	9·6
Assumed Demand of Average Consumer	0·25	1·7	8·3

* Direct current would add £1·5 per kilowatt, plus attendance charges and additional losses.

distribution has come into its own again, and direct current, during its vigorous innings, obtained practically a monopoly of the denser and, therefore, earlier developed areas. Consequently the problem of dealing with the further growth of such areas without the addition of expensive converting plant is a very live one. Sometimes it is possible to make a complete change-over to alternating current, but if the motor load is too great for this to be practicable, a compromise is to change over several large blocks of lighting, heating and cooking load to transformers fed at high pressure. In this way the whole of the motors can be left on direct current, and the substation plant and networks can be relieved sufficiently to enable the smaller new consumers to be taken on to the existing mains.

I have read with much interest Mr. Wordingham's plea for lower voltage alternating-current domestic supplies, but unless one tends towards the American methods one is rather afraid of producing a further increase in the already high capital cost of detail distribution. I find, however, that if 440-volt cables were operated at 650 volts, three-core cables used in place of four-core, and small transformers used for each consumer, the net result would be about the same. It is a matter for careful consideration whether jointers could safely work on live cables at 650 volts and whether the added risks to employees would be more than balanced by the reduced risk to consumers.

Another direction in which improved methods are urgently required is in tapping small demands from high-pressure networks. At one time it was considered a huge joke if a farmer raised such a question as tapping a 20 000-volt main which passed his door. This attitude was really a confession of weakness, and much of American success is due to the boldness with which they tackle this problem. I am hopeful of considerable progress in this respect in the future, and this is essential if we are to meet the anticipations which have been raised of a universally available supply of electricity.

(3) *Tariffs.*—There are two questions which seem to me to urgently require a settlement. The first applies to industrial tariffs and the second to domestic tariffs.

If the economies which may result from power-factor correction are to be realized, the industrial consumer must be penalized for bad power factor, and the most direct way to do this is to bring the question of power factor into his tariff. This involves special metering, which is difficult to explain and justify to a non-technical consumer unless it is a recognized standard. By co-operation it should be possible to agree upon such a standard industrial tariff embodying power factor.

On the domestic side a standard single-meter tariff would undoubtedly do much to promote the general use of electricity for cooking, heating and power and to simplify the use of an alternative to the flat rate for maximum rates under Provisional Orders. The desirability of the general adoption of a domestic tariff based on a fixed charge plus a low unit rate is obvious from the figures I have given in Table 2.

In default of a single organization corresponding to the National Electric Light Association in the U.S.A., I would suggest that the Institution might usefully consider calling a conference of the Associations representing the various groups of electricity supply authorities, with a view to an agreed standardization on these two tariff questions.

(4) *Provision of capital.*—Like all other industries, that of electricity supply will develop most rapidly with cheap capital, and this is directly dependent on the degree of stability of the investment. If electricity supply is examined from this point of view it will be found that it ministers chiefly to the real necessities of the community, and not to its luxuries, while so far from its suffering from any risk of retrogression all the basic indications point in the other direction.

Given a stable industry, and one whose development is essential to the country's well-being, what can be done to assist it ? In the first place, the public should realize the necessity for a flow of capital to it. In reconstructing a bankrupt firm curtailment of expenses is not more important than wise new expenditure on essentials. While Great Britain is not bankrupt, it is paying no dividend at present, and economy must be exercised in every possible direction, but undoubtedly electricity supply is a case for expenditure on essentials. To induce such a flow of capital a satisfactory return, proportioned to modern conditions, must be assured. If it takes $5\frac{1}{2}$ per cent instead of $3\frac{1}{3}$ per cent to run the British Empire, is it not illogical to refuse permission to raise maximum prices until hardship has been proved based on pre-war rates of interest, and what chance is there of raising fresh capital under such conditions ? There seems to me no more short-sighted view than that the community really benefits by certain sections obtaining electricity at less than its economic price. From the broad standpoint, surely the community benefits most when the undertakings supplying its needs are healthy and sufficiently prosperous to be encouraged to adopt a progressive policy of development. At this time of continuously varying values the niggardly and piecemeal revisions of maximum prices should be abolished and substituted by a universal sliding scale based on some convenient official index figure.

The manufacturers can do much to help the supply industry, and incidentally themselves, by getting back as quickly as possible to fixed prices. In the United States the electrical firms have never departed from fixed prices and, while our manufacturers have the excuse that our price fluctuations have been more severe, many of them have certainly been inclined to use their monopolistic position during the war to pass on the uttermost farthing to the purchaser.

(5) *Relations with employees.*—The last few years have seen a vast change in the electricity supply industry. From being one of the least organized industries it is now one of the most highly organized. It possesses one of the most developed and successful of the National Joint Industrial Councils for dealing with its manual workers ; and also a separate parallel organization, the National Joint Board of Employers and Staffs, for dealing with the technical staffs. A most interesting address could be given on this question alone, but I propose to refer to two special points only. The first is the real necessity, in the interests of employers and employees alike, of preventing the use of

the industry's lack of storage capacity as a weapon in external disputes. The second is the accumulating evidence of the incapacity of some companies and of many municipal organizations (despite the splendid work of the I.M.E.A.) to realize their responsibilities as employers both to their staffs and to their fellow-employers. One result of this, which may have far-reaching effects, is that they have succeeded in undermining the natural bias of the directing staffs towards the employer's side and have forced these staffs into sympathetic understanding of many of the difficulties of the manual workers' unions.

(6) *Co-operation between undertakings.*—Since it is economically desirable to have super-power stations and large supply districts, co-operation is the first and most vital necessity of future progress.

The primary aim of the Electricity (Supply) Act is the co-operative formation of Joint Electricity Authorities, guided and assisted by the expert knowledge of the Commissioners.

Difficult as co-operation in itself may be, the correct form of co-operation is more so, particularly as the framers of the Act endeavoured to smooth its path by giving, to all and sundry, representation on the Joint Electricity Authorities, with the result that they will tend to become unwieldy and, in consequence, lacking in enterprise and initiative. Suggestions have been made that this might be avoided by the Authorities acting mainly as local supervisory bodies on behalf of the Commissioners, and delegating their executive powers to private companies who would be in a position to finance and operate such speculative undertakings as super-power stations and coal-distillation plant. If the Authorities decide to do the work themselves, it is very desirable that the large consumers should be fully represented and that there should not be a preponderance of non-technical representatives of local authorities. The main function of the Joint Electricity Authorities will be to generate as cheaply as possible and to sell in bulk to large industrial concerns and to the distributors, who in most cases will be the local authorities. Cheap generation affects large industrial consumers more directly than it does the domestic consumer, since we have seen that for the latter the capital cost of distribution is at least equally important. Consequently a strong representation of the large consumers will tend to the concentration of the Authority on its main functions, the local distributors being represented in proportion to their importance as bulk consumers. This would still give the larger municipalities and companies a considerable representation, while the smaller local authorities and companies would have ample scope in dealing with the detail distribution.

I understand that the National Electric Light Association has done a great deal to foster a co-operative spirit between undertakings in America, and in this country we know of the useful work which has been accomplished by the various sectional organizations, particularly the I.M.E.A. It is worthy of consideration whether, under the auspices of our Institution, a more representative body embracing all sides of the industry, municipal and company, could not be formed with the especial object of rendering assistance to the Commissioners in the working of the Act. Each section would have its own meetings as at present, but, in addition, periodical joint meetings would be held and a special annual meeting (possibly in conjunction with the B.E.A.M.A.) organized each year on the lines of the N.E.L.A. Convention, which would be the most representative gathering of the electrical industry.

Some such means of bringing rival groups into friendly contact would seem to be the most likely means of breaking down the non-technical barriers which impede our progress.

It is a truism that the electricity supply of London is a political, not an engineering, problem, and in this, as in many other directions, London is the mirror of the nation. The preliminary linking up of certain undertakings in London and in some of the industrial areas are only prototypes of what should be the universal practice of the whole country if British prestige is not to suffer severely abroad. All over the world, even in the United States, our study of the economics of concentrated generation and unified distribution networks and our plans for the post-war reorganization of our industry have attracted wide attention. France, Belgium, Holland, Australia and South Africa have all given the closest study to the subject and in many cases have made substantial progress, while America, true to its reputation, is considering a huge scheme for feeding the eastern states, including Boston, New York and Washington, from super-power stations at 220 000 volts.

The Englishman prides himself on showing a high percentage of deeds to words, but in electrical matters the percentage is at present very low, and it is only by the most strenuous and disinterested joint effort during the next few years that we shall be able to satisfy the expectations we have raised. If we are equal to the task and can raise this country to a high level of electrical development, we shall be amply repaid, both by its effect on the prosperity of the industrial life of the country and by the assistance which it will afford to our electrical manufacturing industry in obtaining a worthy share in the markets of the world.

NORTH-WESTERN CENTRE : CHAIRMAN'S ADDRESS

By Julius Frith, M.Sc., Member.

(*Address delivered at* Manchester, 9 *November*, 1920.)

It is usual for chairmen in their addresses to review the progress of some branch of the art to which we all belong, and I should like to crave your indulgence to-night whilst I review the present state of the art of manufacturing electrical energy.

To obtain a perfectly detached and unbiased view of the situation, let us suppose for a moment that the fabled visitors from Mars had expressed a desire to inquire into the methods in use of supplying power to our industries. They would naturally be brought to the Institution of Electrical Engineers as, beyond doubt, upon us is fallen the rôle of purveyors of power to our fellows, and we should blush with honest pride and proceed to conduct our visitors to the nearest super-power station.

But the Martians would naturally want to begin at the beginning and to inquire in what, so to speak, our raw material consists, i.e. what are the available sources of energy which we can tap for the end in view. This inquiry would perhaps put us a bit out, but with a sigh at their ignorance we should sit down and—think !

After a little of this unwonted exercise, we might come to them again with a more or less ordered summary of the situation in some such form as the following, in which we should attempt to put the thing stripped of preconceived notions and in simple phraseology.

Power or Energy.*

1. *The sun.*—(*a*) Using the direct heat of the sun concentrated by mirrors or lenses to work heat engines.

(*b*) Water power, i.e. using the sun as a vast circulating pump to lift water from the sea and put it on the tops of mountains, we making it run back over waterwheels or turbines.

2. *The moon.*—Using its attraction on the waters of the sea—" Harnessing the tides."

3. *Winds.*

4. *Electro-chemical effects.*—Using the difference of electrical potential taken up by two dissimilar conductors when placed in a fluid which chemically combines with one of them when electricity is allowed to flow under this difference of potential.

5. *Molecular activity.*—Or the energy which is given off when some of our larger and more complex molecules disintegrate or explode.

6. *Combustion.*—That is, combining things with the oxygen of our atmosphere to produce heat.

Taking these possibilities in the order in which they are given above, we should explain that the radiation which the earth receives from the sun, although not itself heat, is absorbed and converted to heat on striking against a more or less opaque body. Sufficient of this energy can be concentrated on, say, a boiler, and so made to work a steam engine and produce power. On being strictly questioned on the practicability of this device, we should have to admit that it was but feeble and not suitable to be used in our centres of industry.

On the second application of the sun's activity, we can better congratulate ourselves as, Nature and civil engineering helping, we can extract and put to useful work some 90 per cent of the potential energy of the water, that is, in places which are blessed with a climate ; in the British Isles, where there is only weather, water power is not so reliable.

On our visitors expressing a doubt as to the possibility of the moon being a source of energy, we are driven to admit that the problem is not so simple as it looks, and that the energy which is really tapped in harnessing the tides is due to the difference between the time of revolution of the earth and that of the moon round the earth, and that making use of the tides would tend towards keeping the moon always opposite one fixed point of the earth's surface.

Winds are at best uncertain, though they have been extensively used for grinding corn, pumping water, and driving small electric generators.

The electro-chemical effect is the most promising of those investigated so far : the conductor which suffers disintegration only does so in proportion to the electricity set in motion, there are no moving parts, and the efficiency is very high. It has to be confessed that the destroyed conductor has in the past been made of some very costly material, and that there is no ready market for the chemical combination produced ; but, after all, these are minor points which can no doubt be remedied when we have time to give to them.

No. 5, Molecular Activity, we have to confess is at the moment a little new and puzzling, but it is very promising. In energy given out per pound of material, it exceeds anything else by several millions per cent. It is, certainly, a little difficult to stop and start, and seems a trifle like using up the bricks of which the universe is built, but we shall no doubt be able to report further to a future meeting.

Last on the list comes Combustion. It is known that certain things, notably hydrogen and carbon, will combine with the oxygen of our atmosphere, and in so doing give off heat. But heat is, unfortunately, very low down in the social scale of energy, that is to say, we cannot convert the whole of the energy of heat to other and more useful forms without expending work ; we can put only part of the heat energy of combustion where we would wish it to be, and have to reject part as useless. That is to say, up to now we have laboured under this drawback, but there is nothing

* Never mind the confusion in dimensions for the present.

inherently impossible in Maxwell's supposition of demons who would let swiftly-moving particles through a door which was closed to slower ones.

For the moment, therefore, we make heat by combustion and are still at a loss for a means of converting this to mechanical energy. We need some connecting link between the fire and the generator shaft. Let us consider the theoretical requirements of this connecting medium. It must take up energy from the fire, and in so doing alter its physical state or condition; in which altered state it is introduced into the engine and reverts to its former state, parting with the energy it received from the fire.

Imagine a giant kept at work in the cellar compressing helical springs which are brought up and put into a machine. The catch released, they expand and do work and are dropped back into the cellar to be recompressed.

Now we find ourselves in the painful position of having to tell our visitors that we make a practice of rejecting the springs when they are only about one-fifth expanded, shooting them out into the atmosphere to complete uselessly the remaining four-fifths of their work there. A total energy of 1 019 British thermal units is, we explain, required to convert a pound of water at 212° F. into steam at 388° F. and a pressure of 200 lb. per square inch above atmosphere. If we expand this down to atmospheric pressure and 212° F. and allow it to escape, it will carry away at least 825 B.Th.U., and the best that the engine can possibly do is to use the difference, namely 194 B.Th.U. or, say, 19 per cent, for the production of mechanical energy. We might at some trouble and expense provide a place of much lower pressure than the atmosphere for the engine to exhaust into, say at 1 lb. per square inch pressure and 102° F. The one pound of water stuff would then take 1 129 B.Th.U. from the fire and depart from the engine with at least 788, leaving with the engine a maximum of 341 or 30 per cent. As a matter of fact the actual efficiency of the engine is, of course, much less than these figures. These large amounts of energy received from the fire and rejected by the engine are usually not only wasted but become a source of annoyance to our neighbours. We are accustomed to refer with pride to this as "the age of steam," but the above figures do not seem to justify much pride. Steam, like Jacob, has usurped the place of the first-born not by historic right, nor by any particular aptitude for the rôle, but mainly through the influence of its godfathers Watt and Stephenson. Several other fluids have, from time to time, been proposed, such as carbon bi-sulphide or ammonia, but up to now engineers have turned up their noses at all such and, truth to tell, their noses would be the chief sufferers!

There are, however, some other promising avenues of attack on the problem. One is to make the condenser itself a source of heat for a second thermal-cycle engine, and this idea has not received the attention it deserves from practical engineers, though it has been carried some little way on the theoretical and experimental side and has shown some promise.

Another line to follow would be not to condense the exhaust at all but to pump it straight back into the boiler or heater, there to receive another supply of energy; but this is, apparently, contrary to the laws of thermo-dynamics as we at present understand them. For, if we come to think of it, the steam is condensed to water and the 1 000 B.Th.U. per pound wasted and sent to make things hideous on the cooling towers, not from sheer vice but mainly because, as water, it occupies less bulk than as steam and is, therefore, more easily pushed back into the boiler to be re-vivified. If we could only find some way of putting the exhaust steam back into the boiler as it is released from the engine, instead of making it return to water to be again re-evaporated by our precious fuel just the other side of the boiler plates, the possible efficiency of a steam engine might be almost indefinitely increased.

Thermo-dynamics as expounded by Carnot expresses the efficiency of this conversion of heat to mechanical energy in terms of temperature, and lays down that the maximum possible efficiency is dependent on the temperature range employed in the engine, i.e. is equal to the drop in the temperature of the working fluid whilst passing through the engine, divided by the absolute temperature on entering. The temperature on leaving is dependent on what methods we have available for cooling, for which water, either from a river or cooling tower, generally has to be used, producing a lower temperature of about 100° F.

On the other hand the combustion of most of our fuels with the oxygen of the air will produce temperatures in the region of 2 000° F., thus giving us a possible efficiency of

$$\frac{2\,000 - 100}{460 + 2\,000} \text{ or } 77 \text{ per cent.}$$

Our present practice, however, is to employ our 2 000° F. in raising steam and superheating it to, say, 600° F., giving a possible efficiency of only

$$\frac{600 - 100}{460 + 600} \text{ or } 47 \text{ per cent.}$$

This shows up our wrong-headed way of stating boiler efficiency by expressing it as heat delivered over heat received, ignoring the temperature and, therefore, the *availability* of the heat. One might almost as well calculate the efficiency of a water turbine as water emitted over water supplied.

The gas turbine offers a possibility of using these high initial temperatures, but it would probably exhaust at a temperature comparable with that of an ordinary boiler fire. Further use of these hot exhaust gases could then be made for raising steam.

In a recent Government report it was estimated that on the average 5 lb. of coal are burnt for each horse-power-hour produced in industrial power plants. Taking this figure for what it is worth, I have endeavoured to draw up a corresponding balance sheet, showing receipts and expenditure, though it must be confessed this somewhat resembles Angelina's first attempt at domestic account-keeping—on the left-hand side it ran: "Received from Edwin, £5," while on the right appeared "Spent it all."

Received	B.Th.U.	Per cent
5 lb. of coal at, say, 13 000 B.Th.U. per lb.	65 000	100
Paid out		
Proportion used for banking boilers at nights and week-ends ..	6 000	9·3
Lost in ashes	1 000	1·5
Sent up the stack as hot gases— 100 lb. at 500° F. excess temperature	12 000	18·5
as steam in the flue gases ..	2 000	3·0
as carbon imperfectly burnt to CO	1 000	1·5
Radiation from boiler, engine and steam pipes	4 000	6
Mechanical friction of engine ..	250	0·4
Sent to cooling towers in circulating water (7 lb. of steam per lb. of coal, 1 000 B.Th.U. per lb. of steam)	35 000	54
Used in auxiliaries	1 200	1·8
As one brake horse-power-hour ..	2 550	4
	65 000	100·0

Four per cent !

Let us take these sources of waste seriatim and see what can be done to lessen or prevent them. The banking loss is obviously that due to bad load factor, and decreases as the latter increases ; the most fruitful way of bringing this about is to make use of the diversity factor of different classes of load and to supply as many of them as possible from the same power plant.

The loss due to hot flue gases up the stack can be reduced by reducing either their quantity or temperature. In the example it has been assumed that 20 lb. of flue gases are made per pound of coal burnt, which corresponds to about 11 per cent CO_2 ; it is difficult consistently to obtain better results than this whilst retaining the barbarous practice of burning solid fuel on the bars ; with liquid or gaseous fuel the quantity of flue gases can easily be reduced to two-thirds of this amount.

The temperature could and should be reduced by, first, steam superheaters and then feed-water heaters in the flue. This process is, however, limited in practice by the next item on the loss account, namely, the steam in the flue gases. This steam arises from the original moisture in the coal, from the water of combustion of the hydrogen content, and also from steam jets, which are sometimes used for carrying in the air for combustion.

The more steam is present, the more the process of extraction of heat from the flue gases is limited on account of the corrosive action of the water formed by condensation on the tubes of the feed-water heater. Surely it is time that manufacturers faced the problem of making a cheap form of economizer with, say, cast-iron tubes, which could be put on the tail end of the feed pump and could extract a good proportion of the 21·5 per cent carried away by the flue gases and steam together.

The radiation losses can in general be much reduced by more attention to the lagging and brickwork than often falls to their lot.

But the efforts of the reformer may well quail before the stupendous loss carried away by the condensing water and made hideous in the cooling towers.

Before leaving this attractive picture it will be well to put on record a balance sheet to compare with the 5 lb. per horse-power-hour, but embodying such of the refinements hinted at in the preceding paragraphs as have been at present achieved in the best steam practice.

Working at a good load factor with any large turbine plant, one brake horse-power-hour may be produced from one pound of coal, thus :—

Received : 1 lb. coal @ 13 000 B.Th.U. = 100 %
Expended :

		B.Th.U.	Per cent
In ashes and incomplete combustion		130	1
In hot gases up stack (14 % CO_2) 16 lb. at 400° F. excess	= 1 550		
Less taken up by feed-water heater	800		
		750	5·8
As steam in flue gases		300	2·3
As CO		300	2·3
Radiation		400	3·1
Friction in turbine bearings ..		120	1
In circulating water		7 800	60
In auxiliaries		650	5
In 1 b.h.p.-hour		2 550	19·5
		13 000	100·0

Very high temperatures and pressures may be used, and great increases of both these may be looked for in the future in the attempt to increase the ratio of available total heat in the working fluid.

Let us now turn to the internal combustion engine and gas producer and see how this combination will stand similar overhauling.

Average practice in moderately small non-recovery plants yields about one horse-power-hour for each pound of coal, and the balance sheet for such a power plant would look something like this :—

Received : 1 lb. of coal @ 13 000 B.Th.U. = 100 %
Expended :

	B.Th.U.	Per cent
Banking losses	250	2·0
Lost in ashes	800	6·0
,, radiation from producer and engine	700	5·5
,, engine friction	450	3·5
,, ,, cylinder cooling water	3 000	23·0
Used in auxiliaries, blowers, etc. ..	250	2·0
Carried away in engine exhaust ..	5 000	38·5
Given out as 1 b.h.p.-hour ..	2 550	19·5
	13 000	100·0

The largest of these losses is that carried away in the exhaust gases. A good deal of this can, however, be used for raising steam in a special form of fire-tube boiler, but here again we are limited in the amount of heat we can extract from these gases by the undesirability of condensing the steam they contain; this is another case for the cheap feed-water heater mentioned before. However, as it is, nearly two-thirds of the heat in the exhaust can be used for raising steam, which may amount to 2 lb. of steam per pound of coal into the producers, of which more anon.

The next serious loss is that in the water used for cooling the gas-engine cylinders. This is more difficult to negotiate, as low-temperature heat is rather a drug on the market. Probably, unless there is a demand for hot water for some other purpose, the best plan is to combine gas and steam plant. This has been carried out in some combined engines in America, using both the gas-engine cylinders and the exhaust to raise steam, which is used in another cylinder driving on to a common crank-shaft.

Another proposal, and one that goes much more to the root of the matter, is known as the "Still" engine which employs a common cylinder, one side of the piston being acted on by the gas and the other by the steam generated by the heat in the exhaust. Each side of the engine has to give away just what the other requires; the gas end of the cylinder and the piston receives its necessary cooling by the steam, and the steam end is warmed by the burning gases, and so condensation on the cylinder walls is obviated. I believe and expect that this "Still" engine will "get a move on" presently.

I cannot leave this part of the question without mentioning recovery of by-products, but I feel that this is too large a matter to be included in this address, and, moreover, I should be poaching on the special preserves of other well-known Manchester engineers. The battle at present largely turns on whether the by-product recovery producer can or cannot be made self-supporting in the matter of the steam it uses. We have shown that the auxiliary boiler on the exhaust of the gas engine can provide the necessary steam for this purpose, but the producer reformers aver that the reformed producers will have none of this and will make their own steam and more. If this proves to be the case, it becomes frequently a question between the relative values of coal and by-products whether it does not pay to gasify the coal used for steam power plants; this will probably be done, however, only by the people whose main business is the supply of power, as the tinker, tailor, and candle-stick maker will not often want to combine manufacturing chemistry and salesmanship of fertilizers with his other activities.

It only waits, however, for the perfecting of the gas turbine to make the gasifying of coal and the recovery of by-products a foregone conclusion.

And then, of course, at the back of all this is the immense possibility of gasifying what would otherwise be waste fuel. Mr. Drummond Paton will no doubt tell us, with his accustomed eloquence, how there is being left in the pits enough potential energy not only to supply all the industrial power required in Great Britain, but also endless supplies of fertilizers, disinfectants, and fuel oil, as well as smokeless fuel for our domestic hearths.

There is another possible way of utilizing the heat of combustion and converting it into electrical energy, namely, by means of the thermo-pile. If one junction of two dissimilar metals is heated and the other junction cooled, the two free ends take up a difference of potential. If electricity is allowed to flow under this potential difference the hot junction is thereby cooled and the cool junction heated. Heat is also conducted along both metals from the hot to the cool ends and is made all along both metals by the passage of the electric current. If the heat appearing from these three causes at the cool ends is wasted, the thermo-pile as a means of converting heat to electricity is necessarily inefficient; but it need not be wasted. It was pointed out above that there is an unnecessary loss of temperature between the fire and the water in a boiler, say 2 000° F. to 400° F. This temperature difference could be used in a battery of thermo-piles having their hot junctions in the fire and their cool junctions in the water. The heat which would otherwise be wasted is then used in raising steam, and the heat that disappears in the Peltier effect is converted into electrical energy at 100 per cent efficiency. For every 1 per cent of the total heat generated which is thus converted, the yield per pound of coal burnt is increased 4 per cent in the best, and 25 per cent in the average case, the heat balance sheets for which are given above.

To every thoughtful mind who has devoted much attention to this question of power supply, there must have been forced the conviction that, when all our national resources are used as they ought to be used, ample power would result as a by-product, when with a stroke of the pen all fuel charges would be struck out of the bill for power. It would then remain for engineers to reduce the capital charges, which can be done slowly by reducing the cost of plant, but much more quickly by increasing the load factor and diminishing the transmission losses of our big power stations. Manchester has given a lead in this direction by the cultivation of large power consumers situated near to the station and willing to accommodate their demands to the exigencies of the other loads.

There is a possibility that the use of steam might be justified if the latent heat in it could be extracted and used for some useful purpose such as process work in the factory or a commercial supply of hot water for domestic washing and heating. The latter most attractive possibility is well known and some amount of work has been put into it,* but all the literature on the subject which I have been able to find has been too partial and has stopped short of actually proving or disproving the commercial possibility of the scheme. It may be perfectly true—

(1) That what the industrial household is most in need of is a supply of hot water for washing the person, the clothes, and the culinary apparatus; and

(2) That the power station without much interfering

* Mr. Pearce has had examples of this at work in Manchester for years. See also W. M. Selvey on "A System of Combined Electric Power and Commercial Heating Generating Stations" (Sheffield, 1918).

with its function as a power station could supply hot water. But it does not in the least follow—

(3) That the ratio of the demand for power to the demand for hot water is of the same order of magnitude as the ratio of their production in a joint station ; or

(4) That even if 1, 2, and 3, are all true the supply could be given at a price which the industrial householder could and would pay.

I propose, therefore, taking for granted items (1) and (2) to prove the truth of (3) and (4). For this purpose I have thought it best to take an actual example, giving my assumptions and figures so that any alteration in the conditions can easily be allowed for.

Take an industrial community of, say, 10 000 inhabitants grouped into 1 670 households of 6 members each, 3 of whom are working. Their houses, arranged on garden-city lines, 8 or 9 to the acre, and allowing for wide roads and ample open spaces, would occupy about three-quarters of a square mile. The demands for electrical energy per household would be about 6 400 units per annum for those employed in the factories (1 h.p. per individual), 500 units for lighting and cooking in their homes, and 100 units for carrying the household about in trams and for public lighting ; total, 7 000 units per annum per household. As there are 1 670 households the town would require a power station turning out about 12 million units per annum, which at 25 per cent load factor would have a maximum load of a little over 5 000 kW.

Suppose the station to work with a vacuum of about 20 inches, it would take about 20 lb. of steam per unit. These 20 lb. of steam could, on condensing, yield 20 gallons of hot water at about 160° F., so that we have an average of 400 gallons of hot water per household per day. Allowing 50 gallons per day for baths and washing, 350 gallons are left for warming the house. If we allow the water to cool from 160° to 100° F., about 200 000 B.Th.U. are liberated per 24 hours, which is sufficient to keep a 5-room house comfortably warm. (For those who are more familiar with electrical units this corresponds in heating effect to about $2\frac{1}{2}$ kW for 24 hours.)

In a town of this size there are about 15 miles of roads. To carry the hot water from the central station four 5-inch mains will suffice ; from these would radiate 2-inch mains ; and from these a $\frac{3}{4}$-inch service would supply each house.

Flow and return pipes would be provided, the flow pipes being lightly lagged.

The loss from these pipes would amount to less than 15 per cent of the heat delivered to the houses if the lagging of the flow pipes allowed half of one B.Th.U. to escape per square foot per hour per degree Fahrenheit, which would not be considered a very good steam-pipe covering.

The total cost of the pipes, including lagging, excavating, laying and filling in would be about £45 000. The extra plant required at the central station might add another £5 000. 6 per cent interest and $2\frac{1}{2}$ per cent depreciation (21 years' life) = £4 250 per annum = 1s. per week per house, which surely would gladly be paid for this service.

Due to working at a lower vacuum the coal burnt in the central station would be increased from, say, 14 000 to 15 000 tons per annum. On the other hand, the service of hot water supply to the 1 670 houses would save each householder at least 6 tons of coal a year, so that the community would, by adopting the hot water scheme, burn 1 000 tons more coal in its central station and 10 000 tons less in its domestic grates, a net saving of 9 000 tons per annum to the community, with a corresponding reduction in smoke and all its attendant evils.

SCOTTISH CENTRE: CHAIRMAN'S ADDRESS

By J. E. Sayers, Member.

(*Address delivered at* Glasgow, 9 *November*, 1920.)

CURRENT VIEWS ON ELECTRICITY GENERATION AND ITS RELATION TO COAL ECONOMY.

There is probably no material question of greater importance to the present and future well-being of this country than—How can we make the best use of our coal? It is my purpose to review briefly and in an elementary manner some current views on this question, with special reference to its relation to the generation of electricity.

It is an embarrassing fact that for the generation of electricity we are still almost entirely dependent upon steam as a medium for the conversion of heat into motion —steam being a heat-carrier which, used in the orthodox manner, takes a toll of at least 60 per cent of the heat consigned to it. This may be affirmed without belittling the experimental results obtained by means of that very expensive, and somewhat dangerous, partial substitute, mercury vapour.

The present average thermal efficiency of the steam-operated generating stations of the United Kingdom may be taken to be round about 8 per cent. That is to say, for each quantum of fuel having a heat potential of 100 B.Th.U. fed into the boiler furnace, the electrical equivalent of 8 B.Th.U. is delivered to the station busbars.

In a few modern stations this percentage is certainly considerably higher, i.e. 13 to 15 per cent, and it is reasonably probable that in the case of a super-station having large and up-to-date prime movers and generators, and being equipped with all the known refinements making for economy, the average thermal efficiency may be in the region of 20 per cent. But even such a result shows an absolute loss of 80 per cent of the potential heat of the fuel consumed.

A few years ago, when the question of economy in electricity generation was becoming insistent, much was made of the advantages which would accrue from erecting the generating station on the coal-field. This phase of popular opinion quickly gave place to that which insisted upon the presence of suitable and sufficient water as the first consideration governing location, on the very reasonable ground that the freighting of fuel was practicable, whereas the transport of huge quantities of water for condensing purposes was not. If our future generating station is to be of the steam-operated condensing type, the considerations deciding location are, in the order of importance, proximity to suitable and sufficient feed and condensing water, proximity to suitable fuel, and proximity to the area of supply.

What are the present views on the question of electricity generation and coal economy?

Firstly:—We have the imperfectly informed popular view. It is, briefly, that the heating, lighting and power services of the country are to be provided, at trifling rates, by a small number of imposing electricity super-stations, which will make use of coal in the most approved manner, and in some way or another liquidate the National Debt out of their by-products. Unfortunately, there is no foundation in the hard facts of the case for this very optimistic view, and it calls for no criticism here.

Secondly:—We have the official view roughly indicated by the recommendations by the Coal Conservation Sub-Committee of the Ministry of Reconstruction on Electric Power Supply in Great Britain. These recommendations are:—

To divide Great Britain into 16 districts of supply.

To put down central stations, at suitable points, in which the by-products of coal could be extracted and the resultant coke and gas could be used for the production of electricity.

To use waste heat and poor fuel, wherever it is available, locally, and turn the electrical energy generated into the power company's trunk mains.

To standardize the system frequency and voltage of each district.

To ensure by Parliamentary powers that these large power companies shall not be debarred from operating in districts served by local undertakings in an uneconomical manner.

This official view has been subjected to very strong criticism from two entirely different standpoints; that of the gas engineer, and that of the advocate of the general adoption, in electricity generating stations, of the back-pressure system of operating steam plant.

These two views follow as thirdly and fourthly.

Thirdly:—The view of the gas engineer. It is, in essence, that the carbonization of coal should be in the hands of the gas industry, and the thermal products, gas and coke, distributed by it to the community; gas for heating, lighting and motive power, and coke for steam raising in general and electricity generation in particular. It assumes that the field for gas for the combined service of heating, lighting and motive power is much greater than for electricity, and contains a hint that electricity should be regarded merely as a by-product of the gas-works. I think the view could hardly be more ably presented than by Sir Dugald Clerk in his Paper on "Distribution of Heat, Light and Motive Power by Gas and Electricity" read before the Royal Society of Arts in March, 1919.

Whatever may be one's opinion as to the conclusions

arrived at, few will deny the great value of this contribution to the subject, a value which may perhaps be measured by the volume of criticism it has evoked.

Sir Dugald Clerk's principal conclusions are:

(1) Taking the present average thermal efficiency of gas and electricity production as a base, that for electricity being taken as 8·5 per cent, and assuming the division of the products, i.e. heat, light and motive power, in the proportion 55 per cent, 35 per cent and 10 per cent respectively, the heat used for these services for equal results would be:

	By Gas		Electricity Inefficiency Factor		By Electricity
Heat	55	×	4·00	=	220·0
Light	35	×	0·84	=	29·4
Motive power	10	×	1·60	=	16·0
	100				265·4

Further, if the results aimed at by the projected super-stations are realized, the thermal efficiency of the super-stations being assumed as 19·6 per cent, the comparison becomes:

	By Gas		Electricity Inefficiency Factor		By Electricity
Heat	55	×	2·80	=	154·0
Light	35	×	0·77	=	26·9
Power	10	×	1·10	=	11·0
	100				191·9

Therefore, the ratio 191·9 to 100 represents the advantages of gas over electricity, on a thermal basis, for the three services taken as a whole.

Taking the three services separately the remaining conclusions are:

(2) Electricity cannot compete with gas for heating.

(3) Gas and electricity are, and will be, in keen competition for lighting, and

(4) There are ample fields for the use of both gas and electricity for power purposes.

As regards conclusion (1), it will be observed that it is based on the assumption that the output of the projected super-stations would be divided between the three services in the same proportion as the present output of the gas-producing works. This point I will refer to later, and I will now proceed to review conclusions (2), (3) and (4) relative to the distinct services.

Conclusion (2):—*Electricity cannot compete with gas for heating.* Few will deny that, for heat for commercial use, for cooking, for heating water in quantities, and for any service which requires a considerable amount of heat in the home, gas has a distinct advantage over electricity from the thermal standpoint. On the other hand, gas cannot, for obvious reasons, compete with the small portable electrical apparatus of convenience such as kettles, hot-plates, irons, radiators, and the numerous similar portable heating accessories of a modern electrically-equipped house. For instance, few will contend that a fixed gas-fire renders a service equal, or even similar, to that of an electric radiator.

Conclusion (3):—*Gas and electricity are, and will be, in keen competition for lighting.* The figures show a considerable advantage for electricity; but the advantage is really greater than indicated, as the one-watt lamp has been taken as the basis, whereas a very large proportion of the lamps presently in use are the so-called "half-watt" type. These lamps are now being made of small candle-power, and will soon become general even for domestic use. But can the superiority of electricity to gas for lighting purposes be closely assessed on a thermal basis? Are the two products equally serviceable? The electric lamp provides light without the production of gases injurious to health, the furnishings of the home, and the contents of shops and warehouses. It does not require ignition, and its control approaches the ideal. It is applicable to any situation in an apartment or building and, by means of portable fittings and flexible conductors, may be moved about as desired. With illumination by gas the converse is true in every particular.

There is, I think, little doubt that, but for the vested interest in gas concerns, the initial expense of wiring and fitments, and the impossibility at present for many to obtain a supply of electricity, the gas used for illumination would be a negligible quantity.

Conclusion (4):—*There are ample fields for the use of both gas and electricity for power purposes.* The heat expenditure ratio for the same brake horse-power at the engine and motor pulleys is given as 10 to 11. But is the provision of a certain amount of power at the engine pulley a service equivalent to the provision of an equal amount of power at the motor pulley? Owing to its limitations of service the factory engine has to transmit its power through shafting and belts to the individual drives, whereas, with a properly designed electrical lay-out, motors are directly connected to each drive, or to any group of drives which may be economically operated together.

It is not unusual to find less than half the brake horse-power of a factory or workshop engine accounted for at the machines and tools operated. The worst examples are, of course, those in which the driven machines and tools are intermittently in use.

Those of us who have endeavoured to ascertain the power required by a particular machine or group of machines, by comparing the power developed by a factory engine with the particular machine or group of machines, working and standing, are able to appreciate how large are the losses due to the friction of shafting and belts, and to intermittent working.

It would appear, therefore, that the gas engine must be debited with all such losses in order to make a comparison, even on a thermal basis, and this adjustment would turn the scale heavily in favour of electricity. But, even with this adjustment, the two services are by no means on a parity. The electric motor is applicable to every kind of drive; it may be started and stopped, and its speed may be controlled at will by the operator. It may be successfully applied to a textile spindle or to a 10 000 h.p. rolling-mill. Is there a gas motor having such convenient characteristics and adaptability? In view of these considerations Sir Dugald Clerk's concession "that the electric motor is suitable for small and medium powers" is void of meaning.

Further, the factory engine, whether driven by gas, steam or oil, requires housing and constant attendance. The electric motor is nearly always easily accommodated; no engine house is required; it usually takes up far less room than the shafting and belting it displaces; and it requires practically no attention.

It is therefore probably true that, while there is a wide and increasing field for electricity for power purposes, the field for gas in this service is limited and decreasing.

If these criticisms are well founded, conclusions (2), (3) and (4) might be amended to read:

(2) Electricity cannot, on a thermal basis, compete with gas for heating—where required in large quantities—in commercial, or even domestic, use.

(3) Gas cannot compete, on a thermal basis, with electricity for lighting.

(4) Gas cannot to any large extent compete, on a thermal basis, with electricity for power.

Over 60 million tons of coal are consumed annually by factories alone, i.e. more than seven times the quantity presently consumed by the whole of the electricity generating stations in the United Kingdom. A very large proportion of this coal will be consumed for a return of from 1 to 2 per cent of its potential heat. It may therefore be confidently expected that, if electricity is made available at reasonable rates, and service to probable consumers is facilitated, an enormous increase in the consumption of electricity for power, and also lighting, will accrue.

If these expectations are realized, it follows that the power delivered by the proposed super-stations for the three services would not be in the proportions assumed by Sir Dugald Clerk for the gas-making concerns, and therefore the thermal comparison given in conclusion (1) would also need modification.

Fourthly:—The view of the "back-pressure" advocate. The idea, in the main, is to generate electricity by non-condensing steam movers, and to utilize the exhaust steam, either as steam, as hot water, or both, to provide the domestic and public heating and hot water services. Presumably, cooking and the remaining services needing higher temperatures would be provided for by gas and electricity.

This proposal which, if practicable on the scale indicated, could save a large proportion of the 35 to 40 million tons of coal used annually for the domestic heating service, appears at first sight very attractive, and is vigorously presented by Mr. A. E. A. Edwards in his paper "Steam Engines and their Relation to the Fuel Economy Problem," read before the Birmingham Association of Mechanical Engineers in April, 1918.

Mr. Edwards proposes to have a large number of comparatively small steam-operated stations generating electricity by non-condensing engines, and to produce thereby, in addition to all the electricity required, low-pressure steam, or hot water near the boiling-point, for the heating services.

It is claimed that by this means over 40 per cent of the potential heat of the fuel consumed in the stations would be available as heat. Heat would therefore be the main product and electricity a mere by-product.

Mr. Edwards graphically illustrates the thermal efficiency of his combined station by an interesting heat diagram which starts with the heat value of boiler fuel as 100 per cent, shows 61 per cent rejected to the condenser, and 8 per cent electrical equivalent in useful work. A second path is shown for the 61 per cent of rejected heat which, less radiation and calorifier losses, is added to the original 8 per cent of useful work and shows 13 per cent for hot water service, 28 per cent for heating and 8 per cent for electricity. This deduction is not quite accurate as to the division of the products, although the inaccuracy does not materially affect the claim as to overall thermal efficiency. The fact is of course that, under condensing conditions and an electrical product equal to 8 per cent of the thermal, the heat (61 per cent) rejected to the condenser would appear in the form of condenser discharge at a temperature of less than 90° F., and would therefore be valueless for heating and that, under non-condensing conditions, a larger proportion of heat would be rejected by the steam mover, available either as exhaust steam or as hot water near boiling-point, both being serviceable products, with a considerably reduced electrical product.

"Back-pressure" and "pass-out" systems for combined power and heat production show a remarkable general efficiency, as well as a very high thermal efficiency, in cases where there is a fairly constant load and the demands for power and heat are approximately coincident. But a typical local electricity generating station is the very worst partner for such a combination, as an examination of its load diagram will show. The larger part of its output is supplied during a small part of the day, whereas domestic heat is required from early morning until late at night. It follows that such a station would, for the greater part of the time, really operate as a central heating plant, and steam for the heating services would have to be by-passed through reducing valves from the boilers.

From a purely thermal point of view the scheme shows to advantage, but it is questionable whether an up-to-date combined gas and electricity station providing for practically the whole of the heating services—including cooking and the like—would not run it very close, bearing in mind, of course, that heat would be the chief product and that heating by gas shows a thermal efficiency even higher than that claimed by Mr. Edwards for steam.

As regards practicability and overall efficiency, however, the general adoption of the idea appears to me to be hopeless. What would it mean?

As the practicable radius of distribution would be small, owing partly to the necessity of keeping down heat losses, and partly to the expansion and contraction difficulties with long lengths of piping, a very large number of small stations of comparatively low general efficiency would have to be provided.

All existing electricity generating stations would require additional boiler plant and, as the capacity of the generators would be reduced under the system, additional generating plant.

Perhaps the greatest difficulty of all would be the heat-distribution system. It would in the aggregate constitute an enormous undertaking, probably so formidable as regards cost as to be impracticable, and,

after all, the cooking and high-temperature heat services would have to be maintained in addition.

As already conceded, "back-pressure" and "pass-out" systems are remarkably efficient in every respect if the conditions are suitable, such conditions for instance as usually obtain in chemical works, breweries, dye works, some types of mills and sugar refineries.

There are combined heat and power installations which even to-day produce electricity at less than ¼d. per unit after being debited with all legitimate charges.

Many will also agree with Mr. Edwards that there is a strong case for non-interference with small colliery power houses where steam is raised with washery residue of very small marketable value, and the exhaust steam from the winding engines is used regeneratively for operating low- and mixed-pressure turbines. I am myself responsible for the design of equipments of this kind, and can testify to their economy and general excellence of service.

As there may be some here who are not well acquainted with the regenerative method of employing low-pressure steam, the simple diagram on the wall may be of interest. The particular equipment to which it refers is typical of the arrangement I have found suitable for colliery purposes, and is, so far as I know, in some respects novel. The whole of the exhaust steam from the winding and other surface engines is pooled in the heat accumulator. The feed-water heating is a first charge on the stored heat, and the remainder goes automatically to the mixed-pressure turbine. The feed water is taken from the heat accumulator. The water level in the heat accumulator is kept at a predetermined level by two automatic tanks provided with ball cocks, one of which admits make-up water from the condenser discharge, and the other—of inverted type—discharges water if it should rise above the predetermined level. As it is important that the water in the compartments where the ball cocks are fitted should not be disturbed by the periodic influx of exhaust steam from the winding engines, the tanks are fitted with baffles consisting of plates having a number of small holes near the bottom.

In order to make the diagram as simple as possible many details are omitted, such as the devices for removing oil from the steam, for adding softening agents in solution to the make-up feed water, and for deaerating this water. In some cases, of course, more elaborate arrangements are necessary for feed-water treatment than those indicated.

One important effect of the arrangement is to precipitate solid impurities of the make-up water in the accumulator shell, instead of in the boilers. It is not disadvantageous in the former as it acts as an internal lagging. The shell is so arranged that the precipitate may be easily removed when it becomes excessive.

In this particular installation there are two 500-kW engine sets and one 1 000-kW turbine set, and the engines are so valved that either can work condensing or non-condensing. In the latter case they exhaust to the heat accumulator and then really operate in series with the low-pressure stages of the turbine.

A common view of the colliery exhaust-steam installation is that, although steam-raising locally by washery residue may be justified on the grounds of low marketable value and unsuitability for ordinary steam-raising plant, yet it might be just as profitable to add condensing plant to the winding engine and produce electricity direct by high-pressure steam. Some of us have had to reply to such criticism, and Mr. Edwards in his paper correctly deals with it. The fact is that the average winding engine is under full steam for only about one-sixth of the period of the winding cycle. A condenser would have to be of sufficient capacity to deal with the large amount of steam during the short acceleration period and for the remaining five-sixths of the time——coasting and banking—would, as a condenser, be inoperative. The condenser would then be serviceable for only one-sixth of the period of the wind, although auxiliaries would be working continuously. It would, in fact, have to be at least five times the capacity of a condenser applied to a low-pressure turbine using the same steam through a heat accumulator capable of transforming the periodic peak quantities into a continuous flow. Thus the addition of condensing plant to an average steam winder cannot increase its efficiency, whereas a large amount of energy may be recovered from the exhaust by regenerative and equalizing steam plant.

I conclude that "back-pressure" and "pass-out" steam installation, and the use of very low-grade fuel locally for local service, should be encouraged, but that it is difficult to see how the general adoption of Mr. Edwards's proposed local combined heat and electricity stations could successfully compete with the centralized coal-carbonizing gas and electricity generating station, as a solution of the problem of efficient heat and electricity supply.

Fifthly and lastly:—This may be called the local view and is that the whole matter should still be left to local enterprise, on the grounds that the public knows what it wants, that it is in the interest of the providers of the services to satisfy these wants in the most efficient and economical manner, and that therefore, in the end, the best interests of the State as a whole will be served. It is certainly a fact, however, that the policy of local authorities is nearly always founded on purely local interest, and rightly so. The general welfare of the State is involved in the problems reviewed, and their solution is therefore the State's business.

I think it unlikely that any individual is, at the moment, possessed of the data necessary to enable him to dogmatize in detail as to the means which should be employed in order to ensure that the best use is made of our coal, but I have little doubt that, broadly speaking, the solution lies in its scientific reduction in up-to-date gas works and combined gas and electricity stations for the whole of the heating, lighting and power services of the country.

It seems to me especially desirable that:—

The production of gas and coke should be variable, that is to say, that it should be possible at all centres to gasify coke and, also, steam-raising plant should be designed to consume coke and gas indiscriminately. These provisions would obviate the risk of too much of the one and too little of the other being produced.

No restrictions should be put upon the use of gas for

power purposes, as one of the greatest factors in coal economy would be the advent of the gas turbine for power production.

The ideal is hard to attain, and indeed is not often practicable. May we not hope, however, that the labours of the Electricity Commission—a Commission which comprises some of the most capable men in both the electrical and gas engineering professions—will result in the best possible combination of what is both thermally and economically practicable?

NORTH MIDLAND CENTRE: CHAIRMAN'S ADDRESS

By C. J. Jewell, Associate Member.

(*Address delivered at* Leeds, 23 *November*, 1920.)

I desire to express my best thanks to the members of this Centre for the honour they have done me in electing me chairman for the coming session.

Electricity Supply.

During the past year events have moved quickly and it will always be remembered as the year in which the Electricity (Supply) Act 1919 came into operation, and in which the Electricity Commissioners were appointed.

The powers granted under the Act for the establishment of District Electricity Boards will, it is hoped, ensure that for the future all developments and extensions shall be considered as part of a general scheme for the supply of electricity at the cheapest possible rate, taking advantage of the best sites for power stations, irrespective of the conservatism of the local authority in whose area such power stations are situated.

When the needs of the country can be considered as a whole, instead of each district being allowed to develop or leave undeveloped any material advantages they may have, it is obvious that the electric supply industry will be placed on a much firmer footing, and will be able to extend its enterprise in a manner quite impossible under the limited area of supply which previously obtained.

The elimination of non-standard frequencies will make it possible for stations to be linked up profitably, and will save considerable capital expenditure in stand-by plant, at the same time ensuring that only the most economical plant is kept on load.

In their choice of Commissioners the Government have fortunately made certain that they are delegating their powers to gentlemen who have the confidence of the profession, and who, with their wide and varied experience in municipal and company work, are in a position to deal adequately with any scheme placed before them. I feel sure that the power entrusted to the Commissioners will be used by them in a fair and impartial manner to the advantage of the industry as a whole and to the benefit of the consumers. The appointment of such an outstanding personality as Sir John Snell is in itself a criterion that all schemes brought forward will receive full and sympathetic consideration, and I think we can congratulate ourselves on the local and national advantages which will accrue to the industry as a result of the Commissioners being practical men in the best sense of the term.

The saving of coal—one of our most vital national assets—is of extreme importance to-day, and every up-to-date generating engineer is assiduously engaged in keeping his "pounds of coal per unit generated" at the lowest figure his plant will allow. Considerable interest was awakened in the importance of this matter through the coal-consumption publicity campaign instituted during the war period, when generating stations were asked periodically to supply information regarding coal consumption and class of fuel used. By having attention persistently drawn to the need for economy in coal consumption, improvements which resulted in financial gain were made in many stations, and it is possible that more could be done in this direction. In the coal-field area, prior to the war, fuel of the "slack" type could be bought very cheaply, viz. just over 5s. per ton, and the need for incurring great expense to eliminate the cost of the last tenth of a pound per unit was not regarded as so important as it is to-day, when the cost of coal is from four to five times the price prevailing in 1914.

Whatever may be done in the generating station itself to reduce the consumption of coal, one cannot help deploring the fact that a very high percentage of dirt is present in small coal supplied to-day. In view of the high cost of transport and the difficulty and expense of disposing of ashes, it is a matter of regret that more and better coal-washing plants are not put down at the collieries to ensure that one gets what one pays for, viz. coal and not dirt.

In the near future the question of coal carbonization will no doubt receive great attention, with a view to obtaining the valuable by-products from the coal, and at the same time getting better value from the coal consumed. Up to now gas engines of a power that would be of commercial use for a large electric supply system are not a practical proposition, but given the invention of a reliable gas turbine the coal carboniza-

tion question might possibly be brought to a profitable solution.

From time to time suggestions are made regarding the use of oil as fuel, and although there is much to recommend oil on the score of:

(*a*) Ease of handling,
(*b*) Labour saving,
(*c*) Adaptability for storage,

the questions of high freights and fluctuating markets prevent oil being a serious competitor with coal in electric supply stations of the type and magnitude we shall have to consider in the near future. The use of the Diesel type of oil engine is limited to comparatively small plants, although such engines show considerable economy where the demand is within their range.

With the high coal prices at present prevailing—and there seems very little likelihood of much reduction—the question of obtaining current for power purposes from the supply mains is of increasing importance, and inquiries for supplies from manufacturers are now far more numerous than for a considerable time past; in fact, the use made of electricity in the manufacture of munitions of war has opened the eyes of manufacturers in many industries to its utility and adaptability, and to-day it is almost safe to say that new works will not put in their own power plant if it is possible to obtain a supply of electricity at prices which up-to-date supply authorities can offer.

This demand brings in its train the distribution question in scattered districts. The cost of running an overhead line for works which are situated any appreciable distance from the power station, is a serious item of capital expenditure which has to be taken into account when the cost per unit supplied is considered. Other things being equal, it will be to the advantage of the consumer to build his works or factory as close to the supply authority's station or mains route as possible.

For main transmission lines the demand for higher voltages than those previously looked upon as standard is being experienced, and there are several systems on which pressures of 30 000 volts are being used or will shortly be adopted. I believe that inquiries have been put forward for still higher pressures, and no doubt the cable makers will endeavour to meet the demand when it comes.

The question raised by Mr. Wordingham as to the supply pressure for distributors is a topic worthy of careful thought if the use of electricity in buildings for domestic purposes increases in proportion to the demand for lighting. The present low-tension distributors will be quite inadequate to carry the current demanded without excessive I^2R losses; and if, as Mr. Wordingham points out, the future of electricity supply lies with alternating current, it means that substations will have to be placed very close together, or in the case of large blocks of flats, hotels, and institutions, it will be necessary to go back to some modified form of house-to-house distribution to avoid the expense of running heavy low-tension mains.

A very instructive series of articles on "Power Factors," which has brought in its train some interesting correspondence, has appeared in the technical journals. This is a matter which is occupying the minds of many of our engineers to enable them to explain the number of "unaccounted for" units, which at times amount to a considerable item. If some equitable and easily understood arrangement could be made, whereby the offending consumer could be charged for running his motors at a low power factor, it would probably result in a more careful adjustment of his load to the size of his motor, and would be to the advantage of everybody concerned.

Traction.

One of the questions uppermost in the minds of electrical engineers is the electrification of railways. Up to the present time the railway companies have electrified only the suburban portion of the traffic. The success which they have obtained in that way has shown what the possibilities are when other portions of the system can be converted.

The ease with which great numbers of people can be moved from the large termini of the electrified lines is a revelation when one compares it with the steam days.

The majority of the electrification in this country has been carried out on the direct-current system with 600 volts on the third rail. The Lancashire and Yorkshire Railway, however, have a line running out of Manchester with 1 200 volts direct current on the third rail, which is working very successfully. The London, Brighton and South Coast Railway have a large portion of their London suburban lines equipped for high-tension single-phase current. The system has been running for many years and has given great satisfaction.

With a view to arriving at a decision as to the systems to be installed in the future, the Ministry of Transport has appointed an Electrification of Railways Advisory Committee, and this Committee, after considerable investigation, have issued an Interim Report in which they favour direct current, as against the alternating-current system, but they are of the opinion that the London, Brighton and South Coast railway system need not be changed.

The standard direct-current voltage they propose is 1 500, which will allow of substations being spaced a reasonable distance apart, and also of overhead conductors being used when the third rail is not convenient. No doubt, in the near future, schemes for further electrification of railways will be put forward, which will materially add to the comfort of the travelling public and also give an infinitely better service than is at present possible.

Under the provisions of the Electricity (Supply) Act the Commissioners will have power to arrange for the railway companies to take supply from the new stations where convenient and so avoid the trouble and expense at present experienced on traction stations in having to make provision for peak loads. They will be able to take advantage of the diversity factor, and so enable the stations to run with a better load

factor than is possible if they generate for traction only.

The tramway industry, which at one time was the means of popularizing the use of electricity in this country, and at the same time created a wide market for manufacturers of electrical machinery (something like £57 000 000 having been expended), has been very severely hit by the high prices and wages ruling to-day.

In pre-war times, with low fares, tramways were instrumental in taking the people from the town into the suburbs, and did a great amount of good by enabling people to live in more healthy surroundings. At that time many systems owned by large municipalities made considerable contributions to the rates, besides being able to keep their rolling stock and track in a high standard of efficiency. Regular employment was found for a large body of labour, and, in fact, tramways were looked upon as a thriving industry. I have taken out some figures from the published accounts of several large tramways and find the following:

	Receipts per car-mile	Costs per car-mile	Percentage of costs to revenue
	d.	d.	%
1908 ..	10·72	6·72	62·5
1911 ..	11·145	6·7	60·3
1917 ..	16·54	11·145	66·5
1919 ..	19·727	16·089	82·0

This of course does not take into account any capital charges, depreciation or reserve fund, the net result being that an industry which in pre-war times, even at the low fares charged in those days, served the public, paid its way, and allowed a return on the capital expended, is now fast drifting into a state of financial instability.

The limitations placed by statute on the raising of fares, in face of the advance in cost of all materials and wages, have placed the majority of tramway systems in a very unfortunate position. This applies especially to limited companies, who have not the alternative of having their deficits made good out of the rates; and a general education of the public was necessary to make them see it was as impossible to give a ride on a tramcar at less than it cost, as it was to expect a shopkeeper to sell an article at less than he gave for it.

A Bill has been passed to deal with this question, the Tramways (Temporary Increase of Charges) Act 1920, under which application can be made for permission to increase fares to a limited degree, and the majority of the undertakings have taken advantage of the provisions contained in the Act.

It was difficult to see how a sufficient service could be put on to take the traffic during the rush hours in anything like comfort, as it was only by allowing strap-hanging in the busy times that tramways could take sufficient fares to keep their average takings high enough to cover the cost of operation. It seemed only logical to expect that fares would go up in the near future in the same proportion as the prices of other commodities.

Tramway operators can, however, obtain a certain amount of consolation from the fact that, even at the enhanced prices prevailing, a tramway is the cheapest method of street passenger-carrying transport in existence. The motor omnibuses running on similar routes with competitive fares are costing more per car-mile, while their carrying capacity is smaller. One must also take into consideration the upward tendency in the price of petrol, which will tend to make the comparison still worse and in favour of the electric tramcar in its own sphere, i.e. where the traffic density is sufficiently great to warrant a service of four-minutes headway.

There is another item which bears very heavily on the tramway undertakings, and that is the cost of the maintenance of all the surface of the roadway between the tracks and 18 inches outside. Owing to a tramcar being rail-borne it is impossible for it to wear the road surface, and it seems very unfair that this heavy expenditure should be placed on the users of the lines.

Perhaps when the Ministry of Transport is apportioning the large amount of money which will be obtained from the new mechanically-propelled vehicle tax, it might be possible to make a grant to the tramway authorities with the view of relieving them of a portion of the expenditure incurred in road maintenance.

There is an indication of the revival of interest in the railless traction system, in view of the heavy expenditure which would have to be incurred in laying down the permanent way necessary for a street tramway. Estimates for a tramway track are in the neighbourhood of £25 000 per mile of single track, and, in view of the fact that most routes where the traffic is high enough to warrant this expenditure are already being operated by tramways, it is necessary to find some alternative method of equipping other less densely populated routes with an electric car service at a cost which will enable it to pay its way and at the same time give a cheap and reliable service.

Leeds and Bradford are the two pioneer cities which have taken up this question with a view to catering for routes that could not warrant the heavy capital expenditure on permanent way. Their experience tends to show that notwithstanding the fact that they are operating a very poor service, their costs are much lower than the average costs of rail cars on their respective systems. The Leeds and Bradford systems are further handicapped with a type of car which has a small carrying capacity. Improvements to overcome this defect are being made, and it is to be expected that in the near future this method of traction will be considerably extended. While on the question of electrically propelled vehicles, I should be very remiss if I did not remark on the future which I think lies before the battery vehicle.

Given work which comes within its range of mileage equivalent to its storage capacity, there is no cheaper method of transporting goods. In the transportation of materials where the periods spent in loading are large in proportion to the actual running time of the vehicle, the battery car is the most economic proposition,

In the collecting of refuse and the delivery of coal within a short radius of railway stations no other mechanically propelled vehicle can compare with the battery car in cost and utility, and in its upkeep it is cheaper than either petrol-driven or steam-driven wagons. The battery car is light on tyres. There is only one person required to drive it, as against two on the average steam wagon. The simplicity of the machinery, consisting as it does of motors, controller and batteries, saves considerably in maintenance, and with a price for current in the neighbourhood of 3d. per unit its running costs are less than those of a petrol lorry.

If more accommodation could be provided by electricity supply undertakings for the charging of battery vehicles, I am of opinion that a very remunerative night load could be built up to the benefit of the industry as a whole, it being obvious that the more occasions on which the utility of electric current can be brought before the public eye, the more popular it will become.

Electric Welding.

During the past 10 years electric welding has developed in such a remarkable degree that it is necessary for electrical engineers in general, and particularly those connected with public undertakings, to become acquainted with the current requirements of engineers, shipbuilders and boiler-makers who decide upon installing the new method in their works.

I propose to give a brief review of electric fusion welding as known and used between the years 1910 and 1920. Fusion welding I will characterize as the methods which use an electric arc as a source of heat for the autogenous union of ferrous metal. Broadly there are three processes of electric fusion welding:—

(1) The carbon arc,
(2) The metal arc, and
(3) The quasi arc.

The difference in practice between an arc and a quasi arc is undoubtedly the factor which has produced the developments mentioned, and which may well be described as revolutionary. The developments may best be visualized by comparing the work of repairing a ship's fractured stem or stern frame and the building throughout of a steel sea-going ship by electric welding without a single rivet being used in her construction —the former being the maximum achievement by electric arc welding up to 1910, whilst the latter accomplished fact was achieved by electric quasi-arc welding in 1920.

Before proceeding to give details of the current requirements for the new method it would be well to touch briefly upon the difference in what is described as an arc and a quasi or partial arc. The arc is produced between the metal rod—in some instances covered with a flux and in some instances used quite bare—and the work, by using a high current density and drawing an arc to obtain the heat required, which brings a small portion of the surface under repair to a welding heat. At the same time a drop of molten metal is added from the steel electrode, and sometimes the material is then hammered to close up the spaces; whilst the quasi arc is produced between a metal rod coated with an indestructible permanent slag, which in acting as a non-conductor allows the electrode to be used close up to the work, thus calling for a lower current density to bring about the requisite fusion of the metal. The covering when in a molten state produces an acid slag, which not only cleanses the metal of the weld from all oxide, but also acts as a secondary conductor of electricity, enabling the operator to maintain a continuous flow of molten metal into the weld. The weld thus produced requires no subsequent hammering to bring about a homogeneous junction, therefore the work is continuous and much more speedy in operation. It is interesting to note that, so far as is at present known, the only covering substance which produces these results is blue asbestos.

As a matter of history the names which stand out prominently as inventors of arc welding by the metal electrode as known and hitherto practiced are Slavinoff and Kjellburg—the quasi arc being the invention of Arthur Percy Strohmenger, a name well known in connection with electric storage developments in this country.

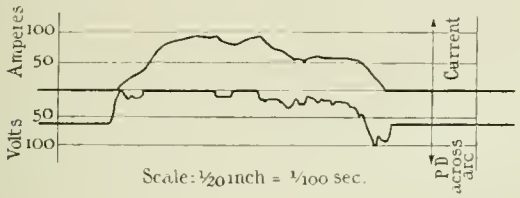
It may not be within my province to inquire why electric welding, after virtually standing still in the hands of only two or three welding companies for a matter of 10 years, should suddenly emerge and be practised by every Naval Dockyard and every important shipbuilder and engineer in the country, unless it can be traced to the improved metallurgical conditions brought about by using the quasi arc instead of the true arc. One certain reason for this would be the ease with which the knowledge of applying the process can be acquired. This was proved during the war by many hundreds of operators—both male and female —who were taught in the various works (notably Messrs. Ruston & Hornsby, Messrs. Clayton & Shuttleworth, Messrs. West's Gas Improvements Co., etc.) to operate the quasi arc in the electric welding of trench warfare bombs, Admiralty mines, etc. The severity of the tests imposed upon these articles bears evidence of the perfection of welding. Hydraulic pressure at 1 250 lb. per square inch was applied upon the cylindrical bombs, which were composed of $\frac{3}{8}$-inch steel plate 9 inches in diameter, with a longitudinal weld to form the barrel, and circumferential welds to attach the pressed ends. When it is taken into consideration that upwards of half a million of these trench warfare bombs, and practically the whole of the mines used in the North Sea, were welded by the electric quasi arc, it will enable one to estimate the enormous service rendered to this country by the use of this improved process.

Current consumption.—The perfect fusion of the blue asbestos slag-covered electrode calls for a pressure of at least 90 volts, and although the voltage between the electrode and the work if measured with an ordinary voltmeter will be found to average about 35 volts, it is nevertheless important to remember that the voltmeter gives no indication of the maximum and minimum voltage during an interval of time, say a fraction of a second. An oscillograph is the only instrument whereby

a curve connecting volts and time may be obtained or graphically seen, and an oscillograph record of the drop in volts across a quasi arc will indicate peaks rising to 90 volts, showing clearly that the higher voltage specified is in fact actually used although not measurable by ordinary instruments.

With the object of attaining the pressure required for these high peaks an exhaustive investigation was undertaken with a view to the discovery of some means whereby a lower terminal voltage might be adopted as a standard, under conditions which would nevertheless instantaneously provide the increased pressure as required from time to time in the course of making a weld. There has now been designed a special reactance coil which fulfils the necessary conditions when used on a direct-current supply of 60 volts having a straight-line voltage characteristic.

The accompanying diagram of an oscillograph record with this apparatus in circuit shows the function of this reactance.



It will be noticed that as the resistance of the quasi arc increases before the arc is actually broken, there is an available pressure of over 100 volts, although the pressure of the line is only 60 volts. When, therefore, this special reactance coil is used in series with each welding circuit, a pressure of 60 volts (direct current) will give results in every way equal to those obtained with 100–110 volts without this reactance, with a corresponding economy in electrical energy.

The sizes of electrode mostly used in engineering works range between No. 10 S.W.G. and No. 6 S.W.G., or roughly $\frac{1}{8}$ inch and 3/16 inch in diameter, and the usual current consumption on these and the intervening sizes would range from 60 to 120 amperes. Assuming an average consumption of 90 amperes at 60 volts, this would call for 5·4 kilowatts per welder where a number of welders are employed ; but in smaller installations where only one welder is employed 10 kilowatts should be provided, or for three welders 20 kilowatts.

On traction systems electric welding has proved invaluable in the re-treading of rails, welding of fish-plates, etc., besides building up various worn parts of car equipments which otherwise would be scrapped. In these days of high cost of renewals, arc welding has shown a saving of considerable magnitude.

Junior Members.

In conclusion I should like to address myself to the junior members of our Institution, and express a wish that they will attend the meetings and join in the discussions. By so doing they will tend to further the interests of the Institution and will, I believe, find the exchange of opinions and the knowledge gained of inestimable value in their future careers.

LIVERPOOL SUB-CENTRE: CHAIRMAN'S ADDRESS

By H. Dickinson, Member.

(*Address delivered* 1 *November*, 1920.)

(ABSTRACT.)

Electric Supply.

One of the effects of the war has been to accentuate the fact that an abundant supply of electricity is necessary to the community, and electric supply has now assumed a degree of importance in regard to the welfare of the country, which is greater possibly than at any time since the first generating station was put into operation. The Electricity (Supply) Act, 1919, provides for the formation of Joint Electricity Authorities in Electricity Districts to be prescribed by the Electricity Commissioners. The Joint Electricity Authority is representative of authorized undertakers within the Electricity District either with or without the addition of representatives of the council of any county situate wholly or partly within the Electricity District, local authorities, large consumers of electricity, and other interests within the Electricity District. There is an important provision of the Act that no scheme shall provide for the transfer to the Authority of any part of an undertaking except with the consent of the owners thereof.

The procedure fixed by the Act is for the Commissioners preliminarily to prescribe an Electricity District and, at a later date, to hold an inquiry to hear objections in regard to the suggested District and to consider any scheme submitted. If a scheme is approved an Order will be made by the Commissioners, which must be confirmed with or without modification by each House. When these formalities are complied with, the actual formation of a Joint Electricity Authority can take place, and a beginning can be made in obtaining a staff and an organization to carry on the work.

Under section 18 of the Electricity (Supply) Act, 1919, provision is made by which the Board of Trade (which powers under clause 39 are transferred to the Ministry of Transport) may, after consultation with county councils, local authorities and authorized undertakers within the Electricity District, construct generating stations, main transmission lines, or other works. This provision should materially facilitate progress if advantage is taken of it, for without such a provision nothing can be done towards the carrying out of any work until the Joint Authority is constituted and its staff and organization is in working order. It must be appreciated, however, that before any work can be carried out under this provision, the Commissioners must consult with the county councils, local authorities and authorized undertakers in the District, and in the event of there being any considerable opposition it is unlikely that the Ministry of Transport would undertake the work.

The Act provides that in the event of schemes submitted being unsatisfactory, the Commissioners may formulate a scheme of their own, but the Act seems to be lacking in the powers given to the Commissioners to ensure a scheme which they formulate being carried out. It is true that the Commissioners have certain powers of coercion owing to the fact that they may refuse their sanction to recalcitrant authorities for extensions of plant. In the event of this course having to be resorted to, it would be unsatisfactory, slow in its effects, and tend to cause discord between the Commissioners and the undertakers throughout the country, which would be most regrettable. It certainly looks as though further powers will have to be granted by Parliament before the ideas propounded in the Electric Power (Supply) Report can be carried fully into effect. It is in the principle of unity of control over a district that the chief feature of the scheme lies, and is the one by which alone the advantages sought for can be secured.

In many city or town areas, owing in most cases to the comparatively small scale of the generating systems, the highest economies that can be obtained under the new proposals by reason of the wider outlook and larger demands have been impossible.

The development of the steam turbine has had a marked effect on the selection of the site for a generating station, when we consider what an important effect vacuum has on the efficiency of the turbine. Sites for the new large generating stations must therefore be very carefully considered in regard to the capability for providing an adequate supply of water for condensing purposes. It is interesting to note in passing that with higher electrical pressures greater flexibility is allowable in the choosing of the site. Many of the existing stations are not ideally placed for larger units and the larger demands that will arise from areas such as those already provisionally prescribed by the Commissioners.

The electrical industry has now arrived at a point where a certain huge demand awaits development. This great potential demand, which in the national interest must be provided for, cannot in the best interests of the country be adequately catered for by the patchwork system at present existing. For the successful carrying out of any great project it is essential:

(1) That there should be a strong conviction of the necessity for the project.

(2) That a bold course should be pursued which will provide adequately for the requirements of the case.

When we consider that electricity by reason of the experience gained by its use in past years has proved to be the most convenient and adaptable form of power, and taking these and other advantages into consideration, it is safe also to say that electricity is the cheapest

form of motive power. The altered conditions due to the high cost of labour make it desirable to install labour-saving apparatus wherever possible. The tendency also to improve the conditions of labour and to ameliorate the hardships which attend many duties which labour is called upon to fulfil will most certainly lead to the installation of the most up-to-date machinery or apparatus which would enable the men employed to effect greater production, and tend to develop the resources of the country for the general benefit of the community.

An essential feature to enable the proposals embodied in the Electric Supply Report to be carried out would be in regard to the use of cables capable of withstanding very much higher pressures than has been feasible in the past. It is well known that the dielectric strength of the paper and compounds of which these cables are composed is very high, and if it were not for other considerations it would be possible to make cables satisfactory for working at very high pressures. Unfortunately the admixture of air in the insulating materials has a very detrimental effect. When the air is stressed to the breakdown point, ozone is formed and attacks the material of which the insulation is composed, with the result that deterioration sets in. The action may be slow, but still there is the danger that this gradual deterioration may take place and failure result. The efforts of the cable makers have, therefore, been directed to effect the elimination of the air in the course of manufacture, and great improvements on these lines have been made, and it is now claimed that it is possible by the new methods to eliminate to such an extent the amount of air enclosed in the insulation that it may be safely ignored. The elimination of air has also the effect of reducing the dielectric losses in the cable, and it may now be claimed that the dielectric loss in a super-tension cable can be kept within such limits as not to be a material factor. It must be admitted that this is a great step in advance, and has a very important bearing on the question of supplying these larger districts by underground super-tension cables, or by a combination of underground cables and aerial lines.

It is gratifying to know that cable makers are prepared to-day to supply cables for working at 66 000 volts, and that at the present time a considerable quantity of cable is in course of manufacture for working pressures of 33 000 and 55 000 volts. It is only natural that manufacturers should wish, for the time being, to work on the conservative side, but as actual experience is gained we may hope to see still further improvements in regard to the manufacture of these underground mains.

As the various existing undertakings are linked up with super-tension cables the transmission mains would pass through districts which could be tapped on the way, and many areas would be able to obtain a supply, which otherwise would not be the case. Any waste heat in the district could be utilized for the generation of electricity, and the electrical energy so provided could be delivered into the transmission mains. In the past there has always been a certain quantity of unmarketable fuel which it would be possible to utilize if a super-tension network of mains were in existence. The useful chemical products in the coal could be extracted and the gases utilized for the production of electricity, or the coal could be utilized in a pulverized form for the generation of electricity. These various advantages cannot be secured without the laying down of a super-tension interlinking network, and it is difficult to see how this can come about unless the whole district so far as generation and main transmission is concerned comes under one authority.

The advantages to be gained by unity of control may be classed under two headings :

(1) Those affecting directly the supply interests.
(2) Those affecting national interests.

It is true that these various advantages are to a certain extent linked up and interdependent upon one another. It is true also that those at present responsible for the control of electric supply are not out to consider the matter alone from the national point of view, their main consideration being one of finance. Those in authority will naturally consider that in the early stages after the formation of a Joint Electricity Authority the expenditure will be very heavy and it will be a considerable time before the initial expenditure is fully remunerative. In some cases on this account there may be hesitation in embarking on these large and comprehensive schemes. If, however, during this initial period arrangements were made whereby money could be provided by the Government at a low rate of interest or with an extended loan period, it would be a very material assistance in the development and would be an undoubted encouragement to the various authorities to enter whole-heartedly into these new proposals.

When these proposals outlined in the Electric Supply Report were being widely discussed before the country in 1918 to 1919, the general consensus of opinion was favourable. After, however, the sudden curtailment of the provisions of the Bill due to the action of the House of Lords, and possibly also on account of the enormous increase in price of plant and the cost of construction, there appears to have arisen a doubt in some minds as to whether these proposals are equally sound when these high costs for the construction of plant are taken into consideration. There is no doubt whatever that owing to the high prices it is most desirable to minimize all unnecessary expenditure at the present time. In considering this very important and complex problem it is essential that the matter should be looked at, not from the point of view of the immediate future, but rather to the position that will arise 5 or 10 years ahead, and certain important facts must be taken carefully into consideration.

(1) At the present time all undertakings are receiving applications for supply at an unprecedented rate.
(2) Undertakers have been clamouring to be allowed to add additional generating plant. Where extensions have been granted this plant will only provide for the needs for a very short period, so that additional plant will be required at an early date.

(3) In some cases authorities are forced to take a supply from their neighbours who have had extensions of plant granted to them, which will bring nearer the time when these favoured authorities will have to renew their applications to the Commissioners.
(4) Some of these stations where extensions have been granted are incapable of further development, and new stations will have to be laid down if the demand is to be catered for.
(5) The sum total of these various additions is likely to be considerably greater than would be the case if the undertakings were linked together under one control.
(6) The total capital expenditure on plant laid down by the various individual undertakings over a period of 5 or 10 years is likely to be considerably greater than would be the case under a system such as has been proposed.
(7) Under the present system of multiple control, the development is likely to be slower, and when railway electrification comes along the separately owned stations will be unlikely to be able to cater for the requirements. The probable result would be that the railway companies would have to install generating stations for their own use, and the improvement of diversity factor due to the railway demand would be lost to the supply authorities.

These are some of the problems that authorities must take into serious consideration when coming to the conclusion as to the advisability of forming a Joint Electricity Authority. On the side of the advantages of a unified system it must not be forgotten that additional safety against breakdowns would be secured owing to the fact that assistance would be available from other stations. The economy to be derived by running the most efficient plant for long hours and minimizing the use of the least efficient plant must not be lost sight of. In addition to this also must be mentioned the economies to be derived from the use of larger units of generating plant.

Let me deal for a moment with the suggestion that the high cost of plant is a reason why the proposals suggested in the Electric Supply Report should not be given effect to. Most certainly the high costs are a serious drawback to the electric supply industry, just as high costs are a serious drawback in every phase of life. The argument of the high costs is not applicable only to the installation of plant put down under the proposals embodied in the Electric Power Supply Report, but are applicable to the installation of any plant, however large or small. I do not think there would be many people connected with the electrical industry who would argue that the very large demands the undertakers have applications for at the present time should be ignored, and that owing to the high costs it is undesirable to install plant to meet these demands.

If we take this for granted, the contention would be that while it is right to expend money on existing generating stations in spite of the fact that the plant bought and installed will have to be purchased at the high cost now ruling, yet it is wrong on account of the high cost to spend money on a large generating station or transmission lines under the proposals of the Electric Power Supply Report. Surely such a contention is not logical and savours rather of bias than sound argument. It might be argued that it will be cheaper to reconstruct existing stations with plant of the largest capacity they are capable of holding. It is quite certain, however, that such a proceeding would only be a temporary expedient, and that ultimately a much more extended view would have to be taken, with the result that, in the long run, the total capital expenditure would be greater.

Naturally each district would have to be very carefully considered from the point of view of its present development and the future possibilities, and these factors must be a guide to those responsible for the recommendation of a joint scheme, and also for the lines upon which such schemes should develop.

If one looks at this problem with an open mind, taking fully into consideration the present state of affairs, the great possibilities of the future for the use of electricity in the industrial parts of the country, one is driven to the conclusion that the only real solution of the problem is to be found under the principle of unity of control over a district.

Many interesting features are under consideration or being tried with a view to improving the efficiency of the generating station. Improved efficiency may be expected from:—

(1) The use of larger generating units working under high vacuum.
(2) The use of higher steam pressure.
(3) The heating of the condensate by means of steam abstraction from the turbine.

A fourth source of possible economy may be found by re-heating the steam at different stages of its passage through the turbine. This involves considerable complication and extra expense, and it has yet to be proved whether the saving in economy obtained will be sufficient to offset the extra cost and the complication involved.

As a general proposition it may be stated that improved efficiency in the turbines must be looked for from a raising of the total temperature of the steam on the one hand and a lowering of the final temperature on the other hand. In recent years the tendency has been to raise gradually the total temperature of the steam as experience has been gained with the materials most suitable to withstand this higher temperature. To-day temperatures up to 700° F. are considered quite satisfactory. This total temperature may be attained by using steam at moderate pressure and a high degree of superheat, or by increasing the pressure with a corresponding reduction of superheat.

It is on these later lines that the most modern developments are taking place, and those upon which it is expected that economies can be secured. Turbines and boiler plant have been in operation for a considerable time working at 350 lb. pressure, and plant to work at pressures considerably higher than this is in course

of construction. These higher pressures raise fresh problems in the construction, lay-out, and operation of the plant, which experience alone can solve. At the other end of the scale there would not appear to be much advantage to be gained by carrying the vacuum to a higher degree than about 29 inches, which has been attained in a number of instances. There is little doubt that in the larger stations using larger units steam pressures up to 350 lb. per square inch will be used, and the tendency will be towards still higher pressures as further experience is gained. It may not be many years before we see stations running with steam at 500 lb. pressure, and with a total temperature of about 750° F. A number of turbines are being installed at the present time, in which steam is tapped from the turbine and discharged into feed heaters through which the condensate from the condenser is passed, the object being to return a portion of the latent heat of the steam to the boiler. By this means, and by utilizing in conjunction with it the heat from the steam used for auxiliaries, a considerable saving is claimed. It will be evident that by utilizing steam from the auxiliaries in conjunction with a system for tapping the steam at various points of the turbine, the feed water can be heated to a very considerable temperature, but it must not be overlooked that to carry the feed temperature beyond a certain figure would diminish the value of the economizer. In the event of this system being adopted it would be necessary to utilize the flue gases to heat the air before it enters the boiler furnace. This involves a bulky and costly apparatus and a considerable complication in ducts to convey the hot air to the boiler furnace, but on the other hand, the expense is relieved to some extent by the absence of the necessity for the economizer.

At the higher pressures it would be an advantage if the economizer could be done away with, as cast iron becomes uncertain and cast steel is very expensive.

Wrought steel tubes would be quite satisfactory so far as the pressure was concerned, but there would be a doubt whether corrosion and wastage of tubes would be excessive. To meet this difficulty a scheme has been propounded whereby pressure on the economizer is kept low by using two pumps in series, or preferably by using a multiple rotary pump, the connection to and from the economizer being placed between any two stages of the pump, depending upon the pressure at which it is determined that the economizer shall work, the remaining stages being sufficient to overcome the pressure in the boiler. A system of this sort, while enabling the pressure on the economizer to be kept within the safe limits of the cast-iron tubes, adds very materially to the cost and complication of the feed pipes.

Dr. Ferranti 10 years ago stated that a conversion efficiency of 25 per cent would be obtained, and that such an efficiency was necessary if electricity was to be available for all purposes, and he has recently made the statement that he considers that in the near future central stations will return on the switchboard 28 per cent of the energy in the fuel. If this forecast is attained it will show what a wonderful advance has been made in economy compared with the overall efficiency of generation in the earlier stations. It is interesting to note that one large generating station which will very soon be in operation has been designed for an efficiency of 24 per cent. The results of this station will be watched with very great interest, as a number of novel features have been embodied in the design.

Distribution.

Under the new proposal of Joint Electricity Authorities or District Boards, the question of distribution and transmission of electrical energy assumes a position of importance greater than that under the older system of divided control. The higher tension to be used and the larger areas covered and the larger power in the form of generating plant connected to the system bring into prominence the necessity for more powerful switchgear and automatic control to isolate quickly any faulty section. A good deal of experience has been gained in this country in some power company areas, but in the smaller systems the necessity for this automatic control has not been felt to the same extent as in the case of the larger districts. These are some of the problems that engineers in the smaller stations will be called upon to deal with when the new district control comes into being. I would recommend our supply engineers to study carefully the various systems at present in operation or suggested, and would point out to them that the addition of this apparatus with extra pilot cables, or split conductors, adds very much to the cost of the mains system. If they could devise some system which would reduce this cost it would be a great advantage to the supply and a material advantage to themselves.

In connection with the distribution of low-tension current there is a problem of considerable moment affecting those undertakings which have developed their distribution on the direct-current system. Many of these undertakings have carried their low-pressure mains far out into the residential districts, and in many cases long distances from the substation where the high-pressure alternating current is converted to low-tension direct current. This has necessitated the use of very long feeders of large size, in which, owing to their length, the current density has to be kept very low in order to provide against pressure drop. With the largely increasing demand for electric cookers and radiators in the residential areas, increasing difficulty is being experienced in dealing with these applications on account of pressure drop.

One solution of the problem would be to substitute alternating current. High-tension mains could be run through the area and static substations laid down at convenient points. The system is convenient and flexible, but the great drawback is the cost, on account of the fact that in most cases the low-tension mains laid are not the most suitable to work with alternating current. In this connection it is very interesting to note the considerable advance that has been made in the design and application of mercury rectifiers. This apparatus is in very successful operation in a number of places on the Continent, and there will soon be some working in this country. There are, however, drawbacks to this apparatus, and the chief of them is that the appliance is not automatic throughout its entire range.

Below a certain load the arc is not maintained; but above this point complete automatic working is attained. Experiments are in hand with a view to making the apparatus automatic throughout its full range from no load to full load. If this can be accomplished, and those engaged on the investigations are very sanguine of the result, such an apparatus would be of immense assistance in dealing with these outlying districts referred to. By installing substations fitted with this plant in suitable sites the existing network would be enabled to work at a very much higher current density than formerly. The cost of the low-tension mains in these areas has been very great, and it is a very big problem to contemplate the replacement by another form of cable. If, however, this apparatus can be made completely automatic, it will, if it does not entirely solve this problem, at least have made it possible to meet developments on the existing network for a considerable time. It will also enable the gradual replacement of the existing network, should it be thought desirable, by one more suitable for alternating current.

Costs.

As costs of production have an important bearing on the developments, it may be interesting to consider this question for a few moments. The war has undoubtedly had the effect of altering very materially the relation of the various items of cost of production to one another as compared with a pre-war basis. The cost of coal delivered into the bunkers has increased from three to four times what it was before the war. At the same time the quality of the coal has materially deteriorated, and, owing to the difficulty experienced in getting supplies, undertakings in many parts of the country have had to use qualities of coal that they would not have purchased before the war, with the result that this item of cost per unit has shown a marked increase. In pre-war days this item of cost per unit was a relatively small one compared with the total cost of production. Now, however, owing to the high costs, inferior quality, and the higher freight charges, it has assumed a much more important place in the item of total costs. Efficiency of generating plant has therefore become a matter of immense importance.

It is to be hoped that in the future the price of coal may come down from its present high level when business settles down and when all controls are removed and the laws of supply and demand ultimately attain full sway again. It is not to be expected, however, that we shall see the same level of prices for this commodity which we were accustomed to before the war. We may, however, expect to see the item of coal cost per unit materially reduced from its present figure on account of lower prices for the commodity, better quality of coal, improved efficiency of generating plant, and higher load factors on the systems when the new proposed Joint Electricity Authorities are in operation. Wages have increased about 150 per cent, and even if the rate of wages remains the same, the actual cost per unit for wages may be expected to be less with the use of larger generating units and higher evaporative capacity of boilers and improvements in labour-saving devices and the larger outputs. The item of repairs at the present time is relatively high.

First, on account of the high labour charges and the high cost of material.

Secondly, owing to the war, the scarcity of plant in most stations has made it impossible to effect the proper repairs.

Stations will gradually return to more normal conditions so far as the general overhaul of the plant is concerned, but can only look to the increased output as a means of reducing the cost per unit. Rent, rates and taxes, and management charges have increased materially, and there seems little likelihood of expecting much reduction for some years in rates or taxes. Increased output may, however, afford some benefit on the actual cost per unit.

The item of capital charges, which before the war was the highest of any of the items of cost, and in many stations was almost equal to all the other charges put together, has fortunately not shown the large increase which has been added to the other items. In many stations, particularly those where the growth in output has been very great, this item has shown a considerable reduction. This is accounted for by the fact that output increased in greater proportion than the capital expenditure, and, further, that the margin of reserve plant which prior to the war was considered desirable had to be encroached upon. Owing, however, to large additions that are being made to the generating plant, and distribution systems costing something like three times the pre-war cost, it is evident that this item is likely to show an increase in the future. The largely increasing outputs in industrial districts where power predominates should, however, prevent this item from showing any very marked increase. In face of the increase in cost that undertakers have had to meet, it has been necessary to increase the charges for electrical energy materially, in some cases up to 120 per cent. In spite of this, electricity is relatively much cheaper than other commodities. It is to be hoped, and it can be reasonably anticipated that, as matters gradually settle down on to a more fixed basis, we may expect to see a marked reduction in the selling price of energy.

In conclusion I should like to say a word to the younger members and students who are just starting their careers in the electrical profession. Many wonder what will be their prospects. Well, I am an optimist so far as the development of electricity is concerned, and I venture to think that there will be plenty of opportunity for all in the electrical industry. It is a profession in which the prospects of development are very bright. It is needless for me to say, however, that the good positions that will arise will go to those most fitted and capable of fulfilling them. Competition with foreign countries will be very severe. In this comparatively new industry we shall not be favoured as we have been in the past in some of the older industries where our trade and connection was established when competition was less keen than it is likely to be in the very near future. It is up to the younger men, therefore, to fit themselves by all diligence and industry so that they may bear their part in upholding the nation's position in the face of all competition.

EAST MIDLAND SUB-CENTRE: CHAIRMAN'S ADDRESS

By F. S. Grogan, Member.

(*Address delivered at* Loughborough, 19 *October*, 1920.)

(ABSTRACT.)

The Institution does a vast amount of progressive work that is little known to many of its members until the results are published. For instance, the I.E.E. Regulations for the Electrical Equipment of Ships, issued in September 1919, represented three years' work. At the present time a lot of hard work is being done by Sub-Committees in connection with the revision of the I.E.E. Wiring Rules. In this connection, some of the difficulties brought before the Wiring Rules Committee might, with advantage, form the subject of a discussion before this Sub-Centre. The question of "Earthing," for instance, brings us to a non-commercial proposition which, in order to carry out fully the logical method of efficient earthing, would necessitate the supply authority laying its own earth main and connecting to it all earth connections throughout every installation.

Mr. C. H. Wordingham recently published in the technical Press his considered view forecasting the distribution of electricity at high pressures in the future and the use of very low pressures on consumers' premises, say 100 or even 50 volts. I feel that he has always in mind a standard low pressure of 50 volts in the house, and I believe that he may ultimately convert us all to this view. Mr. G. Scott Ram's Annual Report on "Electricity in Factories and Workshops" records 20 out of 28 fatal cases of accident from electric shock which have arisen from alternating currents at a pressure below 250 volts; whereas none have occurred from direct current.

Progress.

Mr. Ram in his Report also states that the growth in the use of electrical energy for all purposes, stimulated by the war, still continues unabated, and that the demand for power in some districts far exceeds the capacity of the public supply stations. In fact, some stations are now working without any reserve of plant. Numerous extensions of generating stations are in progress, schemes for new super-stations are under consideration, and two new stations of large capacity and on the most modern lines are about to be put into operation. The Electricity Commissioners at a recent Inquiry in regard to electricity supply extensions in London, had evidence before them of a 5 000-kW load in a chemical works having to be refused. These facts show that manufacturers of electrical apparatus may look forward to a large increase in business in the immediate future. In fact, we find progress in every branch of electrical engineering.

Electrification of Railways.

I will next refer to an important Interim Report by the Electrification of Railways Advisory Committee appointed by the Ministry of Transport. This Report, just issued, deals with certain general points, and further details are promised in a subsequent Report. It decides definitely on the system of three-phase generation and high-tension direct current at the substation busbars, specifying 1 500 volts as the standard and collection from either overhead wires or rail conductors, provided that the details of construction comply with recommendations to be issued in the subsequent Report.

It is interesting, in connection with this decision, to refer to the James Forrest Lecture on "Electric Railways" delivered by Mr. H. M. Hobart at the Institution of Civil Engineers in December, 1915. Mr. Hobart in that Lecture concludes as follows:—

"1st. We are on the eve of the extensive employment of electric locomotives on railroads at present operated with steam locomotives.

"2nd. The direct-current system is the most appropriate.

"3rd. Direct-current locomotives for use with high-pressure contact conductors are now a thoroughly demonstrated success."

Mr. Hobart also went into the question of systems very thoroughly and, in spite of the opposition by the advocates of the single-phase system, stated the above definite conclusions.

It is a matter of regret that the Germans were ever allowed to erect the alternating-current system on the Brighton Railway. It is generally known, however, that they succeeded in doing so by giving absurd guarantees which no English firm would give.

Although the general decision as to system has been given, there is still some latitude allowed as to the actual line or rail voltage. According to the above Report it may be 600, 1 200, 750, 1 500, or 3 000 volts. The Midland Railway have at Heysham an experimental alternating-current line. They also have on their main line a long down-gradient from the Peak into Derby to cause them seriously to consider the saving in electrical energy with direct current and regenerative braking, which system is reported to effect under favourable conditions a saving of something like 20 per cent of the energy consumed.

Speaking of electric traction reminds me of an early experience when I left central station work for tramway work. At the electric lighting station I was taught as a junior how to test under stringent municipal Regulations an installation prior to connecting it up to the Corporation mains at 220 volts. Work in new buildings often had to be condemned under those Regulations, fees had to be paid by the contractor and further tests taken, resulting in trouble

with the new consumer and the contractor. I was suddenly transferred from that atmosphere to become an assistant on tramway contract work. When the first few cars were equipped for a trial run, I naturally asked my Chief for a testing set; we were already threatened with a penalty for late completion. His answer was very practical: "Oh, put the current on, it will soon find the faults." The line voltage being about 500–550 volts naturally did declare itself on occasions.

These two extreme examples bring home to one the value of Meetings such as ours. Had the two chief engineers responsible for those two methods of testing insulation resistance had an opportunity of meeting and exchanging ideas on the subject, they might have educated each other to steer the middle course. The by-laws issued by some electricity supply authorities to-day, in connection with the public supply, are too stringent and tend to retard our industry. However, with the good work which our Institution is doing, it is only a matter of time for all adverse conditions to be removed, and, as I have indicated already, the general outlook for the electrical industry in all its branches was never brighter, and this in spite of the general trade depression and industrial unrest.

PROCEEDINGS OF THE INSTITUTION.

653RD ORDINARY MEETING, 18 NOVEMBER, 1920.

(Held at the Institution of Civil Engineers.)

Mr. Roger T. Smith, Past-President, took the chair at 6 p.m.

On the motion of the Chairman the following vote of condolence with the family of the late Professor J. Perry, LL.D., D.Sc., F.R.S., Past-President, was passed, the members standing in silence :—

"The members of the Institution of Electrical Engineers have learned with profound regret of the death of Professor John Perry, F.R.S., a Past-President of the Institution, and hereby desire to express their most sincere sympathy with the members of his family in the great loss which they have sustained through his death, and at the same time to record their deep appreciation of the great services rendered by him to Electrical Engineering."

The minutes of the Ordinary Meeting of the 30th June, 1920, were taken as read, and were confirmed and signed.

A list of candidates for election and transfer approved by the Council for ballot was taken as read and was ordered to be suspended in the hall.

The following donations were announced as having been received and the thanks of the meeting were accorded to the donors :—

Benevolent Fund : W. Aitken, P. R. Allen, A. Armitage, Sir W. G. Armstrong, Whitworth & Co., Ltd., E. Barlow, H. A. Bastable, A. W. Beaumont, L. Birks, G. Brander, R. V. Broberg, J. A. Bromley, J. S. Buckley, S. Burns, G. W. Carpenter, R. L. Cleaver, W. W. Cook, A. C. Coward, W. J. F. Debley, H. H. Denton, A. C. de Oliveira, A. Douglas, G. L. Drury, A. R. Everest, W. Eynon, H. S. Fellowes, P. S. Fox, F. H. Gardiner, J. Garland, W. C. Goodchild, F. W. Green, H. Green, A. F. Harris, J. H. Havelock, H. Hazelwood, R. G. Hullah, L. J. Hunt, P. Hunter-Brown, F. G. Ketelbey, W. de M. Landon, R. M. Longman, P. M'Dermid, A. E. McColl, A. Morris, F. Morton, A. R. Murray, M. S. Narayanan, E. A. Nash, E. P. Neate, T. B. L. Newstead, G. Nicholson, J. R. D. Norie, W. Orrell, G. C. Paget, J. Paterson, T. W. Price, W. O. Ridley, A. Roberts, W. O. Rooper, W. V. Ryder, S. Simpson, R. T. Smith, J. H. Southern, C. P. Sparks, C.B.E., F. S. Spiers, W. R. Steele, H. M. Stronach, C. S. Taylor, R. Tourover, J. A. Troughton, M. G. Wallace, F. Walton, J. Waring, H. G. Weaver, F. H. Williams, Mr. Broomhall (per R. M. Longman), The "Twenty-Five" Club (per W. B. Esson), Warwickshire R.E. (V) Corps Signal Company (per Captain S. T. Pemberton).

Library : L'Association Suisse des Electriciens, L. W. Bates, D. A. Bremner, Sir Charles Bright, F.R.S.E., The British Engineering Standards Association, The British Thomson-Houston Co., Ltd., Dominion Water Power Branch (Canada), H. Carpmael, Sir Dugald Clerk, K.B.E., D.Sc., F.R.S., The Department of Scientific and Industrial Research, J. R. Dick, Messrs. W. H. Dorman & Co., Ltd., J. S. Dow, F. Fernie, C. C. Garrard, Ph.D., L. Gaster, H. E. Goldsmith, F. Guery, Sir Robert Hadfield, Bart., F.R.S., P. J. Haler, Harvard University, E. G. Hillier, The Hydro-Electric Power Commission of Ontario, The Institute of Inventors, The International Electrotechnical Commission, The Japanese Imperial Department of Communications (Electrotechnical Laboratory, Tokio), Messrs. Johnson & Phillips, Ltd., P. Kemp, The Lancashire and Cheshire Coal Research Association, H. S. Marquand, The Massachusetts Institute of Technology, The Meteorological Office, Messrs. Methuen & Co., Ltd., The National Physical Laboratory, A. M. Nicholson, H. C. Oersted Committee (Copenhagen), Messrs. Price's Co., Ltd., Aldo Righi, E. Rowarth, The Royal Canadian Institute, Messrs. E. & F. N. Spon, Ltd., The Steel Research Committee, H. W. Sullivan, C. Sylvester, W. T. Taylor, The Bureau of Standards (U.S.A.), The University of Illinois, C. F. Wade, A. P. Young, O.B.E.

The Premiums (see *Institution Notes*, No. 21, page 22, May 1920) awarded during the Session 1919–20 were then presented by the Chairman to such of the recipients as were present. He also announced the Council's award of scholarships for 1920–21 (see *Institution Notes*, No. 22, page 3, December 1920).

The Chairman : It is now my privilege to ask the new President, Mr. Ll. B. Atkinson, to take the chair.

The chair was then vacated by Mr. Roger T. Smith and taken by Mr. Ll. B. Atkinson amid applause.

Sir John Snell : A very pleasant task devolves upon me, namely, to move the following resolution : "That the best thanks of the Institution be accorded to Mr. Roger T. Smith for the very able manner in which he has filled the office of President during the past year." I venture to say that it was no light task which Mr. Roger Smith undertook last year in following in the footsteps of his two distinguished predecessors, whose years of office had been marked by an extraordinary outburst of activity on the part of the Institution. When he was called upon to undertake his duties last year he was then suffering severely after undergoing a serious surgical operation, and the way in which he so assiduously carried out his duties as President was no less than heroic. It was not his fault that circumstances during his year of office did not require the Institution to burst out into some new field of activity, and I think it was for the good of the Institution that we should have a period of comparative calm and of consolidation for all that had gone before in the previous years. The year has, however, been one of great anxiety, with coal questions and industrial disputes, and the President has been called upon to give

to the Institution unremitting labour and attention. Further, I would say that Mr. Roger Smith's year of office has left this great Institution more firmly established in the counsels of the nation than before he undertook the presidency. I am sure that the members would desire me to include in our vote of thanks a reference to the President's wife. Having regard to what I have said as to Mr. Roger Smith's ill-health last year, it must have been a great strain to Mrs. Roger Smith, who has by her gracious attention and attendance at many of our functions so ably assisted the President in carrying out his duties.

Mr. J. S. Highfield : It is a great pleasure to me to second the resolution which Sir John Snell has proposed. I can endorse everything that Sir John has said, except that I am not sure that Mr. Roger Smith would agree to the term " comparative calm " as expressing the conditions under which he has had to perform his work during the past year. I should say that a turbulent sea was frequently the condition under which he had to pilot the Institution on its course. An Institution of this kind depends for its force and weight amongst the general public on its sense of discipline, on its dignity, and on the efficiency with which it fulfils its function. Our Institution has, I think, acquired additional weight during the last few years in all these respects, and I hope that it will go on acquiring further weight in these matters, because there is no doubt that a body of selected men, specially trained, whose duties mainly concern work and production rather than mere words, if efficiently led can exercise a very great influence on public opinion in these very difficult times. I am quite sure that no man has done more to contribute to that acquirement of dignity, discipline and efficiency than Mr. Roger Smith, and I have great pleasure in seconding the resolution.

The resolution was then put by the President and carried with acclamation.

Mr. Roger T. Smith : I thank Sir John Snell and Mr. Highfield for the all too kindly words with which they have proposed this resolution, and I thank the members for the way in which they have received it without making that protest which I feel they ought to have made at the over-statement of the case. From that I would except the very gracious reference to my wife. The Institution now has the greatest membership of any engineering Institution in this country, and it is the endeavour of every President that its character should correspond to its greatness in numbers. The burdens and duties of the President grow with the increase of those numbers, and I think that that burden would become almost too great if it were not that each year gives the Institution a Council which more closely represents the varied interests of the Institution and of the industry which it represents. I can assure the members that they have been represented during the last Session by a very able Council indeed, and I should like to thank them collectively and individually for the good work they have done for the Institution. At the same time, I should like to thank the Secretary and his staff for the way in which they have done their work under very difficult circumstances—I refer of course to the fact that we are not yet in our own building—circumstances which I hope will very shortly be altered.

The President then delivered his Inaugural Address (see page 1).

Colonel R. E. B. Crompton : I have heard a great many Presidential Addresses in this and other rooms and I think this is the best that I have heard for years. When it is printed in our *Journal* it will be a work of reference of high quality. I have known our President for 40 years and in the early days he nearly became one of my pupils. The years that followed were strenuous ones about which the President has said very little in his Address. He has referred to the giants of those days, but we were only giants because we were up against the financial men who would not help us with money. I recollect meeting our President at the testing beds of Messrs. Willans & Robinson's works at Thames Ditton. There we had a number of young fellows all trying to develop the modern electrical generating machinery, all helped by Peter William Willans. Our President was one of those young men. One of the reasons why it is such a pleasure to me to talk about our President is that he is not sufficiently well known ; he is a man who has been throughout the whole of his life quietly interesting himself in the right things ; a keen observer and ready to take advantage of what he has observed he has been able to turn it to the profitable uses of his profession. Unfortunately, men of his calibre, the workers of the world, do not come into the limelight. I am not going to enlarge upon his work for the cable makers. Like me, he is a farmer and a gardener, and I have taken notice of the very things which he also has observed. I have observed that the starting of a crop in spring can be retarded by the temperature of the soil at a few inches below the surface being 2 degrees below the normal. He has given us a case of snow being ploughed in. and the effect lasting for years. This I can confirm. When I began to try to raise the temperature of the soil by embedding electric coils in it, I discovered that a very small variation of 2 or 3 degrees above 60° F. had the effect that the crop in the spring started so much ahead of the rest of the crop that it made a record crop for that part of the field. I think our President has also done splendid service in pointing out what an enormous field there is for electrical engineers in food production, by stimulating the producing power of our soil. Food supply, power supply, and transport are the engineer's duties. In conclusion, I should like to say, in view of the tension which exists at the present moment between the operatives and the thinkers among workmen, that I believe the President has put forward the real remedy : we must educate our workmen in such manner that they will become really interested in what they are doing. Mass production, so much praised, can only lead to greater dissatisfaction than ever, unless we find some means of counteracting it, and I suggest that one of the ways of doing so is to get a large proportion of our workers to interest themselves in their non-working hours as either farmers or gardeners. There is no such change of occupation for an electrical engineer equal to gardening or, if he is getting old, as I am, retiring to a farm.

I beg to move "That the best thanks of the Institution be accorded to Mr. Llewellyn B. Atkinson for his interesting and instructive Presidential Address, and that, with his permission, the Address be printed in the *Journal* of the Institution."

Mr. W. A. Chamen: I have great pleasure in seconding the resolution which Colonel Crompton has just proposed. Like him, I think that it is the best Presidential Address to which I have listened. The days in which we live are, of course, quite different from the pre-war days, and I wonder whether such matters as our President has put into the latter part of his Address have ever been heard before in an assembly of this Institution or any other scientific Society. I am very thankful that he has done it. He has gone altogether outside science, as we have thought of science. But, after all, what is the use of all the discoveries we can make, if we are going to have, first of all, such a fearful war, in which every discovery has been turned to the destruction of life, and then, after the war, to have such trouble at home as we did not have during the whole time the war was on? All these things must give us cause to think. They have given our President cause to think, and he has thought well and deeply. One cannot criticize anything he has said, of course, but I myself believe that the troubles from which we are suffering are not so simple as he seems to think, and they cannot be got over entirely by the methods which the President suggests, good as they undoubtedly are. There is something else which must be done. We have all been brought up to think that the only way in which commercial success can be obtained is by buying materials and labour at the very lowest price. That is what we have been taught to consider moral commerce. I want to suggest that that is fundamentally wrong. We have all been taught that self-preservation is the one thing we must look after. That is right and necessary to a point, but imagine for a moment that everyone was firstly concerned with the welfare, safety and comfort, not of himself at all, but of his neighbour. This is considered to be an impossible proposition, but that arises simply from the fact that it has never been tried at all. In my opinion we have arrived at the day when that order of things has got to come.

The resolution was then put to the meeting and carried with acclamation, and after the President had briefly replied the meeting terminated at 8.5 p.m.

11TH MEETING OF THE WIRELESS SECTION, 24 NOVEMBER, 1920.

(Held at the Institution of Civil Engineers.)

The President, Mr. Ll. B. Atkinson, took the chair at 6 p.m.

The minutes of the Wireless Sectional Meeting of 23rd June, 1920, were taken as read, and were confirmed and signed.

The President: Before I ask the Chairman of the Section to give his Address, I want to introduce to those who do not know him, and to express the pleasure we have to-night in having with us, one of our Honorary Members, the veteran inventor of the telephone, Professor Alexander Graham Bell. This is not the first occasion on which he has attended one of our meetings. I do not know them all, but our records show that on the 31st October, 1877, he gave us an address entitled "Researches in Electric Telephony." The telephone, with the human ear coupled to it, is of such extraordinary delicacy, and gives so much information which no galvanometer can give, that it is not too much to say it has been one of the great features of the rapid advance of wireless telegraphy. I propose to you the following resolution: "That this meeting of the Wireless Section of the Institution of Electrical Engineers conveys to Professor Graham Bell their pleasure at seeing him, and their appreciation of the honour which he has done the Institution in coming to this meeting. We desire to express to him the hope that he may long live in good health. We wish him a pleasant completion to his journey to England—and Scotland—and a happy and comfortable journey home." Without any formalities I will ask the meeting to pass that resolution by acclamation.

The resolution was then carried by acclamation.

Professor Alexander Graham Bell: I cannot state what pleasure it gives me to meet the members of the electrical profession—a profession which I forsook a good many years ago. It gives me special pleasure to meet that section of the electrical world which represents advance, the Wireless Section, which comprises the people who are going to dictate the future. I am one of those who can speak perhaps of the past. In wireless telephony we have a field that is completely new, and really we do not know what to think about it. It was only a short time ago that Mr. Carty, of America, made a remarkable experiment at the Arlington station near Washington, in an attempt to speak across the Atlantic to the Eiffel Tower by wireless telephone. The attempt was successful, but still more startling was the result—that a wireless operator in Honolulu overheard the conversation. From Honolulu to the Eiffel Tower is over 8 000 miles, one-third of the distance round the world; and that seemed to me to foreshadow the day when a man in any part of the world might speak by telephone to a man in any other part of the world without wires at all. The telephone has gone far beyond my early conceptions of it. I assure the members that it gives me great pleasure to meet the advance guard of the electrical world to-night.

Dr. W. H. Eccles, Chairman of the Wireless Section, then delivered his Inaugural Address (see page 77).

A note of thanks to Dr. Eccles, proposed by the President and seconded by Professor C. L. Fortescue, was carried with acclamation, and the meeting terminated at 7.45 p.m.

SUMMARY OF PROCEEDINGS AT SPECIAL GENERAL MEETINGS HELD ON THE 2ND AND 17TH DECEMBER, 1920.

The notice convening the above meetings was as follows :

Special General Meeting, Thursday, 2nd December, 1920.

Notice is hereby given that a Special General Meeting of the Corporate Members (Members and Associate Members) of the above Institution will be held at the Hall of the Institution of Civil Engineers, Great George-street, Westminster, London, S.W. 1, on Thursday, the 2nd December, 1920, at 6 p.m., for the purpose of considering and, if thought fit, passing the subjoined Resolution in the manner required by Section 69 of the Companies (Consolidation) Act, 1908, for the passing of an Extraordinary Resolution as therein defined, with a view to the same being subsequently confirmed as a Special Resolution :

Resolution.

(*a*) That the following be substituted for Article 27 of the Articles of Association of the Institution :

27. Persons who have not compounded shall from the 1st January, 1921, pay the following annual subscriptions :

	In the United Kingdom £ s. d.	Abroad £ s. d.
Members	5 5 0	4 5 0
Associate Members	3 15 0	3 5 0
Associates	4 5 0	3 15 0
Graduates	2 10 0	2 10 0
Students, until the end of the calendar year in which the age of 21 is attained ..	1 1 0	1 1 0
Students, after the calendar year in which the age of 21 is attained	1 11 6	1 11 6

(*b*) That the fourth paragraph of Article 33 beginning with the words "Members who . . ." and ending with the word "guineas" be amended by the substitution of the word "four" for the words "two and a half."

Special General Meeting, Friday, 17th December, 1920.

Should the above Resolution be passed by the requisite majority at the above meeting, the Resolution so passed will be submitted for confirmation as a Special Resolution to a second Special General Meeting of the Corporate Members (Members and Associate Members) of the Institution which will be held at the Hall of the Institution of Civil Engineers, Great George-street, Westminster, London, S.W. 1, on Friday, the 17th December, 1920, at 6 p.m.

By Order of the Council.

P. F. Rowell,
Secretary.

1, Albemarle-street, London, W. 1.
4th November, 1920.

SPECIAL GENERAL MEETING OF 2ND DECEMBER, 1920.

(Held at the Institution of Civil Engineers.)

The chair was taken at 6 p.m. by Mr. Ll. B. Atkinson, President, and the meeting was attended by 183 members.

The President: First of all, I ask your permission to take as read the Notice convening the Meeting. Will those in favour please raise their hands? [The meeting agreed.] You have already been prepared for the proposed increase of subscriptions by our Past-President, Mr. Roger Smith, in many addresses last year and by our Hon. Treasurer in his speech at the last Annual General Meeting. I believe that I and those who with me will lay before you the case as it presents itself to the Council will be able to convince you of the necessity of the step, and that the increases proposed are reasonable, and equitable to and as between the various classes of membership of the Institution.

In 1912, when the subscriptions were last revised, the total annual expenditure was £13 730. Of the intervening years I shall not speak in detail beyond mentioning that whilst up to 1914 the situation was much the same, a new condition arose in 1917 when the Institution Building was taken over by the Government. Believing it was only for a short time, premises were provided for the Institution by the Government at a nominal cost to us, considerably less than the rent received for our own premises, with the result that the Institution had an actual margin in hand on rent account, whilst, at the same time, the shortage of paper prevented the publication of proceedings in the *Journal*, so that during this period the effect of the greatly increasing costs of carrying on the Institution were masked. Table 1 shows a comparison of the expenditure under the principal heads in 1912, and that budgeted for 1921.

The increases in expenditure arise from two circumstances: First, the greatly increased costs and prices, most items having approximately doubled, and second, the increased activities of the Institution. The cost per member of each item is shown in shillings. I estimate that if the number of members had remained at 6 600 and the activities remained as 1912, the cost per member to-day would have been 75s. 6d. as against 74s. 6d. actually shown for 1921, that is, the benefit

of the increased membership has been nearly balanced by the increased activity.

Table 2 shows the Hon. Treasurer's budget for 1921, from which you will see that there is an estimated deficit of £5 425, the sum which has to be covered by the increase in subscriptions. Any proposal for dealing with the subscriptions must deal with this sum, unless a definite way of diminishing the expenditure can be put forward. The distribution of this sum among the several classes of the Institution has been a matter of prolonged consideration by the Council. It was felt that it is so desirable to encourage Students, from whom our Institution is largely recruited, that at all costs they should not be deterred from joining on the score of expense. For that reason their subscription has not been altered. The subscription of Graduates has been raised also by the smallest percentage. It was felt that the class of Associate, being filled as it

TABLE 1.

Approximate Expenditure per Member, 1912 and 1921.

	1912	Per Member (6 600 Members)	1921	Per Member (8 400 Members)
	£	Shillings	£	Shillings
Administration	4 090	12·4	11 000	26·2
London Meetings	250	0·7	500	1·2
Journal (printing and postage)	2 310	7·0	7 170	17·1
Cost of Institution Building (net)	2 730	8·3	4 680	11·1
Territorial Centres	870	2·6 (7·3 per local member)	2 750	6·6 (15·9 per local member)
Interest on Mortgages and Repayment of Mortgage on Building	2 170	6·6	2 255	5·4
Miscellaneous expenditure (*Science Abstracts*, grants to other bodies, etc.)	1 310	4·0	2 920	6·9
	£13 730	41·6s.	£31 275	74·5s.

TABLE 2.

Budget for 1921.

Estimated Expenditure.	£
Administration	11 000
Institution Building (less £3 000 rent from tenants)	4 680
Interest on Mortgages	1 340
Furniture repairs	50
Journal	7 170
Lending Library	50
Science Abstracts	500
Meetings (London)	500
Grants to Territorial Centres	2 750
Premiums for Papers	190
Special Grants (Research, Engineering Standards, etc.)	650
Annual Dinner	100
Conversazione	550
Legal expenses	250
Miscellaneous expenses	150
Payments on Capital Account.	
Repayment to Economic Society	915
Sinking Fund Premiums	280
Reference Library	150
	£31 275

Estimated Revenue.	£
Subscriptions (at old rates)	22 300
Entrance Fees, Vellum Diploma, Fees, etc.	1 500
Dividends, Interest, etc.	1 600
Miscellaneous Publications (Sales)	50
Tothill-street Property (net return)	400
Estimated deficit for 1921	5 425
	£31 275

is by men who have made their position, could afford to be raised by a proportionately larger amount. This done, the balance was dealt with in the subscription of Members and Associate Members in about the same proportion. The proposals of the Council, which represents all classes of the Institution, were unanimously adopted and have been received by the body of members as very reasonable and equitable, and eight Territorial Centres who have considered them with explanations given by their Chairmen who sit on the Council have informally expressed their approval. Apart from this, members have shown by the overwhelming number of proxies sent in that they approve the proposals. Certain points, points of importance, have been raised as objections and I propose to deal with them.

The Institution Building.—An objection is that the necessity for increasing the subscriptions arises from the fact that we are about to return to our own Building. That is a mistake. The necessity arises not because we return to our Building, but because we have to leave the premises we are now in, which are worth double what we pay, though quite too small for our purpose, and because we have to resume our normal printing programme. An opinion widely entertained is that the Institution Building is a very expensive luxury and, moreover, that the expense of it is only for the benefit of a small proportion of the members. I shall treat the two points separately. It has been argued that the Institution could have premises at a much lower cost. Table 3 shows the running cost of

TABLE 3.

Annual Cost of Institution Building (1912 *and Estimate for* 1921).

	1912 £	1921 £
Ground rent	2 200	2 200
Rates	1 615 (6·46s. on £5 000)	3 900 (13s. on £6 000)
Insurance	65	140
Repairs	270	400
Lighting and heating	165	750
Cleaning, etc.	115	290
	4 430	7 680
Less rent from tenants	1 700	3 000
	£2 730	£4 680

the Institution Building in 1912 and 1921, and shows how greatly the costs, particularly the rates, have increased; on the other hand, so have the rents paid by the tenants. The figure shown in 1921 is the estimated figure given in the general expenditure (Table 1). But to consider whether this is too much or too little, I have had prepared Table 4, which shows the total cost of the Institution Building, that is, including the interest and sinking fund on all the capital the Institution put in from investments sold or has since borrowed.

TABLE 4.

Table Showing Result if Present Building were Given Up.

Institution Building.

	£
Annual cost including heating, lighting and cleaning	7 680
Interest on whole capital cost	2 745
Sinking fund premiums	280
	10 705
Less rent received	3 000
	£7 705

Alternative.

To rent offices 7 000 ft. at 15s.	5 250
Heating, lighting and cleaning	500
Rent of lecture halls	125
Storage of books	100
	£5 975

The difference is £1 730, and the Institution has to go to other buildings for its meetings and has no library.

Now what is the alternative? If I may act as Devil's advocate, putting the matter as strongly as possible, from the point of those who raise the objection I am dealing with, if we were to-day to take offices (if we could get them, which at present is extremely doubtful) we are advised that we should have to pay in rent £5 250, in heating, rent of other halls such as we are in (again if we can get them), and storage room for the valuable library, a total of £5 975. So that for premises with no library, no lecture theatre, no facilities for gathering other meetings and congresses under our roof, we should save a sum of £1 730, whilst the whole Institution would suffer in prestige. In view of this Table 4, I maintain that £1 730 is all that our Building costs over and above a most meagre accommodation. I may add that if the Institution is satisfied to have no library or hall of its own it can let the accommodation in its Building and would then be at a cheaper rent than it can hire the accommodation anywhere. Personally, I agree that the ideal would be one great engineering home with separate accommodation for the great Institutions and a better load factor on the other facilities of the Building, but this is not a practical question for this or for some years at least.

Another objection raised is the suggestion that London members get more out of the Institution than country members. In Table 5 are set out the items of expenditure previously shown; and where they can be said to appertain to all members, to London members only or country members only, I have so allocated them. Allowing for argument's sake that the whole of the expense of the London Building except the minimum alternative rent is allocated to London members, and two-thirds of the Conversazione and Annual Dinner expense is similarly allocated, we have the result that the actual allocation for the year 1921 for territorial

members exceeds that for London members. In letters in the Press it has been suggested that the Building is used by London members as a club. That is not the case. I wish it were; if it were, the club would be a separate organization and pay a proper rent for the accommodation and have to be self-supporting. A certain number of members have written in the Press, and a few to the Secretary or to Territorial Centres, urging that where members are not within reach of the London or of a Territorial Centre they should pay a lower subscription. The idea seems reasonable; it would of course involve that the subscriptions of the members who are in touch were increased so as to realize the same total sum, but as far as members in Great Britain are concerned the number affected is less than 10 per cent of the membership. Speaking for myself, I consider that, if the grievance be real, an effort should be made to meet it. The great difficulty is to find a formula. Distance *per se* from London or a Centre is not a criterion; it is rather difficulty of access which matters; but if a definite formula can be found and a time comes when the subscriptions can be eased, I think this class of members is one that the Council should earnestly consider.

I must refer to one other matter which has been mentioned in the Press, namely, the question of proxy voting. The Council have issued to every member an open proxy in a form determined by the Articles and informed members that, if they desired to support the Council proposals, the signed proxy sent to the Secretary would be filled in and used by certain members of the Council in support of the proposals. It is suggested that the Council should have nominated some one who would vote against the proposals. This appears to arise from an entire misunderstanding of the position of a proxy. The proxy attends the meeting on behalf of the giver of the proxy paper. He hears what is to be said, he decides whether he will support or not, and he may in fact use the proxy in any way he likes. But for the Council to set someone up who has tied his hands in advance before the matter has been discussed would, at least for the Council, be impossible. He would have no freedom, there might be amendments acceptable to a free proxy holder, but such an official objector would be tied. The electrical Press have given full opportunity for holders of any particular view to get in touch with those of the same view attending the meeting, and I wish here to express my thanks to the Press for the facilities granted. I shall now move the Resolution on the notice, and I shall ask Mr. Esson to second the Resolution; I shall then put it formally to the meeting for discussion. The Honorary Treasurer, Sir James Devonshire, will take note of any questions and will reply thereto at the end of the discussion. I hope members in discussion will confine themselves to the points at issue, and avoid side issues. The issue is simple. Is the Institution to continue its great work? It is doing a great work.

TABLE 5.

Sub-Division of Expenditure incurred for All Members and for London and Territorial Members.

		All Members	London Members	Territorial Members
Estimated Expenditure, 1921.	£	£	£	£
Administration	11 000	11 000	—	—
Institution Building (*less* £3 000 rent from tenants)	4 680	3 130	1 550	—
Interest on Mortgages	1 340	1 340	—	—
Furniture Repairs	50	50	—	—
Journal	7 170	7 170	—	—
Lending Library	50	50	—	—
Science Abstracts	500	500	—	—
Meetings (London)	500	—	500	—
Grants to Territorial Centres	2 750	—	—	2 750
Premiums for Papers	190	190	—	—
Special Grants (Research, British Engineering Standards, etc.)	650	650	—	—
Annual Dinner	100	—	66	34
Conversazione	550	—	367	183
Legal Expenses	250	250	—	—
Miscellaneous Expenses	150	150	—	—
Payments on Capital Account.				
Repayment to Economic Society	915	915	—	—
Sinking Fund Premiums	280	—	280	—
Reference Library	150	—	150	—
	£31 275	£25 395	£2 913	£2 967

Our membership curve (see Fig. A) shows growth equal to that of the premier engineering Institution in the country, a fact which must inspire us all to greater efforts, to a greater pride in the Institution and in its

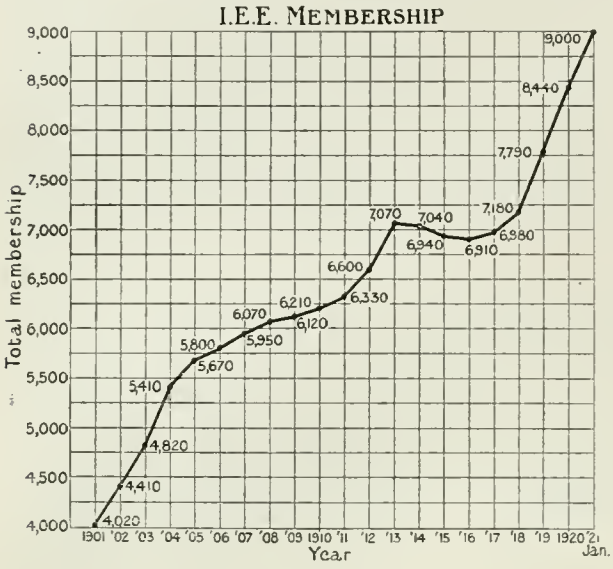


FIG. A.

home. I repeat: Is the Institution to continue its great work? Is the expenditure reasonable? Is the method of meeting the expenditure reasonable? Is there any alternative?

Mr. W. B. Esson: It was only after going into all the figures in another way that I felt it my duty, as a very old member of the Institution, and as an outsider, not being a member of the Council which puts the scheme forward, to come down and support the Council on this occasion. The figures have been put so fully before you that there is no need for me to say much about them. Possibly in the few remarks I am about to make I shall be preaching to the converted; but if there is anybody here with some lingering doubt whether the subscription should be raised, then I think it is my duty to try to convince him that there is no other possible course to adopt if the Institution is to go on in the way that it has been going on during the past 10 years.

[Mr. Esson then analysed the Accounts for 1918 and 1919, and after comparing them with the Budget for 1921, continued:] That leaves us with a deficit of £5 425. Working it out in a different way, the President brought it out at £4 521, so that there is no mistake about it, that the sum is about £5 000, and that money must be found or otherwise the Institution must suffer. I do not say the work of the Institution will come to a full stop, but the prestige of the Institution will be lowered, its activities will shrink, its influence will fade, and generally it will not be anything like the Institution that we have known unless that money is forthcoming. With these remarks I formally second the Resolution.

The President: The Resolution has been moved and seconded, and it is now formally before you. It is now open for discussion.

[At this stage, the President, in response to Mr. F. W. Purse, explained the procedure in connection with amendments to the Resolution.]

Mr. L. Joseph: I rise to oppose the Resolution. It is because we are so proud of the Institution that I am going to oppose the Resolution. There are ever so many members who, in my own experience, cannot pay an increased subscription. I will go *seriatim* through a number of things which we can alter in order to cut down expenditure. We do not want the expense of the Annual Dinner or the Conversazione. I am sure there are any number of powerful bodies that would be only too pleased to hire our Hall. Also, there are ever so many outside people who would be only too pleased to read the books in our Library if they had an opportunity of hiring them out at the price of 2d. or 3d. a volume. With regard to the *Journal*, in one or two countries abroad I have seen the Proceedings of Institutions published in a journal of high standing in the electrical world. I see no reason why that procedure should not be carried out by our Institution. If that were done, the present large expenditure on the *Journal* would immediately disappear. I did not quite understand in the Budget for 1921 the rise in the cost of the Building. Perhaps it will be explained later on. [**The President:** The principal difference is the increase in the rates put on the Building by the rating authority.]

Mr. F. W. Purse: [After taking exception to the manner in which the Council had issued the proxy papers for the meeting and to the President's opening statement not having been circulated before the meeting, Mr. Purse continued:] In opposing the Resolution to-night I do not do so on the score that we do not want some increase of subscription. The question is: What is the minimum amount that we want? Now let us examine the proposals of the Council. They state in their figures in regard to the members that they are going to wipe away all differentiation. The subscription of some Members will go up by 1½ guineas, others by 1 guinea; the subscription of some Associate Members will go up by 12s., of others by £1 2s. 6d; the Associates are going to be bled a little more. The Graduates are to pay an increase of 8s. only, and some of the part-compounded members are going to pay an increase of from 2½ guineas to 4 guineas. No attention at all is paid to entrance fees. Surely something might be done by way of increasing the entrance fees. Furthermore, in regard to the members who have compounded, I think we ought to treat these compositions as the Government did during the war in connection with building contracts. I cannot see why our members who have compounded should not also be called upon for some increase of subscription. Dealing again with the question of expenses, if the *Journal* is retained as at present I calculate that an additional £5 000 per annum should be ample. That is an average of 12s. 6d. additional subscription per member. There is something to be said for an all-round increase, because the increase in the cost of the *Journal* applies to all the members. Why should the burden of that be thrown

upon a few, and not upon the whole of the members? If, however, a proportionate increase to each member is more equitable, then the increases to bring in £5 000 would be in the following order:—The present 4-guinea Member should pay £5, against 5 guineas as suggested by the Council. The 3½-guinea Member should pay £4 10s., as against 5 guineas proposed by the Council. The 3-guinea Associate Member and Associate should pay 3½ guineas, as against £3 15s. proposed by the Council; and the 2½-guinea Associate Member and Associate should pay 3 guineas, as against £3 5s. proposed by the Council. The Graduate, instead of paying the present subscription of 2 guineas, should pay £2 10s., which is the same figure as that suggested by the Council. The 1-guinea Student should pay £1 5s. instead of 1 guinea as suggested by the Council, and the 1½-guinea Student should pay £1 15s. as against 1½ guineas suggested by the Council. Those figures would, I estimate, bring in about the £5 000 I mentioned. Then I should like to say that all the papers that are published in the *Journal* are not of interest to every member. A proposition has been put forward in this way: to do away with the free issue of the *Journal* to all the members; send round the ordinary proceedings and notices of meetings, and then say to every member, "If you want a copy of the paper by Mr. Jones it will be 3s.; if you want a paper of Mr. Smith's it will be 5s." Those members who want copies of the papers will pay for them, and in that way we should make the *Journal* self-supporting, so that we should not spend this £7 000 out of the funds of the Institution.

Mr. A. H. Dykes: When I found that as a Member my subscription was to be increased from 4 guineas to 5 guineas, an increase of 25 per cent, it seemed to me a very reasonable amount, because I knew that the value of the sovereign had fallen from 20s. to 8s. Therefore if we had gone on in exactly the same old way I should have expected my subscription to be increased in the ratio of 8 to 20, less of course a certain reduction owing to the increase in the number of members. But it must not be thought that the expenses go down directly as the number of the members increases. It is true that some of the standing charges become less through an increase in membership, but there are other charges, such as the printing of the *Journal* and so on, which are increased by an increase in the number of members, in the same way that postage and other items of that sort also increase. Therefore it does seem to me that the increase suggested shows that every endeavour has been made to cut down expenses. It is easy to say that one may cut down this and cut down the other, and that we should not have a conversazione and dinner and that sort of thing, but there are certain things which a big Institution like this ought to do and can afford to do; and I venture to think that the increase which is asked for from the different grades of members is not so great that it can be looked upon as a hardship. The increase is a moderate one and is not nearly as great as the increase which has been suggested by some other Institutions. I should like to congratulate and thank the President very heartily indeed for the trouble he has taken in putting the facts before us to-night. I never saw a proposal for an alteration in subscription put forward in such detail and with such care as this has been put before us to-night. I appeal to those members who have come here, and who have possibly received proxies sent by people who have not had an opportunity of considering the matter carefully, that they should not consider the increases as at all exceptional, having regard to the increases that have taken place all round; that they will think over carefully what the President has told us and agree to the proposals of the Council.

Mr. J. E. Kingsbury: I might remind you, sir, that the possibility of the necessity of an increase in subscriptions was hinted at at a somewhat earlier date than you have mentioned. At the Annual General Meeting before last, when I appeared in an official capacity for the last time, I hinted at the possibility of an addition to the subscription being required in order to carry on the work of the Institution. My remarks were unauthorized and unpremeditated. I did not know then what we all know now—that costs have gone up to stay. If I had known that then, I should have been more precise in my observations than I was upon that occasion. When I received the notification of the proposed increase of subscription the idea I had about it in the first instance was not "Is this wrong?" but "Is it enough?" And I confess that I came here to-night with the idea of suggesting to the members that we should carry this Resolution with unanimity. It seems to me that some of those who have spoken to-night have been in the main considering the individual and not the Institution. I would ask them to approach the subject from a different angle, not the individual, but the Institution. Their idea is that the Institution is spending money or doing something on behalf of the individual. That is not so. The theory of this Institution is that individuals combine for the purpose of promoting some object which cannot be attained without that combination. We have had evidence that the Territorial Centres cost so much. I have heard a suggestion to the effect that that is to the benefit of the territorial members. It is nothing of the kind. It is to the benefit of the science and the industry, in order that the experience of the territorial members may be obtained red hot for the benefit of the Institution and the world at large. In a similar way it is said that the Institution Building is a luxury and that it belongs to the London members. I protest strenuously. It is nothing whatever of the kind. The Institution Building is the home of the Institution. If the members would go back, as I do, to the time when they had a longing, as every individual has, for a home of their own they would not talk as if the Building belonged to a part of the Institution and not the whole. The Institution Building is the home of the Institution, and I cannot conceive any member of the Institution desiring that it should be abandoned. I cannot conceive in any respect that this subject can be approached by any member with the idea that the only other alternative which is open to us shall be taken, which is to reduce the activities of the Institution, to minimize its influence and to decrease its prestige. "Breathes there a man with soul so dead"—and so on if I could

remember the quotation. Breathes there a man with soul so dead that he seeks to-day to reduce the Institution to a level which it has always been its object in the past to rise above? I cannot imagine for one moment how the Council can be criticized for the proposition which they have put forward. It is hard, I know. Some of us undoubtedly are unable to afford the increases which are coming along. But can the Council suggest that the subscriptions of all the members shall be reduced to that of the lowest common denominator? It is not reasonable. The Council has full powers to temper the wind to any few shorn lambs there may be amongst the members, and in my experience the Council has always been ready generously to exercise that power. There is one small point upon which perhaps it is my duty to reply to Mr. Purse's observations, but not in detail. He made some comparisons between 1914 and 1919 and he assumed that this matter had been sprung upon the members. I am obliged to call attention to the fact that the reason why it arises now is because in the intervening years of the war we have in the first place received money from the Government; in the second place there was a curtailment of our activities; and in the third place there was an increase in our income. With the cessation of the war we are all anxious to return to our old activities; with the cessation of the Government occupation of our Building our income becomes reduced. Instead of criticizing the Council for what they are now putting before the members, I think of all the criticism they would have laid themselves open to if they had not put such a proposition before us. Imagine what our situation would have been. At the end of next year either one or other of two things would have happened: the curtailment of the Institution's activities with all that that means, or there would have been a deficit. I congratulate the Council on the proposition they have made. It is nothing but a business proposition, and I again appeal to the members to give the Council their support by a unanimous vote.

Mr. J. J. Wolff: We are all anxious that the prestige of the Institution should be maintained and we are willing to pay some increase in subscription. Mr. Purse has suggested an increase which it seems to me is a matter of detail, but he agrees to an increase of subscription. What I feel is this. The Institution is spending as much as £11 000 per annum in administration expenses. We do not know how it is spent; we have no accounts given to us and we do not see any results. I think if the Council could take some means of bringing their work before the members they would probably thoroughly support Mr. Kingsbury's suggestion. I should therefore like to suggest that this matter be deferred to give the Council an opportunity of putting before each member of the Institution what he gets for his money in results. I am sure the Council would be able to make out a very excellent case, and then I am sure the members would thoroughly support Mr. Kingsbury. With regard to the *Journal*, that costs £7 000. A suggestion has been made that a charge should be made to each member. Looking at it from an egotistical point of view, at the present moment what does each member get out of his subscription? It really is the *Journal* and the Conversazione, and I think the latter should not be curtailed. I think the Conversazione every year is a great opportunity for many colleagues to meet each other who would never meet in the ordinary course in any other way at all. With regard to the *Journal*, I certainly think it would be a great mistake to make a charge to the members for it. Every member does not want the whole of the *Journal*, and it seems to me that each member should be asked by the Council: "What subjects are you interested in?" and that certain papers should be sent only to the certain members who want them—free of charge, of course. Those members who want the whole lot should have it. I should like to suggest that the Institution Building should be open until—for the sake of argument—8 p.m., and that tea should be served from 6 to 7 p.m., so that members on leaving their work can go to the Institution, have a cup of tea, talk to their colleagues and read the papers and books that are there. [**The President:** We are advised that if we provided tea except on the nights of the meetings we should have to pay income tax on the whole of our income, and that would be £9 000 a year.] I am obliged to the President for telling us that. That satisfies the members that they cannot have it.

Mr. W. R. Cooper: In bringing forward a proposal of this kind two main ideas underly it, generally speaking. The first idea is that everybody has much more money as a result of the war. But unfortunately it is not the case. The increase will mean a considerable hardship on many of our members who have not got much means. The second idea is that costs will always remain what they are at present. That is entirely contrary to economics. They may not go down to pre-war level, but they are bound to come down. I think it is very undesirable to take a permanent step to deal with a temporary embarrassment. If there is anyone who will second my suggestion I should like to propose this amendment: To insert the words "until 31st December, 1922" after the words "the 1st January, 1921" in the first line of the Resolution. That means that this increase in subscription would hold good for two years, after which the whole matter would be reconsidered.

[Mr. Cooper's amendment was seconded by **Mr. R. H. White.**]

Sir Charles Bright: There are two points that I should like to touch upon. It has often seemed to me there is a good deal of money spent on the *Journal* that is rather wasted. A good many of us think that the *Journal* might be reduced in the same way that the Royal United Service Institution and one or two other Institutions have reduced their *Journals*. The other expenditure which I personally think unsuitable at the present time is on the Conversazione and also on the Dinner. I have nothing to say against the general scheme, except that I think we should go in for economy to meet the many people who are suffering tremendously at the present time.

Mr. W. P. Adams: I think we must all admit that some increase at any rate is imperative. Even the most

adverse critics admit that. I rise not by any means to oppose an increase, although I am one of those who will be hard put to to find the extra subscription, but simply to ask one or two questions with the object of throwing a little more light on the subject. The *Journal* has been touched upon by several speakers, and I should like to add a word on that point. I have here the last printed balance sheet that was issued, and I should like to commend to the attention of the member who spoke in the middle of the room the fact that he can there get a full statement of the management expenses, and I have no doubt that the Secretary will gladly add to the information there given. Taking the figures published in the balance sheet, it appears that a sum of £4 200 is spent on the publication of the *Journal* and *Science Abstracts*. In 1921 we expect those items to cost about £7 000. In addition to that, there is an item of printing under "Management Expenses" of close upon £800. You will therefore see that printing costs a very substantial sum of money. Whether that could be reduced is a point that one or two speakers have mentioned, and I think it might receive attention at the hands of our staff. I suggest that the £7 000 which is foreshadowed, very closely approximates to £1 per member. That does seem to be a very heavy expenditure when you consider that, I suppose, not one member in a hundred attempts to read any large proportion of each issue. The articles published deal with subjects covering an enormous range. Some of them are highly theoretical. The huge expenditure on the *Journal* is a great point and I put it seriously to the Council whether that expenditure might not be materially reduced by the adoption of a certain scheme. I should like to ask: Could we embody a few advertisements in our *Journal* and thus get some of our money back? Then a very good suggestion has been put before us of having some part of the "Proceedings" printed in the electrical Press. The electrical papers would probably be willing to do that and expense might be saved on that head. I hold the view that the Institution should maintain to its utmost every social effort that it is carrying on at the present time, and that it must look forward to doing more for the social welfare not merely of its members but of the whole electrical profession. Those of you who were present when Mr. Atkinson gave us his delightful Presidential Address must have been greatly struck by his reference to social questions and the remarks that were made afterwards. In considering this question of increased subscriptions, is the Council asking for enough to cover possible developments that we may have to consider in the future? I merely throw that out as a hint. We do not want to be niggardly over this matter. Up to a certain extent there must be an increased subscription. Let us pay it cheerfully and not count our blessings one by one as several members have done. Surely we can take a broader view of the influence and value of the Institution in the engineering world. I am quite sure the Council has spent hours over this matter and I am sure that what they suggest is desirable at the present time.

Mr. L. M. Jockel: I was hardly prepared for the excellent case that our esteemed President has made and the excellent way in which he has put forward the case for the Council. I have never yet seen a case in regard to an increase of subscription put in such an excellent and businesslike manner, but there are one or two items which should receive the attention of the younger corporate members. Being a corporate member of three other important Institutions, I naturally compare the accounts of the Institutions, and I am led to the conclusion that the management charges of our own Institution seem to be high when compared with those of the other Institutions, namely, £1·138 per capita. The next item which rather appeals to me is the item in the accounts for the Conversazione and reception of members who served in His Majesty's Forces. The sum spent was £858 8s. 5d. I understand that about 70 per cent of the membership of the Institution consists of provincial and foreign members. Bearing that in mind, it seems to me a very excessive expenditure on behalf of those members of the Institution who served in His Majesty's Forces. Then I should like to refer to the question of the increase of subscriptions from the point of view of the status and registration of the engineer. I think some of the younger members of the Institution would be very gratified to learn this evening or at the next meeting whether there is any prospect of registration. That would mean a great increase in the *esprit de corps* of the younger members. I know that some increase of subscription is necessary, but if we are assured that eventually this matter of increased status and registration will be dealt with in the present session it would be a great benefit to the younger members at any rate.

Mr. A. Cunnington: I hold a proxy from a provincial member who asked me to express his views at this meeting. Although I think his objections have largely been met by the President's opening statement, there does seem to be still, in looking at the case from his point of view, some argument for differentiation between the provincial members and the London members. However accessible the Territorial Centres may be, and whatever advantages my friend in the provinces may get in that way, he cannot use the London building in any way and he cannot get access to the library. May I say one word personally, apart from that point? It is inevitable in a matter like this that there should be a lot of shades of opinion, small varieties of opinion, and it does seem to me very regrettable that the whole of the statement of the case which was so ably put forward by the President at the beginning of the meeting was not in our hands before. It is so absolutely impossible to come to a meeting of this kind and discuss a matter really ably unless every fact is known by us. I should like to suggest, therefore, that the whole matter be referred back to the Council, for this reason—that we do not know whether the points that have been raised by the members have been considered by the Council. They might, for instance, desire to consider a flat rate of increase. That seems to me a desirable thing. I do not see why everyone should not bear a uniform percentage increase. There is little difference between the ability to afford. Why, for instance, should the

Student be differentiated ? The percentage of increase will only be slight, and the Student might be asked to pay for the privileges that he will gain.

Mr. A. H. Walton : I should like to say a few words on behalf of the members abroad. I come across a great many men who are members of this Institution out in various foreign centres, and they all with one accord say the same thing, and that is this : When here they have nowhere to go to to meet their professional friends. Will the Council kindly bear that in mind when they are considering the subscription of the new members ? There ought to be some facilities for these members to meet their friends—I do not mean in a business way, but more or less to meet people in a social way. For instance, they should be able to come here and meet our Secretary and be welcomed and that sort of thing. You may get an odd one here or an odd one there who does it, but it is not general.

The President : I will now ask the Honorary Treasurer to deal with the questions that have been raised in regard to the accounts and the expenditure.

Sir James Devonshire (Hon. Treasurer) : Let me say at once that I am very sympathetic with the view that many members have expressed in regard to the question of the increase of subscriptions. Personally I hate it, but we have to face facts. Mr. Purse agrees that there should be an increase of subscription, but he quarrels with the way in which we have done it. All I can say is that this matter was very carefully considered by the last Council, and it was absolutely affirmed and confirmed by the present Council. I think it would be impertinent for me to say more than that. It has had a very great deal of searching examination by those two bodies of men. Table 1 (page 67) shows that we want £5 000 odd from increased subscriptions to pay our way. You will observe that, taking full credit for all that we can think of, we still have a deficit of £5 400. My estimate, as close as I can make it, of the value of the increased subscriptions is £5 000. Of course it is possible, judging by the results disclosed by the curve of increased membership, that we shall get a somewhat larger income from subscriptions by that increase of membership. There is no suggestion to the contrary at present. The curve is still a rising one, and therefore I venture to predict that the estimated deficit of £5 425 will be just about covered by the increased subscriptions to which we ask you to agree. You may say : " Well, but have not you cut the thing very fine ? " The answer is : " Yes, we have." On that Table we cannot show any surplus to carry to the credit of Reserve. We have enjoyed a surplus in past years and we have invested that money as is shown in the Accounts. This Table does not suggest that we can go on doing that. With regard to Mr. Cooper's amendment which was seconded by Mr. White, I am willing to recommend, provided that the proposals of the Council are adopted, that a proviso should be tacked on to them in this sense, and I will ask Mr. Cooper seriously to consider whether he and Mr. White will withdraw their amendment and in its place accept this proviso which I beg to move. " Provided that the Council shall have power at any time if in the opinion of the Council the financial position of the Institution so allows to prescribe that the respective annual subscriptions or any of them shall be less than those set out above but not less than those respectively prescribed by the Special Resolution confirmed on the 30th May, 1912." That is the last time that the Articles of Association were changed, and they are the ones that it is proposed shall be changed to-night.

Mr. W. R. Cooper : In view of the amendment put forward by the Hon. Treasurer, I shall be very pleased to withdraw the amendment I put forward myself. [**Mr. White** agreed.] I hope you will allow me, sir, in withdrawing the amendment, to express my appreciation of the way in which you have put forward the case for the increase of the subscription. I should like to appeal to all the members here this evening to consider the whole thing broadly and not narrowly ; to consider the welfare of the Institution as a whole and to support the Council. The Council, when all is said and done, know far more of the details than the individual members can possibly do. I have much pleasure in seconding Sir James Devonshire's amendment.

Mr. F. W. Purse : I propose as an amendment an all-round increase of half a guinea in the subscription. I have it all set out in practical form here and I claim the right to hand it in to become the first amendment before the meeting. The increase would be something over £4 000. It would be more than that, because I include the compounded members.

The President : I think you must strike out of the amendment all questions of compounded members, because we are advised that we cannot touch them. You are putting a smaller percentage increase on the older members and a heavier percentage increase on the younger members.

Mr. W. A. Gilbert : I second Mr. Purse's amendment.

The President : I will put the amendment to the meeting, but before doing so I shall make my closing remarks. I want to refer to one or two essential elements. Grave objection has been taken by Mr. Purse to the whole question of proxies. This proxy question, as was pointed out when we brought it into force, is a very difficult one indeed. It is hardly possible to satisfy everybody whatever you do. This is the first occasion on which proxies have been issued by the Institution for a meeting of this kind, and when all is said I think the only real objection that Mr. Purse makes that has anything in it is that we sent out stamped addressed envelopes addressed to the Secretary. That was done with the object of saving members trouble. They could have sent in the proxies to the Secretary in those stamped addressed envelopes filled in with anybody's name. All we said was : " If you send the proxy paper in in blank a member of the Council will use it to support generally the resolution." The next main point I think that Mr. Purse made was that the Council had not issued a full statement. Let me say that practically the whole of the matter that has been shown on the screen in the way of figures relates to answers to objections that have appeared in the Press. We could not in advance answer those objections. I do not in the least grudge the criticisms of any member, though I do think, with all respect to the Press, that

many of the criticisms or questions might well have been sent to the Secretary to be dealt with by the Council and not by way of question and answer through the newspapers. I think the proper function of the Press in the matter would rather be that if anybody has a view to express let him express it in the Press and let him ask other members who like to join with him in any action to get into communication with him, and not that there should be questions and answers through the Press, because the Council cannot take up the line of answering through the Press before the meeting. As regards expenditure, if any efforts of ours could have squeezed the expenditure within the old subscription it would have been done. The Conversazione is perhaps a question which may need consideration as to whether it is desirable that the body as a whole should pay for it. The question of the Dinner is not in the same category. Members who come to the Dinner pay for their own dinner, but it is meet and proper that an Institution like this should invite a certain number of guests. The sum that appears in the accounts for the Dinner is substantially the sum that we pay for our guests. The question has been asked: What do members get for their subscription? The last Annual Report runs into something like 50 of 60 paragraphs, nearly the whole of which deals with the activities and work of the Institution in all kinds of directions. With regard to the opening of the Building in the evening and so on, if the members want it open in the evening there is the Council and they can write to the Secretary. These matters will all come before the Council. But everything that has been said in these directions means further activities, and further activities again mean more expense. They may be worth while having, and, if so, and the members will pay for them, let us have them. But we are in a little difficulty about the Institution and the use of it in any way, if I may use the word, as a club. The income-tax regulations are very strict on this point. All Institutions are having great difficulty in keeping their funds out of the clutches of the income-tax officials, and we have got to be very careful in what we do that we do it all along the lines of the main work of the Institution. Nevertheless, if any suggestions are made of a definite nature the Council will consider them and may in fact take some kind of a postcard referendum of the members as to whether they would like the thing done or not. If they agree, the thing will be done. The same applies to the question raised by Mr. Walton. The Secretary is always at the Institution and only too delighted to welcome any foreign members who come in to see him. He is only too anxious to render all the help he can in Institution matters or in any other direction. That brings me to the end of what I have to say. I want now to explain to you the procedure we have to go through in accordance with the requirements of the laws by which we are governed. In the first place, I have to put to the meeting any amendment to the Resolution, and to take a show of hands. It requires a three-quarters majority to carry any amendment on a show of hands or otherwise. It is competent, after the show of hands vote is taken, for me as President or for any three members to demand a poll. If a poll is demanded, then we have to go through a certain procedure which may take about an hour. If we come to a poll—if it is necessary—I will then indicate to you the procedure we shall have to go through, so as not to confuse the issue of the question at the present moment. In the first instance we shall take a vote on this amendment by a show of hands. I now therefore put to you the amendment which has been proposed and seconded and read by me: "That persons who have not compounded shall pay the following subscriptions," and then follows the half-guinea increase all round. Those in favour of the amendment please hold up their hands.

The amendment was then put and lost, 9 voting for and the bulk of the meeting against.

[Mr. Purse, seconded by Mr. Gilbert and Mr. Rogers, thereupon demanded a poll, but after some discussion, and after the President had stated that some 1 300 proxies had been received in favour of the Council's proposals and not more than 80 against, Mr. Purse withdrew his demand for a poll.]

The President: I shall now put to you the main Resolution, with the addition of the following words after the subscriptions set forth in the Resolution: "Provided that the Council shall have power at any time if in the opinion of the Council the financial position of the Institution so allows to prescribe that the respective annual subscriptions or any of them shall be less than those set out above but not less than those respectively prescribed by the Special-Resolution confirmed on the 30th May, 1912." This has been proposed and seconded. I now put it to the meeting. Will those in favour of that Resolution please hold up their hands?

The Resolution was then put, the bulk of the meeting voting for and 9 against.

The President: I declare the Resolution carried.

The meeting terminated at 8.30 p.m.

SPECIAL GENERAL MEETING, 17 DECEMBER, 1920.

(Held at the Institution of Civil Engineers.)

The President, Mr. Ll. B. Atkinson, took the chair at 6 p.m. Sixty-three members were present.

The Secretary read the notice convening the meeting.

The President: This is a Confirmatory Meeting to confirm the Resolution adopted at the meeting held on 2nd December. I formally move the confirmation of the Resolution adopted at the meeting held on Thursday, December 2nd, 1920.

Mr. S. Evershed: I realize, and have done so for the last few years, that as a member of the Institution I should sooner or later have to pay a bigger subscription. I have much pleasure in seconding the motion.

The President: The Resolution has now been pro-

posed and seconded and it is open to anybody to discuss it, but at the same time I must point out that no question of amendments arises; it is simply a question of confirmation or rejection.

Mr. W. R. Cooper : I hope the Council will not take to heart the many criticisms which were made about the *Journal* at the last meeting. It seems to me they were entirely ill-founded and certainly will not be supported by the general body of members. They indicate a very restricted outlook, and in technical matters it is very important that the outlook should be broad. If the outlook is narrow, men get behind and then discover developments of which they knew nothing. I hope there will be no splitting up of the *Journal* or anything of the kind as a result of the various criticisms which were made.

Mr. L. Joseph : Before I go into the merits of the case, I should like to make a very strong protest against the most inconsiderate and unjust letter which we have received. We have received a letter which demands, if we want to oppose this measure, our presence here. We are bound to come in person at expense and waste of time. Has it occurred to any of you how undemocratically you are acting in this democratic country when you have those who have compounded for life not paying anything? They are not paying anything towards this increase, whereas the member who pays year by year has to shoulder the whole burden. It is unfair and unjust and I hope that members will have the justice to vote decidedly against this atrocious increase at the present moment.

Mr. F. W. Purse : All I wish to say is more in the nature of a personal statement. I do not want to offer any further criticisms on the matter. With your kindness, Mr. President, we had our field day at the last meeting, and I do not think I can add anything more. I honestly believe the proposals are inequitable. We all have our own opinions on money matters, but I believe in majority rule, provided that rule is not a crushing one. I could not put the proposals of the Council down as crushing ones. I say they are inequitable but not crushing, because, as I stated at the last meeting, I recognize that some increase is necessary and it is only a question of how that increase should be dealt with. I am not prone to throw compliments about, but I must pay my tribute to the very genial and patient manner in which the President dealt with me at the last meeting, and also the very kindly interview he gave me before the meeting. No one could have received a more kindly and sympathetic interest and I am very delighted to have met Mr. Atkinson under these conditions, and I am glad to see him occupying the position and steering us through what are very stormy times. I hope, however, we shall get into smooth waters again and continue our happy progress.

Mr. A. Cunnington : May I just say that I am here to-night to protest mildly on behalf of a provincial member who felt that the case had not been clearly put or had not been put in advance of the meeting a fortnight ago? I think his feeling was that it would have been a good deal fairer to the provincial members if an abridged statement such as we heard from the President, which so ably cleared the whole matter up, could have been placed before all the members in advance. It would have afforded an opportunity to provincial members to judge the case in a very much better way than they were able to do under the circumstances.

Mr. A. H. Allen : I think it would be of interest to be informed of the total cost of these two issues of proxies by the Institution.

The President : The Secretary informs me that £55 and £65 are respectively the costs of the issue of the proxy papers. I am pleased, on behalf of the Council, at the words which Mr. Purse has just addressed to us, because, after all, I think in all these things what we want to have is, as far as possible, not necessarily unanimity of opinion but at least a feeling that the whole matter has been thrashed out and a solution has been reached which is in accord at least with the wishes of the great majority of the members, and we hope that those whose views have not been accepted will, for the time being at all events, regard the matter as settled. With regard to Mr. Cunnington's criticism, I may say that more than half the members in the country are attached to Territorial Centres, that figures were placed in the hands of the Chairmen of the Centres, and that every Centre called a meeting at which the matter was put before them. It is only those at outlying stations who can be said to have any grievance in that respect. I think it is a thing which should be noted, to see if we cannot in future put in the hands of those members some brief statement in advance. I repeat what I said at the last meeting, that a great deal of the matter which was discussed or brought up in the shape of figures could not have gone out in advance, because it arose out of criticisms in the Press on individual points, of which the Council had no knowledge at the time the notice went out. I will now formally put the Resolution, which has been proposed and seconded, for adopting the Resolution passed on 2nd December. Will those in favour please hold up their hands?

The motion was carried with two dissentients.

The President : I declare the Resolution carried, and there being no other matter before the meeting I declare the meeting closed.

JIEE 59 (1920/1)

INAUGURAL ADDRESS TO THE WIRELESS SECTION

By W. H. Eccles, D.Sc., Chairman of the Section.

(*Address delivered* 24 *November*, 1920.)

I propose to devote the main part of this address to a review of the work done by the Wireless Section of the Institution in putting on record the development during the war of the practice and theory of wireless telegraphy and telephony, but first of all I think this occasion offers a suitable opportunity for saying a few words concerning the formation of the Section.

Many of the members present will remember that in the year 1916 a movement was started for the purpose of founding a professional Society or Institution of Wireless Engineers somewhat on the lines of the American Institute of Radio Engineers. This latter Institute had already scored a great success in the valuable work of bringing together many of the best minds in America for the oral or written debate of wireless problems, and had established and conducted an admirable *Journal*. The Institute had obviously stimulated and assisted, both by its meetings and by its *Journal*, the fruitful exchange of views and information among wireless engineers. The lack of a corresponding organization in this country was very marked, and the end of the war emphasized the deficiency. The progress of wireless during the war was so great that all existing books on the subject in this country were out of date. Other nations were beginning to publish accounts of the work done by their Signalling Services and, indeed, some of the developments and discoveries made in this country began to appear in foreign journals. Meanwhile those who were considering the possibility of forming a new professional Institution of Wireless Telegraphy were confronted by an array of financial difficulties.

At this juncture Mr. C. H. Wordingham, C.B.E., then President of the Institution, ever on the look-out for chances of doing good deeds, suggested that the Institution should form a Wireless Section for obtaining, reading, and discussing papers which might afterwards be printed in the Institution *Journal*. Thereupon the fears which I have just mentioned—printer's and publisher's bills, rent of premises, salaries of staff, etc.—disappeared. The chief flaw in the proposal appeared to arise from the fact that the papers read before the Wireless Section would be mixed up in the *Journal* with papers on heavy-current engineering and would therefore be less easily referred to in the bound volumes; but this did not weigh greatly with those who had heard that a new wireless journal was being planned wherein all the specialized matter that every student of wireless wishes to keep together for reference would be concentrated. I speak of the *Radio Review* which has since made such a splendid position for itself under the able guidance of Professor G. W. O. Howe and Mr. P. R. Coursey. In addition, I think I may say that the Institution will probably shortly see its way to arranging its *Journal* so that the wireless portion can be bound separately by those members who so desire.

I think that the Wireless Section will not feel satisfied unless they can see that they are able to give some return to the Institution for the great material benefits they receive from it and the ready-made prestige which they enjoy. Shall we as a Section flatter ourselves unduly if we claim to have done the Institution some service by bringing into its Proceedings many important papers which might otherwise never have reached it, and that in some ways the Section has contributed to the prestige of the whole Institution? We can, I think, safely claim that a considerable number of new members have been enrolled as a consequence of the formation of the Section. Thus the step initiated by Mr. Wordingham may already be said to have been amply justified. Much of the success is due to the strong and unfailing backing which the Section has received from Mr. Roger Smith during his term of office as President; that happy state of affairs promises to continue under our new President, Mr. Atkinson.

That the formation of the Wireless Section has been appreciated by all those interested in wireless communication, can be seen from the records of attendance at the meetings. The number of persons who signed the Attendance Book was 1 297, the average attendance being 130.

Two lectures and eight papers have been given. I believe that some of these papers might never have been written had it not been for the persuasion directed upon the authors by the Committee of the Section. These papers have all been of such a high order of merit that the Council of the Institution have been —if I may tell such secrets—greatly embarrassed in distributing the prizes or premiums awarded to the authors of papers of outstanding merit. In this matter of premiums the Institution has been most generous; the Council has decided to give three premiums to papers read before the Wireless Section, the first of these being named the Duddell Premium of value £20; the others are of value £10 each. An extra premium was given last Session. The Council have also drawn up during the past Session rules for the government of the Section and the election of its Committee, thereby giving to the Section a very generous measure of autonomy.

During the past Session the Committee of the Section have endeavoured to secure that the principal advances made in wireless communication during the war should be described by the men who played so large a part in making them. From the material thus collected I

propose to make a sort of stock-taking—to set forth briefly some of the more striking improvements achieved.

In 1913 the musical spark method of transmission was still in use at some of the largest stations in the world; the arc, the timed spark, and various forms of alternator with and without frequency changers were, however, formidable rivals. Signals from spark stations were received by crystal detectors or by Fleming valves, or by audions with two electrodes; continuous-wave signals by the ticker and its cousin the tone-wheel. The continuous-wave circuits of those days did not show outstanding advantages over spark circuits. It is true that the heterodyne system of reception—in which the received signal is combined with a local oscillation of almost the same frequency so that the relative motion of two currents is observed—had been introduced by Fessenden long before 1913, and that he had used arcs and alternators at the receiving stations for generating the oscillations destined to beat with the incoming signal oscillations; but this heterodyne method was cumbersome in practice and lacked many of the features which have made the modern methods so successful. It is true, again, that de Forest's audion with three electrodes and the Lieben Reisz vacuum tube were also becoming known, but both of these tubes were used only as amplifiers, singly or in cascade, and they were equally useful with both spark and arc signals. Certainly at this date there seemed no imminent likelihood of continuous-wave telegraphy completely ousting the spark in long-range work.

But during the year 1913, directly after the art of amplification had been grasped, the possibility of generating oscillations even of very high frequency was seen, and then the heterodyne method of reception of signals sprang on to a different plane. For this method of generating feeble oscillations for combining with the signal oscillations was so simple, perfect and manageable that it realized the highest ideals of the wireless engineer. But this is not all; the three-electrode vacuum-tube oscillator magnifies the signals while it generates the oscillations. For the oscillator must of necessity possess positive retro-action between the anode and the grid circuits, that is, the back coupling from the anode to the grid is such that any electrical movement in the anode circuit produces one in the grid circuit whereby the original movement is reinforced and, therefore, any stimulus applied in the circuits is magnified. In achieving this the three-electrode tube did not lose the rectifying property of the Fleming valve. It retained this unimpaired or, indeed, in an enhanced degree. Now rectification is necessary before the beats produced by the combination of the received and local oscillations can give an audible effect in a telephone receiver. It was thus found possible in 1913 (by Captain H. J. Round in this country) to generate local oscillations, to magnify the received signals and to rectify the combined result all with one tube, which process is often called the autodyne method of reception. This happy combination of qualities was so especially favourable to continuous-wave wireless telegraphy that this received an immense impetus over the spark method for long-distance work.

Such was the situation in this country at the beginning of the war. Of the progress made since that date we can find a fairly complete chronicle in the lectures and papers read before this Section. It will be found that progress has not been so great with regard to the transmitting plant as with regard to the apparatus for reception, though a paper by Mr. M. Latour has given us a glimpse of recent developments regarding some of the alternators being installed in long-range stations.

As regards progress in methods and apparatus for reception and in small transmitting sets, we obtained a very perfect account from Colonel A. G. T. Cusins who dealt with designs evolved for use in the Army, and from Major Erskine Murray who dealt with the designs of the Air Force. In addition to these Major C. E. Prince* dealt with wireless telephony on aeroplanes and Captain Eckersley† with duplex wireless telephony. A complete theoretical and practical account of amplifiers as developed for use in the Navy was given by Professor Fortescue, and that remarkable instrument the oscillatory valve relay was described by its inventor, Captain L. B. Turner.‡ All these papers may be regarded as records of finished achievements complete in themselves, but certain of the other papers are in the nature of unfinished symphonies, and these I shall refer to shortly.

On the whole the outstanding feature of this development is the three-electrode thermionic tube sometimes called the pliotron or the audion or the three-electrode valve, but which I call a triode in order to avoid offending any of the partisans. This instrument is in somewhat the same stage of development as the dynamo-electric machine was 40 or 50 years ago. Evidently, in order that it may be developed to the limit of its possibilities, its characteristics must be studied, endless measurements must be made and its theory built up. As in the case of the dynamo and motor, theory and practice must proceed together—we may almost say "useless each without the other." The design of these tubes for reception and many of their properties were discussed in a paper by Mr. B. S. Gossling§ and valuable data concerning the filaments were communicated to the *Journal* of the Institution by Mr. G. Stead‖. Mr. Gossling's paper covered an immense range dealing with several years of work in the Service laboratories. I do not propose to say anything in the way of repetition, but should like to call attention to some of the inquiries provoked.

I may put first certain results not yet fully discussed which arise in the use of these tubes even in respect of such a fundamental parameter as the voltage factor. The voltage factor is the ratio of the free electromotive force appearing in the anode circuit in response to that which is applied in the grid circuit. In certain modes of using the tube, for instance in the tubes occurring in the later stages of a many-stage amplifier, the value of the voltage factor realized is disappointing with certain adjustments. I have had measurements

* *Journal I.E.E.*, 1920, vol. 58, p. 377.
† *Ibid.*, 1920, vol. 58, p. 555.
‡ *Ibid.*, Supplement to vol. 57, p. 50.
§ *Ibid.*, 1920, vol. 58, p. 670.
‖ *Ibid.*, 1920, vol. 58, p. 107.

made during the past year or two by many students on many tubes in which a rather large change of voltage was applied to the grid. The method of measurement is a very simple and handy one when these conditions are permissible.

In Fig. 1 the anode is connected in series with a battery E, an instrument I, a resistance R, and the filament. The grid is connected to a switch through any battery (not shown) as may be desired. The resistance R is preferably of dial pattern. The operations consist, first, in giving R any convenient value, say R_1, while the grid circuit switch is on the point marked 1, and, second, in putting the switch to 2 and adjusting the resistance to bring the anode current to its previous value. Let this second value of the resistance be R_2, then the voltage factor is

$$(R_2 - R_1)/R_1$$

For quick measurement it is very convenient to make

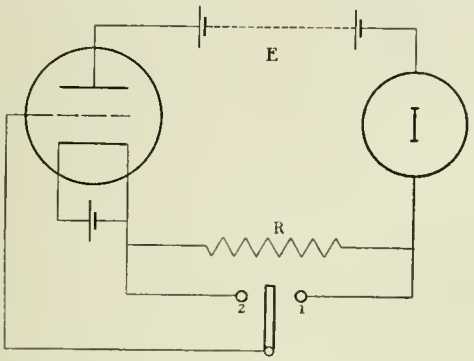


Fig. 1.—Connections for determination of voltage factor.

$R = 1\,000$, then the final reading of the resistance box minus 1 000 gives the voltage factor by the movement of a decimal point.

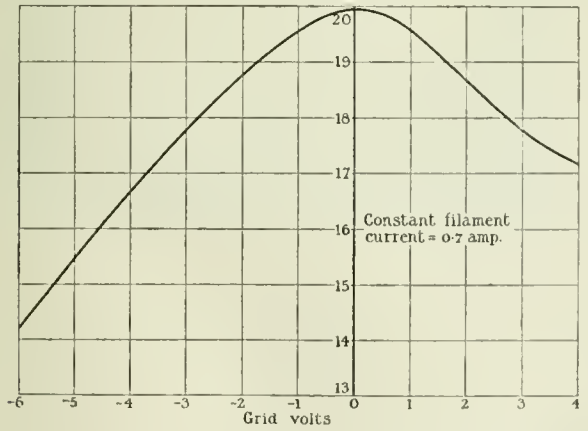


Fig. 2.—Variation of voltage factor with grid voltage.

The same apparatus can be applied to the measurement of the internal resistance of the triode by keeping the switch on the point marked 1. The operations consist in observing the current when $R = 0$, and, secondly, in observing the changed value of the current when R is any convenient value. If S be the resistance of the shunted galvanometer and i_1 and i_2 the observed currents, the internal resistance of the tube is

$$\frac{Ri_2}{i_1 - i_2} - S$$

A quick way of using this method is to read i_1, take 10/11ths, and adjust the resistance to bring the current

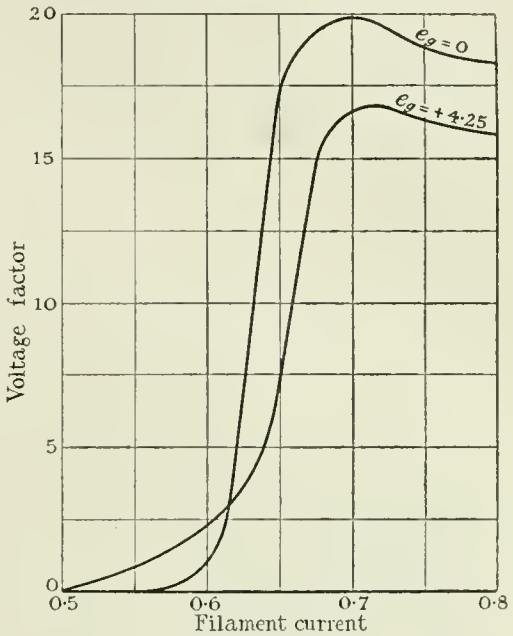


Fig. 3.—Variation of voltage factor with filament current.

reading to this value; then, neglecting S, the internal resistance is 10 times that in the resistance box.

The results of measurements made by this method

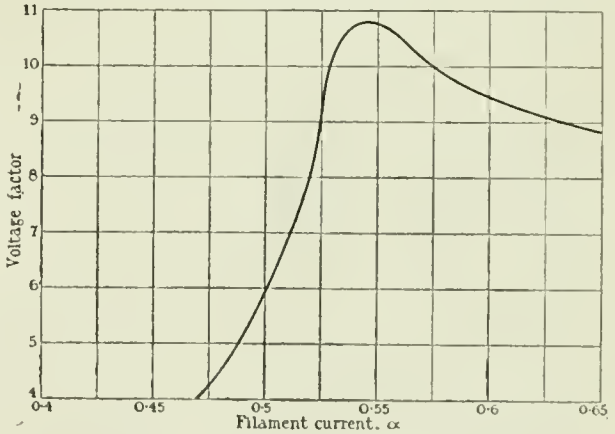


Fig. 4.—Variation of voltage factor with filament current.

are shown in Fig. 2, in which the ordinates give the voltage factors and the abscissæ are the voltages of the battery permanently connected in the grid circuit, the filament current being constant.

Another set of results showing how the voltage factor

varies with the filament current is given in Fig. 3, and for an entirely different tube in Fig. 4. These measurements have been made by the following students at Finsbury Technical College:—Messrs. C. W. Cosgrove, C. A. Wilck, J. L. Hewitt, W. Gwynne, R. L. Dawson, and E. M. Walden.

An interesting series of curves appears in Fig. 5, the abscissæ being the volts on the grid and the ordinates the voltage factor. The filament current is constant on each curve. These curves were made by Messrs. Everett and Newman. It will be seen that the voltage factor undergoes great variation when these tubes are used for magnifying rather larger changes of voltage than are applied to them in ordinary wireless telegraph experiments. The reason for the variation of the voltage factor arises out of the unevenness of the temperature of the filament to some extent, but chiefly

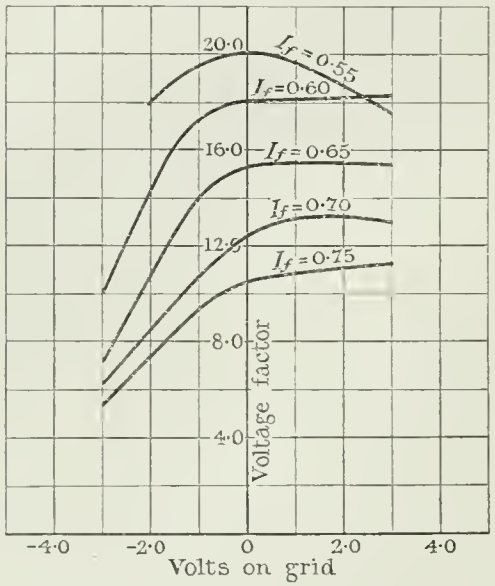


Fig. 5.—Variation of voltage factor with volts on grid, at various filament currents. Anode volts constant = 50.

out of the drop of voltage along the filament. For instance, if the grid be connected to the negative terminal of the filament the grid is 5 volts below the positive end of the filament in potential. Therefore, when a rather large negative voltage of, say, 2 to 4 volts is applied in making the measurements or in actual use, this end may be put out of action, with consequent lowering of the average voltage factor utilized. Again, if the filament current be increased the difference of potential between the grid and the positive end of the filament also increases, with the result that the average voltage factor may again be lowered. Further, if the filament current be considerably reduced the ends of the filament may become saturated and the effective voltage factor again reduced. I am calling attention to these matters because they lead to apparent anomalies in the behaviour of triodes applied to other purposes than the reception of faint wireless signals.

Variation of the average value of the voltage factor as the filament current is altered (as shown in Figs. 3 and 4) bears a striking likeness to the curves depicting the results of some measurements made by Dr. Vincent in 1918 on the change of the wave-length of the oscillations generated by a triode under certain conditions. Perhaps the changes usually occurring in the wave-length generated as the filament battery runs down owe their origin to variations in the average value of the voltage factor. By availing oneself of the flat part of the curves the filament current may be set so that the inevitable slight running down of the filament current will for a long time produce no change in the wave-length. This matter of constancy of wave-length will become of growing importance as the world becomes crowded with wireless stations; in fact, the crowding of wave-lengths is continually receiving international attention now. Methods of transmitting within very narrow ranges of wave-length may have to be devised and, in consequence, receiving antennæ and apparatus with sharp tuning may become essential. There is another reason for this latter tendency, namely, the fact that sharp tuning and low losses go together. Important problems will then arise concerning the increased capital expenditure for building low-resistance antennæ and apparatus, and the interest and depreciation will have to be compared with a possible saving

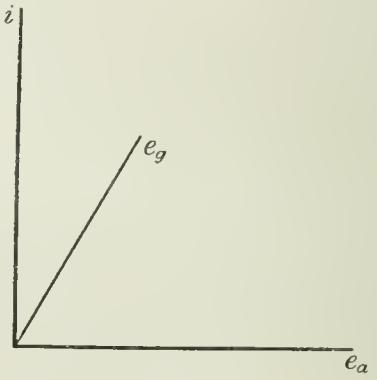


Fig. 6.

of energy and the value of the other advantages of precise tuning. This reminds me that a paper read before the Section by Professor G. W. O. Howe on the high-frequency resistance of wires* has an important bearing on these problems.

Among other insufficiently investigated properties of the triode are those associated with certain portions of the characteristic surfaces which may be used to represent all the principal properties of the tube. When the filament current and voltage are kept constant there remain four principal variables, namely, the anode current i_a, the grid current i_g, the anode voltage e_a, and the grid voltage e_g. If we represent anode current and grid current separately, each of these must be regarded as a function of the two independent variables e_a and e_g.

Fig. 6 shows three lines at right angles; the horizontal line marked e_a is the axis of anode voltage; that marked e_g is also horizontal and is drawn per-

* *Journal I.E.E.*, 1920, vol. 58, p. 152.

pendicularly through the page away from the reader to represent grid voltages. The line marked i is vertical and is used to represent either grid or anode current. Upon these axes we may erect surfaces to represent the currents flowing in a triode at any assigned values of the two voltages, by measuring off to scale these voltages along the appropriate axes and erecting at the point so determined a vertical with two ordinates equal in value to the observed currents.

When this is done we obtain for the anode current such a surface as is shown in Fig. 7. The flat top represents the saturation values, the concave sloping surface at the right-hand side follows in the ideal case the three-halves power law, and normally only a small part of this area on the extreme right hand of the picture is utilized. On the left hand where the surface descends in a kind of cliff to the axis of the grid voltage we have a region little investigated and scarcely utilized as yet. This surface was constructed from curves such as those of Fig. 8, which are, in fact, sections of the surface made by vertical planes parallel to the axis of anode voltage and represent what happens to the anode current, at various constant grid voltages, as the anode voltage is increased.

It will be noticed that at low anode voltages the anode current rises very rapidly. In the next figure (Fig. 9), which represents a model of a grid current surface, it is seen that at low anode voltages the grid current falls rapidly as the anode voltage is increased from zero. On the left hand of the sharp ridge are represented values of grid current when the anode voltage is negative, that is to say, when practically no anode current is flowing. The concave sloping part obeys the three-halves power law and the flat

Fig. 7.

top represents saturation. Fig. 10 shows shapes of the grid characteristics from which such a surface as that of Fig. 9 is prepared. All the familiar characteristics seen in books and papers can be obtained from these surfaces by making sections.

The rapid rise of anode current and accompanying fall of grid current in the region of low anode voltages requires explanation. In Fig. 11 the diagram on the left indicates possible paths of electrons when the grid is slightly positive and the anode is at zero voltage, A being the anode, G a grid wire in section, and F the cathode. Electrons starting from the cathode with considerable normal velocity may perform wide orbits round the grid wire but are finally drawn into it. But if the anode is slightly positive many of the electrons not passing too near the grid wire end their career on the anode. It may be shown that the grid current has the value

$$i_g = A'(e_a + \nu e_g)^{3/2} \exp\left\{ - \alpha \sqrt{(e_a/e_g)} \right\}$$

and that the anode current is given by

$$\log (1 + i_a/i_g) = \alpha \sqrt{(e_a/e_g)}$$

at low voltages, where ν is the voltage factor and A' and α are constants that are different for each tube.

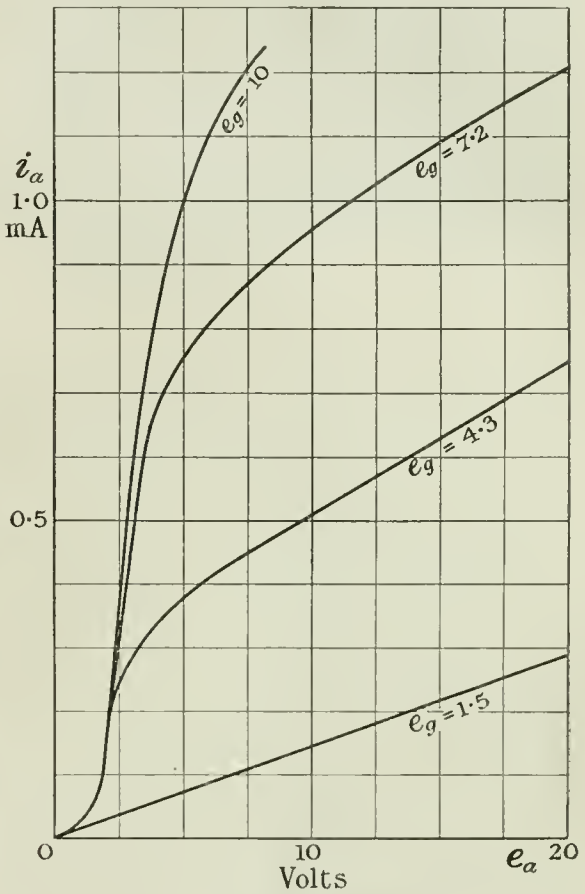


Fig. 8.—Variation of anode current with anode voltage, the grid voltage being constant.

For ordinary reception triodes α has the value 1·3 approximately.

These considerations call attention to a fact that does not appear to have received adequate notice, namely, that the grid in a triode may exert two distinct types of action. The type just described, which is characterized by having the bulk of the grid current contributed by electrons that have suffered great lateral deflections, may be called a snatching action; the other type of action, which is that usually utilized in triodes, is of the nature of an acceleration or deceleration of the motion of the electrons from the cathode

to the anode. In brief, one type of action by the grid is mainly a lateral attraction of electrons, and the other is mainly a longitudinal effect in the direction of motion. The steep surfaces near the axes of grid voltage in Figs. 7 and 9 are due to the snatching action.

In Fig. 12 is shown a surface representing by its ordinates both the grid and anode currents of an

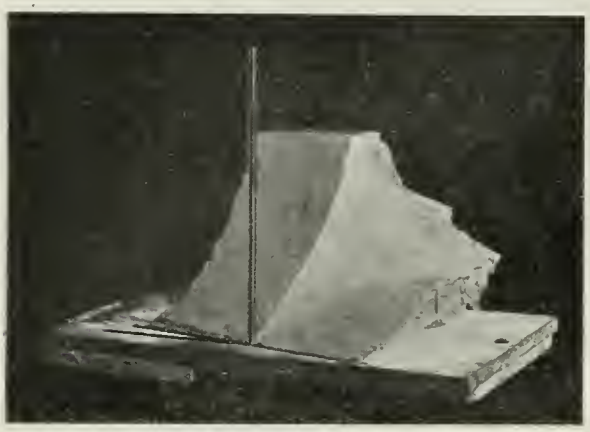


Fig. 9.

ordinary form of triode. It is seen that the snatching action that goes on between the grid and the anode at low anode voltages produces a gully separating the two surfaces. The question immediately arises: What is there beyond the region embraced by this figure?

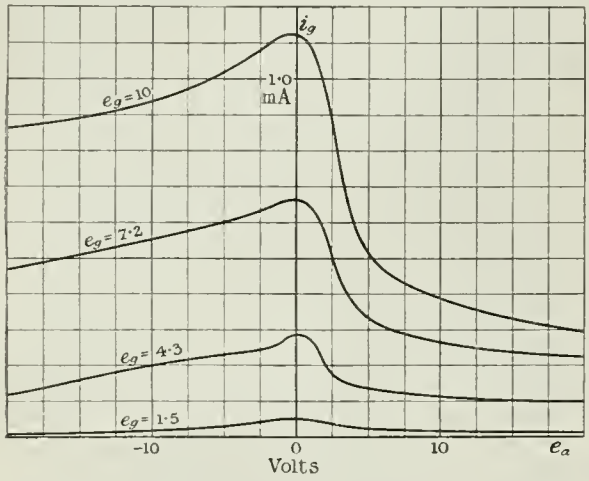


Fig. 10.—Grid characteristics.

Very little published information is available for answering this question, but during the past 18 months I have had parties of students studying the geography of the gully and the plateaux seen in Fig. 12. In particular I have to mention the work of Mr. L. Grinstead and Mr. T. W. Price in this connection.

As a result I am able to illustrate in Fig. 13 a surface representing most of the properties of ordinary triodes. The gully noticed in the previous figure is seen to take a sharp turn. On the extreme right of the model a section taken at a fixed and fairly high anode voltage is seen, and this section is therefore the characteristic curve of the grid and anode currents as the grid voltage varies. On following the curve of section from front to back the anode current is seen to rise from zero to a maximum as the grid voltage rises, to remain nearly constant till the gully is reached, and then to descend suddenly; during this descent the grid current rises and becomes greater than the anode current. The plateau beyond the gully belongs to the grid current. This is not strictly what happens in an ordinary reception triode, but certain types of tube give such a surface with fair exactitude. A form of triode described by

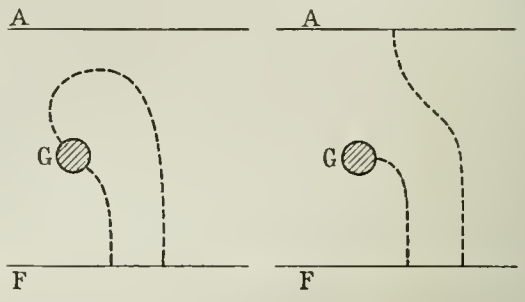


Fig. 11.—Possible paths of electrons.

Major Erskine Murray gives a characteristic very like that shown by this section of the model.

It should be mentioned that these models relate to somewhat ideal tubes. In actual tubes, for the reasons already stated, the edges seen in the surfaces are rounded off considerably.

Let us now turn to another of the papers read before

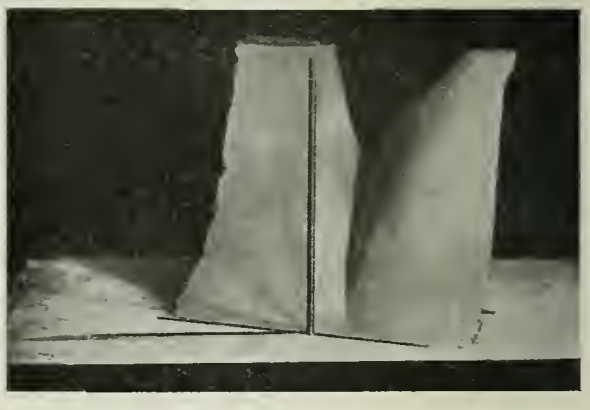


Fig. 12.

this Section which was vastly provocative and stimulating, namely that by Captain H. J. Round on Directive Telegraphy. The perfecting of high-power amplifiers had the important result of making possible the directive reception of distant stations by small antennæ, such as simple frames or crossed aerials with the radiogoniometer. For instance, it became possible to receive transatlantic signals in England with a small frame aerial a yard square. The direction of the sending station can be determined by rotating the frame, or the radiogoniometer coil, about a vertical axis until

the signal intensity is zero. But it was soon found that false directions were obtained during sunset and sunrise, the errors often ranging over 30 degrees of arc. Throughout the night, too, false directions are given erratically and, moreover, the sharpness of the minimum is often spoiled. This has of course disadvantageous consequences in respect of duplex working utilizing balanced aerials.

Captain P. P. Eckersley has suggested that much of the directional error might arise from the rotation of the plane of polarization of the signal-bearing waves by reflection at non-level portions of the Heaviside layer between the sending and receiving stations. The whole matter provokes inquiry if only because the phenomena afford another mode of gaining knowledge about the upper atmosphere.

I am going to suggest another reason than that just stated for the production of waves with rotated planes or polarization, but to show the errors caused by such rotation, however produced, I have here a model of a receiving frame aerial movable about a vertical axis and a ray of electromagnetic radiation incident

Fig. 13.

upon the frame. The ray is represented by a straight rod, and this is fixed at an inclination to the horizontal, because it is abundantly proved that rays bending round the globe follow a curved trajectory resembling that of a projectile; Captain Eckersley, for example, has shown that a horizontal loop can receive signals. In the daytime these rays have their magnetic force horizontal. In this model the magnetic force of the wave is represented by a wooden bar fixed perpendicularly to the end of the rod representing the ray; and when the rod is pushed so that its end and the bar touch the frame, this latter assumes the position in which an actual frame aerial must be placed to receive no signals, that is, so as to have none of the magnetic force of the waves linking with it. But now suppose that at night the plane of polarization is rotated in any manner; we mimic that by rotating the rod and its perpendicular bar while keeping both in contact with the vertical frame aerial. It is observed that the frame must rotate also. That is to say, the apparent direction of the source of the radiation, namely, the perpendicular to the frame aerial, is erroneous. It is easily seen that if γ is the error, α the inclination of the ray to the horizontal, and β the rotation of the plane of polarization

$$\tan \gamma = \sin \alpha \tan \beta$$

Thus if it be true that the trajectory of the ray always lies in a vertical plane through both stations, the error in azimuth enables us to compute the angle at which the ray descends to the receiving station.

But there still remains the problem of the cause of the rotation of the plane of polarization. Those who have thought about the behaviour of an ionized atmosphere towards electric waves passing through it, will see at once that the earth's magnetic field will introduce obliquity into the motion of ions propelled by the electric force of the waves. Whether the obliquity so introduced can be of appreciable magnitude is decided by an easy calculation. I have made this calculation on the assumption that the waves traversing the upper ionized atmosphere have an electric field of the order 1 volt per kilometre. Waves passing over any particular ion evidently impart to it a to and fro motion of the same period as that of the waves, but lagging behind the electric force of the waves by about a quarter period on account of inertia.

Now the ion when moving in the earth's magnetic field is deflected by that field and so acquires an oblique component of velocity. The question is: What magnitude of magnetic field is required to produce an oblique component of velocity of the same order of magnitude as the vertical component produced by the waves? On taking the case of an oxygen ion I find that a magnetic field of 1 700 C.G.S. units is required; for a hydrogen atom 100 units, for an electron 0·06 unit. Now the earth's magnetic field at the ground has a horizontal component of about 0·18 unit. Therefore if I have made no mistake in the arithmetic (I hope I have not omitted the velocity of light from the calculation) we conclude that a rotation such as is demanded by the observed facts of directive wireless telegraphy could be furnished by the passage of the waves through a space containing free electrons.

The next step is to see how this motion of the electrons affects reception. In the first place, the setting of the electrons in motion absorbs energy from the waves and in fact may extinguish them. But every vibrating electron is itself a small Hertzian oscillator and the accumulated effect of the simultaneous oscillation of a whole cloud of electrons may produce an effect at the receiver—the cloud is, in fact, lighted up by the waves and then the cloud acts as if it were a source. The plane of polarization of this secondary radiation is the same as that of the motion of the electrons. In these latitudes the vertical electric force of wireless waves will, in conjunction with the earth's magnetic field, cause an electronic vibration in a line descending from the north-east point to the south-west point. Hence the radiation from a cloud of such electrons will arrive at a receiving station due north of the cloud with the magnetic field dipping from west to east, at a station due south with dip also west to east, and at stations due east and due west with dip from south to north. These dipping magnetic fields at the receiv-

ing aerial will cause errors of direction as already explained; and if part of the primary waves from the sending station get through as well, the minimum will be ill-marked—in fact the simultaneous reception of the primary waves and the secondary waves in time quadrature will be equivalent to reception of elliptically polarized waves. Leaving aside the primary wave, which does not necessarily cause any directional error, we see that waves travelling from the south through a high layer of free electrons should give rise to a westerly error if the above conclusions are correct; waves from the north give also a westerly error; waves from the east and from the west give southerly errors. It ought to be possible to test these conclusions by observations.

The velocity gained by an electron in waves of the same field intensity is greater the lower the frequency. Thus long waves should give greater directional errors than short waves. It has been recorded by Captain Round that the marking and spacing waves of Horsea arc station have been known to give at night direction readings differing by 30 degrees; but unfortunately it was not recorded whether the result on the longer wave or on the shorter was further from the true direction. Again it is to be remarked that if a station were to transmit a group of slightly differing wave-lengths, a kind of spectrum analysis could be performed by aid of a rotating vertical loop or radiogoniometer. Since the imposition of a musical tone upon continuous waves is equivalent mathematically to the introduction of extra frequencies, this "spectrum analysis" could possibly yield tonal effects.

The information gained from observations made with directive aerials must be combined with the knowledge furnished by the analysis of the records of magnetic variations and magnetic storms. Professor S. Chapman* read a paper before the Institution setting forth the conclusions he drew from such an analysis. Magnetic storms, he considered, were mainly due to currents in the auroral layers, and these never come lower than about 100 km. Below this is a layer about 50 km. thick in which flow the currents causing much of the diurnal variation of the magnetic elements. The conductivity of this layer is produced by the sun's action, but remains throughout the night. Magneticians do not require any other conducting layers in the atmosphere, and this is in sharp contrast with the requirements of the wireless observer; for the enormous difference between day and night signals absolutely demands diurnal ionization and de-ionization of some of the upper layers. Professor Chapman suggested that the auroral layer was conducting chiefly because of the presence of α-particles that had come from the sun; I think that wireless investigators would suggest that the layer beneath the auroral layer is occupied by electrons that have come as β rays from the sun. The atmospheric pressure at above 50 km. height is only about a millionth of an atmosphere; the mean free path of the electrons is therefore long and they may possibly remain permanently free in large numbers.

It would seem that the base of this region charged with free electrons must be regarded as the ceiling usually known as the Heaviside layer. And consequently the diurnal variations of the range of wireless signals must be due to the kind of ionization of gases familiar in laboratory work with X rays and the γ rays from radium. In fact I suggest that this diurnally ionized layer below about 50 km. is ionized by the γ rays and other ultra-violet rays from the sun, and involves the splitting of electrons from molecules to make positive ions; and at the gas pressures in fairly low levels many of the electrons will combine with neutral molecules to form negative ions. Both positive and negative ions of molecular size give rise to ionic refraction and the arched trajectory of the Hertzian rays, and incidentally to absorption also. Professor Watson's solution of the daylight propagation problem takes account at least partially of such a layer of conducting gas, and from his result we may deduce that if the ionization is effective down to 25 km. the conductivity need not be a quarter as great as that of sea water in order to ensure results such as are summarized in the Austin-Cohen empirical formula.

Dr. Graham Bell, whom we are so glad to welcome here to-night, and who has addressed us so invigoratingly, has told us that this Section of the Institution represents to him the advance guard of the British electrical world. It is noteworthy that wider public interest attaches to wireless telegraphy than to any other branch of electrical engineering. There are, for instance, innumerable amateur societies for the study of wireless telegraphy, but none so far as I know for the study of the Wheatstone bridge. What is the reason of this? Is there romance in holding communion with persons and places one may never see, and in receiving messages that have come over the wastes of the globe or even across the frozen pole? Or does the lure reside in the enormous miscellany of physical phenomena and principles that is called upon to do direct service in wireless transmission and reception? Whatever the fascination is due to, it seems to me to be the expression of a true instinct that in our subject we live nearer to the growing edge of electrical science than in other branches of engineering. There is infinite scope for knowledge, discovery, invention, and imagination; and I am delighted to know that Dr. Graham Bell is also of that opinion.

And along some of the avenues of our particular technology we may catch glimpses of the radiant expanses of the dawning electrical age. The thermionic vacuum tube in itself contains for the engineer with vision some of the most resplendent of these vistas. For from the glowing filament the most subtile and potent of all the gases is distilled, namely the gas made up of pure electrons, a gas we cannot at present harness as easily as steam is harnessed, but which must take its turn as a servant of man. And in those future days strange engines of giant strength, utilizing electrostatic forces and thermodynamic cycles perhaps as yet unimagined, will be fed with electrons urged by millions of volts along vacuous conduits, and will be presided over by matterless switches. And the people of that age will look back and wonder why we in the twilight of the age of steam were so long content to urge slow floods of electricity through the crowded atoms of copper cables.

* *Journal I.E.E.*, Supplement to vol. 57, p. 209.

THE DISTRIBUTION OF ELECTRICITY.

By W. B. Woodhouse, Member.

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Summary.

The principal conditions of economic distribution are determined by conflicting technical and financial considerations.

Certain approximations are shown to be possible which assist in deciding the principal technical conditions.

The necessity for several working pressures is shown.

A general expression is obtained for capital expenditure on mains and substations.

The effect of load factor on losses is considered and a formula established for general use.

The cost of supply is analysed and appropriate expressions obtained.

The approximations reached enable Kelvin's law to be applied to decide economical current densities and pressure-drop.

A consideration of expenditure on a power distribution network indicates a method of arriving at the economical number and size of substations.

The special considerations accompanying the use of high-pressure cables and of overhead lines are enumerated, and the method of arriving at the economical working pressures for various parts of the system is shown.

Introductory.

The economical development of an electricity supply system may be divided into two stages: first the development from a number of isolated nuclei, next the association of these into a compound organism. This second step is engaging the attention of engineers in all industrial countries at the present time.

The necessity for economy and increased production brought about by the war, and the general demand for improved conditions of life and health, require not only fuel economy and cheap power but the redistribution of the industrial population over a wider area and, in our own country, the abolition of smoke.

To meet these requirements not only must the undertakings supplying dense industrial centres be united, but a supply of electricity must be made available in districts chosen for new developments of industry where no demands exist to-day.

The technical and financial considerations involved in the problem of electricity supply over a wide area are so numerous as to make it a matter of some difficulty to decide the principal conditions to which such a scheme should conform.

Certain principles may be stated and certain approximations made in the preliminary calculations, which tend to eliminate some of the conflicting considerations and to simplify the problem, and it is with these objects that the present paper is written.

The general problem is to provide on a national scale, with reasonable security and satisfactory service, a supply of electricity to all classes of consumers at a minimum cost.

Conditions peculiar to each country will modify the solution, but as the demand for electricity is merely an expression of human activities, the needs of one area of supply will be a guide to those of all others, and the method of solution will not vary.

Area of Supply.

The first consideration is that of the area of supply, which will determine the total amount of power to be provided and the maximum distance of transmission that may be necessary.

In any area of supply physical conditions and social requirements will be such that the density of demand will vary considerably from place to place, and the area may include districts which, either on account of their remoteness from the main sources of supply or owing to the paucity of demand in them or in intervening districts, it would be economical to deal with separately.

Separation such as this sacrifices the considerable advantage arising from diversity of incidence of maximum demand, and the aim of every scheme should be, not subdivision, but the association of existing undertakings throughout the widest possible area.

Areas which it may be economically necessary to regard as isolated in the first instance, may, in the course of time, develop so that interconnection is justifiable; this ultimate development of the scheme should always be borne in mind.

The Time Element.

The economic development of any such system must take into account the factor of time in relation to capital expenditure.

Development, human and industrial, is a matter of gradual growth, and consequently the consideration of a complete scheme without regard to the stages of growth may lead to false economic conclusions.

This is particularly probable in the business of electricity supply because the annual revenue rarely exceeds one-fourth of the capital employed, and a large expenditure in anticipation of future requirements may, by the burden imposed on the business, outweigh the ultimate advantage.

In short, one must consider not only the ultimate

development but also the expedients by which the desired result is to be obtained without financial disaster.

On these grounds the development of a national distribution system from existing industrial areas and the introduction from time to time, as the need arises, of higher and higher working pressures, are more likely to be economically justifiable than the immediate adoption of the highest pressure necessary for the country as a whole.

A step-by-step development, it will be seen, is consistent with economy, both ultimately and during the early period of development.

It should be noted in passing that throughout this paper the assumption is made that a nominally constant-pressure three-phase alternating-current system of supply is adopted, and that each change of pressure referred to above will involve transformation.

Sources of Supply.

The area of supply and the approximate total demand being known, it is necessary to consider the sources of supply before the consideration of distribution can be proceeded with further.

The principal sources of power will be waterfalls and coal; if the former the power-station sites will be chosen with less regard to considerations of distribution for two reasons: the locality of the fall is fixed, and the value of energy losses in distribution may be very low in the early years of development. As a consequence the economical pressures and currents in the various parts of the system may differ considerably from those of a system supplied by coal-fired stations.

If coal be the source of power no separate consideration of such matters as fuel economy, generation costs, or transmission costs can determine the solution of the problem without reference to the final test of cost to the consumer. This remark is felt to be necessary because the economic solution in any case is the result of balancing conflicting considerations.

The unit costs of constructing and operating power stations tend to decrease with the size of the stations, and it does not appear that there is a superior economical limit to the size of a power station. On the other hand the transmission of electricity is, from an economic point of view, not a virtue but an expensive necessity.

The concentration of generating plant in a small number of stations involves greater expense in distribution, and is only justifiable so far as the advantages of centralization outweigh the extra costs of distribution which result.

Apart from limitations imposed by the circumstances of the site, the economical limit to the size of a coal-fired station is determined by a condition external to the station, namely, the density of load in the area to be supplied.

Considering a wide area, physical and social conditions will lead to the supply from a number of stations so interconnected as to provide mutual assistance and to obtain the advantages of diversity of demand.

As a consequence, provision must be made for the transfer of energy from one centre to another from time to time in addition to the normal needs of distribution.

A number of interconnected stations being assumed, the problem of distribution is much simplified because the transmission mains may be utilized to provide additional feeding points on their routes between stations.

The adoption of a system of interconnected stations, it should be noted, abolishes the necessity of placing the stations in the centre of areas of heavy demands, as the transmission mains may be drawn from at the most convenient points for distribution wherever the stations may be situated.

Distribution of Load.

The general scheme of distribution may now be considered. The load or demand on the system will be irregularly distributed, concentrated in large towns and along valleys, main roads and railways, and arising in large and regularly distributed amounts as in a coal-field, in large and closely grouped amounts as in a factory district, or in small individual amounts in other cases.

A statement of the number of users and of their average demand is insufficient to enable the main technical considerations of distribution to be determined, and it is advisable to consider consumers in classes according to the nature and amount of their use.

Broadly a division into three classes is sufficient, such as:—

(1) Domestic and small power users	0–50 kVA.
(2) Factories, mines and workshops	50–2 000 kVA.
(3) Users over	2 000 kVA.

Consideration must be given principally to the industrial areas of the country in which the bulk of the demand will arise; the supply to other areas will not substantially affect the decision as to the situation of the stations or the manner of distribution to the industrial areas.

Pressures of Supply.

The next consideration will be that of the pressures of supply to be adopted.

For a national system of the magnitude required for this country it is economically necessary to employ three or more pressures: the first, that required for detailed distribution to small users. The limitations imposed by the nature and use of the consuming apparatus such as lamps, heaters, cookers and small motors, are considerable, and pressures of from 100 to 250 volts have been established for this class of use.

These detail distribution pressures are so generally established in most countries that they must be accepted and the distribution scheme based on the local standard. Were it not so the decision would be a matter affecting the problem in an important degree on account of the large proportion of the total demand on the system which arises from small users.

The second pressure is that required for power distribution to factories, workshops and detail distribution centres. The economical pressure for this class of user will depend on the number of and demand from each

factory, the density of load in each locality or sub-area, the size of motor in general use, and the nature of the industries supplied.

If, as in some districts, a large number of power users of from 10 kVA to 100 kVA are found, as well as a class of larger power users, it may be economical to introduce two power distribution pressures ; for example, the 2 000 volts and 10 000 volts which the author has adopted in Yorkshire.

The justification for a pressure intermediate between the main power distribution pressure and the detail distribution pressure, it is of interest to note, may arise in districts of totally different character ; for example, a straggling village and a dense office quarter.

A usual limitation to the main power distribution pressure has been in the past the maximum safe pressure of generation, say 11 000 volts ; the advantage of supplying direct to the distributing system without transformation is apparent.

The third pressure is that required for the interconnection of generating stations and for that form of distribution which is usually called transmission ; it will be decided primarily from a consideration of distance and number of feeding points, but will be subject to definite practical limits imposed by the nature of the mains.

The relationship of the three or more systems indicated above raises a number of interesting questions as to their method of protection.

There will be a large number of separate and distinct low-pressure systems, each consisting principally of radial and branch mains fed from a central transformer station without any provision for automatic disconnection beyond overload devices.

In the power distribution system the importance of the individual consumer calls for the provision of alternative mains so interconnected that the supply may not be interrupted by the breakdown of any one of them.

The extent of this interconnection or network will depend on the nature of the transmission system ; the limiting condition would be the complete interconnection of the power-distribution system throughout the whole area with protection such as is afforded by the Merz-Price or kindred device. In such a case, however, it would seem inadvisable to interconnect the higher-pressure transmission mains of the system because of the complications which might arise. The solution most favoured is the division of the power distribution network into a number of sections each of 20 000 kVA or more, and fed from an interconnected transmission system.

It may be noted here that the progress of engineering has generally passed through three phases : first the use of faulty apparatus, next the addition of elaborate protective devices and, finally, improved apparatus and the simplification of protective devices. (*Quis custodes custodiet* applies to mechanism as well as to men.)

The transmission system is therefore the most likely to be completely interconnected and fully protected ; first, because of the advantage of economy and security it gives to the generating stations, and next, because of its comparative simplicity.

Capital Expenditure.

The introduction of these various pressures of supply calls for transformation of pressure at a number of points and for a large expenditure on switchgear and transformers and substations, which must be taken into account when considering the compromise between expenditure and efficiency which represents the maximum economy.

The costs involved in laying down a system such as has been indicated above include :—

(1) Transmission mains connecting generating stations and main feeding points working at pressures of 30 000 volts and over. *System A.*
(2) Substations transforming from the pressure of system A to that of the power distribution networks.
(3) Networks of power distribution mains supplying power users' installations and detail distributing substations at working pressures not exceeding 20 000 volts. *System B.*
(4) Substations transforming to consumers' pressures.
(5) Groups of detail distributing mains at working pressures up to 500 volts. *System C.*

On this expenditure will depend the investment charges which form so important a part of the cost of supply.

Each transformation and each interconnection must produce advantages which justify the expenditure, or the result will be an increase in the cost to the consumer.

A little consideration will show that a general expression for capital expenditure on the various parts of the distribution system will take a simple form ; some part of the expenditure will vary with the working pressure, some part with the output or carrying capacity, and the remainder will be independent of either. For example, the cost of providing a definite length of underground cable for the distribution of electricity may be divided into three parts :—

(1) A part independent of the cross section of the conductors and of the working pressure, being principally the cost of excavating trenches, laying and jointing the cable, and reinstating the surface of the ground.
(2) A part proportional to the cross section of the conductors, being principally the cost of copper, insulation, sheathing, armouring, and the initial manufacturing costs.
(3) A part dependent on the working pressure, being the extra cost of insulation and the increment of sheathing and armouring costs consequent on its increased thickness. This cost is some function of the volume of insulation, which is dependent on the maximum potential gradient permissible. For practical purposes it may be assumed that the cost is proportional to the square of the pressure.

The cost per mile of a cable *in situ* may therefore be expressed as :—

$$A_c + B_c \text{ (area of core)} + C_c(kV)^2$$

where kV is the working pressure in kilovolts and A, B and C are constants.

An expression of this form agrees very closely with actual figures; for example, the cost of three-core cables designed for working pressures of from 6 600 to 66,000 volts have been analysed and are plotted in Fig. 1 for comparison. Those for 6 600 to 22 000 volts are based on costs in the year 1907; those for higher pressures are based on estimates of present-day costs. The calculation of the most economical current density is much facilitated by an expression of this kind, the increment of cost with section of conductor and not merely the increment of cost of copper being balanced against the cost of losses to obtain a correct result.

The cost of overhead lines may be analysed in a similar way, though with such apparatus the increment

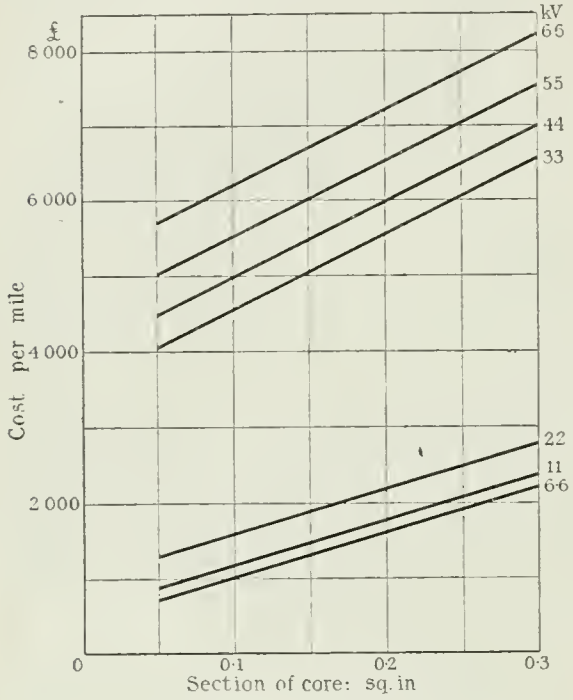

Fig. 1.—Cost of cables in ground.

of cost with section of conductor is much more nearly that of the metal used than in the case of cables. Within the limits of a particular type of construction the increment of cost with pressure is less rapid than in the case of underground cables, and the expression of cost per mile would more accurately be expressed in the form

$$A_0 + B_0 \times \text{area} + C_0(kV) + D_0(kV)^2$$

The choice of pressure for an overhead line depends, however, on considerations of the nature of the country to be crossed and of reactance and pressure regulation, which have greater weight in the final decision than when making the choice for underground cables; and bearing this in mind it is sufficiently accurate for a first approximation, when considering pressures between 6 000 volts and 60 000 volts, to assume the cost of an overhead line as proportional to the pressure and to neglect the last term of the above expression.

The cost of a transforming substation depends on the number and output of the transformers, on the type and number of mains brought into the substation, on the primary and secondary pressures and on the maximum power in the supplying system.

The total cost may be analysed into three parts, the initial cost, independent of output and pressure, a part proportional to output, and a part proportional to the square of the pressure, assuming that the primary pressure and the maximum power vary together. For any pressure the cost of transformers of the same type follows very closely a straight-line law of the form

$$A_t + B_t(kVA)$$

For different pressures a third term is added proportional to the square of the pressure as in the case of cables.

The cost of primary switchgear depends on the total "power behind the switch," on the number of switches, and on the pressure.

The equation of cost for the substation as a whole takes the form

$$\text{Cost} = A_s + B_s(kVA) + C_s(kV)^2$$

From the form of these equations of cost it is seen that the capital expenditure per kilovolt-ampere varies with the number of substations.

Load Factor and Losses.

The determination of the economical current density in distributing mains and of the economical pressure of supply involves a consideration of the total losses in the distributing system during the period of time taken as representative.

The losses in the distributing system may be subdivided into:—

(1) Resistance or copper losses.
(2) Magnetic or iron losses.
(3) Dielectric or leakage losses.

The magnetic and dielectric losses are approximately constant; the resistance losses vary with the load.

The variation of the load is not fully defined by the expression "load factor," the time rate of change must also be known to enable the resistance losses to be estimated.

The limiting conditions of load variation for any particular load factor and for the same maximum load are:—

(1) Sustained maximum load for a fraction of the time equal to the load factor.
(2) Momentary maximum load and sustained fractional load equivalent to load factor.

Between these limits the load may vary in an infinite number of ways, in which the losses are less than those in case (1) and greater than those in case (2).

Expressing the losses at full constant load (unity

load factor) as 100 per cent, the losses for any load factor produced as in case (1) are proportional to the load factor, and in case (2) proportional to the square of the load factor.

Fig. 2 shows the variation of losses with load factor in these limiting cases; it will be seen that for load

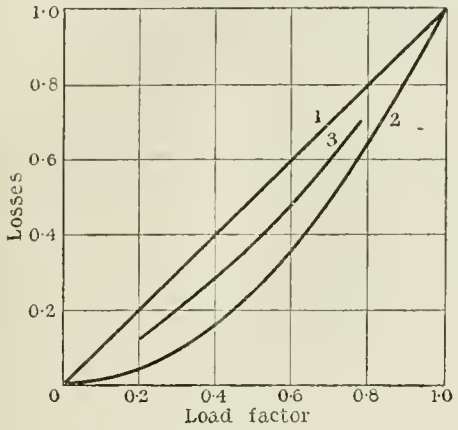

Fig. 2.—Variation of losses with load factor.

factors of from 0·25 to 0·6 such as arise in practice, there is a considerable difference in the amount of the losses.

To simplify the calculation of these losses it is necessary to consider the time variation of load or shape of load curve likely to be met in practice. The comparison of load curves is facilitated by re-plotting the ordinates representing load (and proportional to the current) in order of descending magnitude (see Fig. 3).

If this be done with characteristic load curves of different supply undertakings it is found that there is a

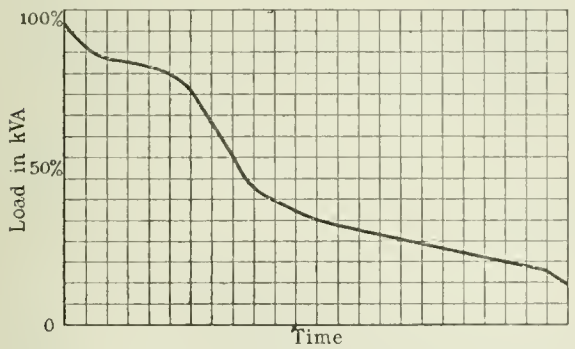

Fig. 3.—Graded load curve.

general approximation to a common form, from which a general expression can be derived connecting load factor and losses with a sufficient degree of accuracy.

That this is likely to be the case might be predicted from a consideration of the nature of the demand for electricity at any moment. The working hours and habits of the people: factory hours, traction "rush times," lighting and domestic demands, holidays and workdays, day and night, winter and summer, all account for demands whose incidence can be forecasted with some degree of accuracy.

The general form of load curve arrived at may be further analysed into parts by a time division. Taking a year as the working period it may be divided into either (1) two equal parts representing day and night, or (2) three equal parts representing factory hours, week nights and week-ends.

For each of these periods the boundary of the load curve may with a sufficient degree of accuracy be represented by a straight line, and the resultant load curves will be of the forms shown in Figs. 4 and 5. Taking Fig. 4, in which the period of time represented by the base line OT is that of the complete cycle of variation and OM = MT, as the approximate form and measuring

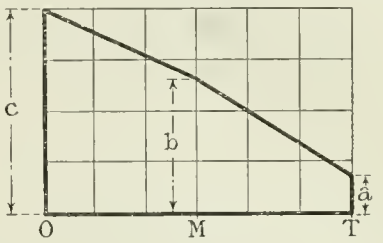

Fig. 4.

the ordinates a, b, and c it will be seen that the load factor (F) is:—

$$F = \tfrac{1}{4}(a + 2b + 1)$$

the maximum load c to be taken as unity and a and b expressed as fractions of this.

Alternatively, if the "factory hour" form of curve be preferred (Fig. 5) the expression for load factor becomes

$$F_2 = \tfrac{1}{6}(a + 2b_1 + 2b_2 + 1)$$

In any general consideration, then, it is sufficient for the purpose of estimating the load factor to know the maximum load, ordinate c, the half-time load (b), and the minimum load (a).

From these the load factor may be calculated or,

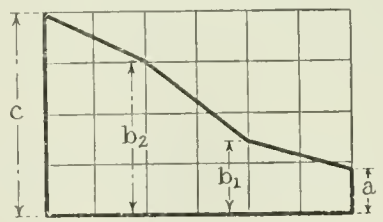

Fig. 5.

if preferred, a chart made, such as Fig. 6, from which it may be read direct.

Having thus simplified the load curve the losses corresponding to any variable load of this character may be readily estimated. The copper losses corresponding to any variable load are $K\int_0^T I^2 . dt$; they may be expressed most conveniently as a fraction of the losses in transmitting the maximum load continuously (i.e. at unity load factor).

It follows that if the load curve be of the form assumed in Fig. 4 the losses as a fraction of maximum load losses are:

$$\tfrac{1}{6}(1 + 2b^2 + b + ab + a^2)$$

This expression may be used in considering the variation of losses with the form of the load curve.

For our present purpose a still more general expression is sufficient.

It will be found by calculation of typical cases from the above expression for losses that approximately the relationship between the load factor of such a system as is considered and the losses is

$$L = \tfrac{1}{2}(F + F^2)$$

where L = losses as a fraction of the losses at sustained full load, and F = load factor as a fraction of unity.

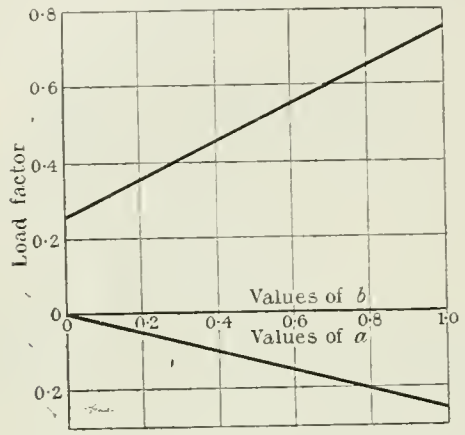

Fig. 6.—Chart for ascertaining load factor.

Note.—The load factor for any values of a and b is found by addition.

The graph of this is set out in Fig. 2 (3) where it may be compared with the relative losses under the limiting conditions referred to above.

The above expression may be regarded as the load factor of the losses (F_1) in terms of the load factor of the load (F) i.e.

$$F_1 = \tfrac{1}{2}(F + F^2)$$

Cost of Energy.

The cost of providing a supply of electricity for a consumer may be divided into three parts:

Production or working charges.
Administration or management charges.
Investment or capital charges.

Taking as the basis the amount of power for which provision is made, the investment charges are proportional to the size of the consumer's installation, the management charges largely independent of this, and the working charges proportional to the demand and to the quantity used.

The cost of generating energy may be expressed as made up of a charge per kilovolt-ampere per annum and a charge per kilowatt-hour delivered.

Let the annual charge per kilovolt-ampere be S pounds.
Let the charge per kilowatt-hour be b pence.

Then at load factor F_1 the cost per kVA-year is equal to

$$(S + 36 \cdot 5bF_1)$$

The cost of energy after transformation or transmission is not accurately expressed in this form but may be expressed as

$$S_1(1 + c) + 36 \cdot 5b_1F_1$$

where c is a term dependent on the expenditure independent of demand, and b_1 the cost per kilowatt-hour after transformation.*

This equation, it should be noted, is only true within the limits of a particular type of construction and a particular class of supply, but by adopting values of S, b and c appropriate to the particular class of user under consideration, equations of similar form will express the cost with a close degree of accuracy.

Using the appropriate expression for the case under consideration, the annual cost of losses in mains is found by inserting the values of the constants and the calculated value of F_1.

The losses in the system as a whole include the magnetic and dielectric losses referred to above, but for a first approximation these may be regarded as constant and allowed for in fixing the constants in the equation of cost.

The efficiency of a transformer varies directly with the size of the transformer and inversely as the pressure, but within the limits of size and pressure likely to be considered this variation is not great. The copper losses in transformers do, however, vary with the load and are subject to the same consideration as those in the mains.

When the problem of determining the most efficient arrangement of a system resolves itself into a consideration of whether to transform or not, then the relative losses in mains and transformers become of importance and the analysis must be extended.

Diversity of incidence of demand must also be taken into account; in other words, the load factor of each section of the system must be evaluated separately and diversity allowed for in their combined demand on the stations.

Kelvin's Law.

The general expression obtained for losses at various load factors and of the cost of mains permits an application of Kelvin's law to determine the most economical current density, and clears the way for a preliminary determination of the economic working pressure which otherwise could only be arrived at by a tedious process of trial and error.

Kelvin's law of the most economical size of conductor reads:—

"If the capital outlay on conductors varied in strict proportion to weight of metal, then the most economical size of conductor would be that for which the annual cost of interest and depreciation equalled the annual cost of energy wasted."

* W. B. Woodhouse: "The Cost of Electric Supply," *Electrical Times*, 1919, vol. 56, p. 159.

The extra cost involved by an increased size of conductor forming part of a cable laid in the ground, or of an overhead line, does vary in strict proportion to the weight of metal, though in the case of the cables the increment of cost is several times that of the metal.

The capital charges on this increment of cost must be balanced against the cost of the energy losses.

The calculation of the annual cost of energy wasted having been made in the manner indicated above, the expression for economical size may be arrived at:—

Let P = cost of losses in pounds per annum per kilovolt-ampere of maximum load,

Q = annual capital charges on the increment cost of a mile of cable per square inch of conductor,

a = section of each conductor in square inches.

Then for the case of a three-core cable the annual cost of losses per mile

$$= 0{\cdot}000128\, PI^2/a$$
$$= Qa$$

From which the current density

$$\frac{I}{a} = 88{\cdot}4\sqrt{\frac{Q}{P}}$$

The ratio I/a is the economical current density and it follows that, considering copper losses only, this is independent of the size of conductor and of the pressure of working.

To facilitate calculations a curve may be plotted from the equations set out above connecting the value of P with the system load-factor F.

The conclusion that there is an economic current density for any ratio Q/P implies a constant pressure-drop per mile.

In a three-phase system the pressure-drop per mile due to resistance is proportional to I/a, and therefore to $\sqrt{(Q/P)}$. For copper conductors the pressure-drop per mile is approximately $E_r = 6{\cdot}23\sqrt{\frac{Q}{P}}$.

Networks.

The simplest condition of distribution of energy over an area is that in which the load is uniformly distributed, infinitely subdivided, and supplied by a conducting sheet spread over the whole area.

If the area supplied is circular and the feeding point is situated at the centre, and the current density be assumed constant throughout, then if R be the extreme radius the cross section of conductor at any lesser radius r is proportional to $R^2 - r^2$.

From this it follows that the total weight of conductor is equal to that required, with the same current density, to supply the total load concentrated at a point $\frac{2}{3}R$ from the feeding point. This hypothetical case is of value as a standard of comparison with the weight of conductor employed in any distributing network.

When we come to consider the economic current density in an actual distributing network, a number of considerations arise which tend to modify the decision.

First, as has been pointed out above, the cost of energy losses is increased by the effect of the capital charges on transmission mains and principal substations. Next, the load factor is reduced below that of the main system by the division of the load into groups supplied by separate networks; and, finally, the cost of mains to supply a load of definite amount will depend considerably on the number of substations.

As an example, the simple case of a rectangular network may be considered, the load per unit area p being assumed equally distributed between N substations. Assume that the area A is square, with sides R miles long, and that the distance between substations is D miles.

Consider the relationship between the number of substations and the other items affecting cost. The load per substation by definition will equal

$$\frac{pA}{N} = pD^2$$

The cost of substations will vary with the number and size, the total cost decreasing with the number.

The cost of mains will vary with the route length and with the weight of copper.

The route length of mains will be

$$L = \frac{(R - D)2R}{D} = 2R(\sqrt{N} - 1)$$

The total weight of copper in the network is proportional to the length of each section, the number of sections, and the average load per section, or to

$$\frac{(N - 1)pAR}{2N}$$

As the number of substations is increased this expression approaches a limiting value in which the weight of copper is equal to that necessary to supply the total load concentrated at a point distant from the feeding point by half the length of a side of the square, or $\frac{1}{2}R$.

It must not be overlooked that to increase the distance between substations involves a corresponding additional expenditure on mains from the substation to the consumers; considering only the copper in the networks the variation of weight with the number of substations is not great, and the decision as to the most economical number is practically independent of the item of weight of copper.

The compromise is determined by:—

(1) Number of consumers for whom separate substation is justifiable.
(2) Total amount of such load.
(3) Number of small consumers.
(4) Amount of their load.
(5) Cost of principal substation.

The economical pressure is therefore proportional to the pressure-drop permissible and to the amount of load.

In practice, networks are not rectangular and uniform, but the equations for the hypothetical case are of assistance in indicating the governing conditions.

If the time element be taken into account with prospective developments, the original network may, as it grows, be subdivided, and the distribution pressure chosen may in some intervening period be less than the economical pressure.

Before proceeding to the final consideration of working pressures, regard must be had to the limitations imposed by the nature of the apparatus employed. This is of particular interest in the case of cables working at the higher pressures.

High-pressure Cables.

The minimum section of conductor is determined by two considerations, that of maximum permissible dielectric stress and that of minimum mass to withstand overheating under transient fault or short-circuit conditions on the system. Assuming the use of paper-insulated cables of the usual form of construction, each core being made of a close laid strand, and the insulation being ungraded, a minimum section of 0·05 square inch appears necessary for pressures above 10 000 volts.

On the score of overheating under short-circuit conditions a calculation may be made, from the known specific heat and resistance of the conductor, of the current density which will melt it in a given time, it being assumed that no heat is carried away in the brief interval of time during which the short-circuit is maintained.

The maximum section of conductor and the consequent overall diameter of the cable are limited by considerations of transport and handling, such as the maximum diameter of cable drum and the liability to crack the insulation by bending the cable.

An increase of the working pressure for which a cable is designed involves an increased thickness of dielectric, which not only reduces the permissible current density in the conductor on account of its heat-insulating properties, but the higher pressure involves increased dielectric losses.

As a result the permissible current density must be reduced. The amount of this reduction is a matter of interest on which further information from makers of cables would be of value.

The permissible current density in a single-core cable designed for any particular pressure is approximately proportional to the circumference of the core, or the square root of the area, and therefore must be reduced as the area of the conductor is increased.

This is approximately true for three-core cables; for example, the safe rating of an 11 000-volt cable laid direct in the ground is approximately

$$I = 742\sqrt{a} - 37$$

where a has any value between 0·05 and 0·3 square inch.

The dielectric losses will reduce this rating in cables designed for higher pressures by an amount which depends on the design of the cables. The dielectric losses are proportional to the product of the working pressure and the charging current; and the charging current at any frequency is proportional to the pressure and the capacity, so that in any particular cable the losses will vary as the square of the pressure.

If it be possible to design cables for different working pressures so that the maximum dielectric stress is the same in each case, then the variation of capacity will follow a recognized law and the capacity will be less in cables designed for higher pressures.

It would follow, therefore, that the dielectric loss would increase less rapidly than the square of the pressure, but until further data are available the exact ratio is somewhat indefinite.

From the data available it would seem that three-core cables of the same type of construction may be obtainable for working pressures up to 60000 volts, and that the present methods of construction limit the size of conductor to about 0·25 square inch section for the highest pressure.

If it be necessary to consider the use of cables at a still higher pressure, a single-core cable, being free from some of the limitations of three-core cables, may be adopted.

Cost per kVA-Mile.

The cost per kilovolt-ampere transmitted by a cable per mile of route, or shortly the cost per kVA-mile,

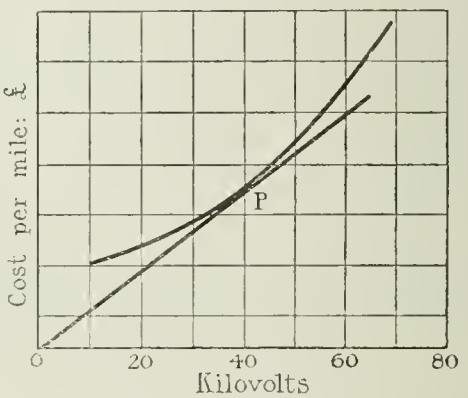

Fig. 7.—Cost per mile of cable at different pressures.

may be derived from the expression for the cost of cable per mile and that for the economical current density.

On the assumption of equal current densities for all pressures there is a pressure corresponding to a minimum cost per kVA-mile.

This pressure depends on the section of conductor and increases with it; for any particular section the minimum cost is reached when the part of the total cost per mile which varies with the pressure is one-half of the total cost.

For example, in Fig. 7 the cost per mile of a cable is plotted against rated pressure. The tangent (OP) to the curve from the point O will meet the curve at a point P whose abscissa is the pressure corresponding to minimum cost.

Actually, however, the current density must be reduced as the rated pressure is increased, and the economical pressure is correspondingly less.

A further consideration arises here, namely, the increased cost of transformers and switchgear as the pressure is increased.

The proportionate cost of the transforming and controlling apparatus when added to that of the cables will still further reduce the economical pressure.

It may be found that the economical pressure is less than that required for satisfactory regulation, in which case the latter must be taken ; if, however, the contrary be the case, other practical limitations may decide the maximum.

Overhead Lines.

The cost of overhead lines per kilovolt-ampere transmitted is not likely to increase as rapidly as the working pressure, and therefore on the score of capital expenditure there is no economical limit to the pressure which may be used.

In such case the limit will be imposed by a consideration of :—

(1) Minimum size of conductor required for mechanical strength.
(2) Subdivision of circuits necessary for security of supply.
(3) Limitations imposed by capacitance, regulation and leakage losses.
(4) Increment cost of substations with higher pressures.

Determination of System Pressures.

The minimum working pressure for the various systems of mains is determined by the permissible pressure-drop, and in the transmission-system interconnecting stations by considerations of the transfer of load and idle current.

Some regulation of pressure will be permissible at the stations to counteract the pressure-drop in mains and transformers, and regulation of pressure may be necessary at other points in the system. The close relationship between pressure-drop and power factor has been pointed out.*

It appears probable that, without the necessity for new types of cable, main transmission by means of underground cables is feasible for many industrial areas such as exist in this country and, provided a reliable cable can be obtained, the advantage should lie with this method of transmission.

Should, at a later date, a higher pressure be required for the economical extension of the system of supply, a further system of mains at the higher pressure could be superposed and applied to the existing systems.

The maximum working pressure depends to some extent on the necessity for subdivision of circuits or the provision of alternative mains, a consideration which applies to both overhead and underground mains.

When dealing with very large amounts of power transmission, mains must at least provide an alternative route of supply to feeding points. The failure of a main conveying, say, 50 000 kVA is not lightly to be contemplated, and, moreover, the gradual development of the system and the necessity to conserve capital must be taken into account.

It would seem reasonable therefore to have regard to the probable demands at the end of a period of years determined by the rate of growth of the load, and to adopt the lowest pressure of transmission which will satisfy the requirements of pressure regulation for the period chosen.

If this pressure is sufficient to ensure the required regulation and does not involve the use of too many cables, there is unlikely to be any reason to go beyond it, particularly as experience of the use of cables for pressures exceeding 33 000 volts is limited.

On the other hand, the adoption of a transmission pressure which is too low for ultimate needs will involve either the addition at a later date of a further higher pressure system, or of continued excessive losses.

Conclusions.

The conclusions finally reached from a consideration of all the conflicting conditions are :—

(1) Three or more pressure systems are necessary for economy.
(2) The detail distribution pressure is fixed by considerations of use ; in this country a single-phase pressure of from 200 to 250 volts is established.
(3) The size of the distributing substations is determined by the density of load and the size of power users' installations.
(4) The distance between these substations depends primarily on the detail distribution pressure ; the substations should be as widely spaced as considerations of pressure-drop permit.
(5) The main distribution pressure is determined by the number of substations in the area, the total load, and the economical balance between expenditure on mains and on substations. In industrial areas in this country a pressure of 10 000 volts has been found to be economical.
(6) The extent of each power distribution network may be reduced with economy as the load develops and the size of substations increases.
(7) The transmission pressure is primarily determined by a consideration of pressure-drop and total power ; no advantage is gained by exceeding a minimum fixed to meet demands anticipated to arise in a period determined by the rate of growth of load.
(8) Main substations are determined in number by the load on the power distribution networks and the pressure of distribution, as well as by the number of generating stations.

[An instalment of the discussion on this paper will be found on page 108.]

* *Journal I.E.E.*, 1917, vol. 55, p. 72.

SOME ECONOMIC ASPECTS OF EXTRA-HIGH-TENSION DISTRIBUTION BY UNDERGROUND CABLE.

By Reginald O. Kapp, B.Sc., Associate Member.

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Summary.

It is pointed out that, with the heavy cost of feeders on modern power schemes, due to the centralization of the generation of electricity in very large power stations, a saving in the capital outlay on the E.H.T. distribution system will assume an economic importance comparable with that of reductions in coal consumption. Various means for keeping the cost of the distribution system as low as possible without sacrificing safety are discussed.

It is shown that the position of the power station with reference to its load influences the cost of feeders very greatly indeed, and methods of determining its most economical position are given. It is shown that the same methods can be employed to determine the most economical position of step-down transformer houses.

Rules are given with reference to the design of cable lay-outs and for determining the most economical voltage of transmission. It is shown that after an allowance for diversity factor and stand-by cables has been made, the total kVA capacity of feeders leaving a power station is still considerably greater than the maximum station load, on account of uneven sharing of the load, and means are suggested for reducing the discrepancy.

It is shown that, on account of the capacity of cables to store heat, their safe continuous rating may be lower than their peak load. The true safe continuous rating is the equivalent continuous load of a daily load curve, and a simple method of finding this is described.

It is shown that the cost of the unit of electricity depends to a notable extent on the consumer's load and power factor and, therefore, tariffs devised to encourage an improvement of both are equally to the advantage of supply undertakings and consumers.

The primary object of generating electricity in super-power houses is the conservation of our coal supplies. Incidentally, centralized generation reduces the cost of administration, superintendence and maintenance per unit sold, as well as the capital charges on the power-house buildings and plant. The net result is that electricity is to be had at a lower cost at the power-house busbars in a super-station than in a smaller power house. Beyond a certain size, however, no appreciable gain in economy is to be obtained by further centralization. On the other hand, the cost of the extra-high-tension distribution system increases steadily the more power is concentrated in a single power house. To obtain the price of electricity on consumer's premises, the capital charges on the distribution system must be added to the cost at the power-house busbars. Therefore, there is a limiting size of power house for any neighbourhood for which the cost per unit to the consumer will be a minimum. It is quite likely that, in many cases, this most economical size will be exceeded by the super-power houses of the future, as other questions, such as a plentiful supply of water for condensing purposes, coaling facilities, etc., are more likely to dictate the size of power house than the question of minimum cost. The result of this is that the capital charges on the E.H.T. distribution system of a super-power scheme will be comparable with the coal bill and may even exceed it.

How very great the variations in the cost of distribution may be will be seen from the following figures. If the power house with a peak load of 40 000 kVA is at the centre of a supply area extending over 5 square miles, and the supply is at 6 600 volts, the capital cost of distribution would be at present prices about £4 per kilovolt-ampere of peak load. This figure includes cable and 6 600-volt switchgear. If the power house is on the fringe of the supply area the cost would be from £10 to £12 per kilovolt-ampere for similar conditions. If the supply area extended over 40 square miles with a peak load of 40 000 kW the cost, with the power house at the centre, would be about £6·5, and, with the power house on the fringe, from £16 to £19 per kilovolt-ampere of peak load. From this it follows that a supply undertaking will have to include in the average charge per unit a figure which may be as low as about 0·05d. or as high as about 0·22d. to cover the capital charges on E.H.T. distribution. For very large power schemes it is even likely to be considerably higher than this figure. By comparison we may hope that the cost of coal for very large power schemes may be 0·28d. per unit.

The increasing size of the distribution system will introduce a new set of problems into the design of electrical undertakings. Whereas in the past by far the greatest amount of thought was given to the problem of making the most economical use of coal, we may in the near future expect a greater reduction in the cost of electricity to come from methods for cheapening the distribution system than from increases in generating efficiency. Considerations which appeared to be unnecessary refinements in the case of the comparatively small supply systems of the past will assume economic importance in the future, comparable with the saving obtainable with the use of economizers, mechanical stokers and feed heaters, and very much greater than that of a multitude of the schemes in current use in a modern power house for reducing coal consumption.

There is no doubt that a great deal of thought is being given to these problems in connection with large schemes now under consideration, and the conclusions of those members who are working at large distribution

schemes would be of very great value. The time seems therefore to have come when it is profitable to discuss on what lines economies in distribution systems may be effected. The object of this paper is to state some of the problems which it appears worth while to discuss in this connection.

The scope of this paper has been restricted to E.H.T. distribution rather than to distribution in general, in order not to cover too wide a field. The principal point on which E.H.T. distribution differs from low-tension distribution is that in the former case the limit to the capacity of the cables is almost always set by heating, whereas in the latter case it is more usually set by voltage-drop. Where overhead transmission is used, higher voltages than for cables can be safely adopted, and therefore large quantities of power can often be sent more cheaply in bulk over a long distance. On the other hand, overhead line work is not as cheap as cables when small quantities of power have to be distributed to isolated points of a complicated network. Therefore, the remarks in this paper are only partly applicable to overhead transmission problems.

The aspects of the subject on which this paper dwells are as follows :—

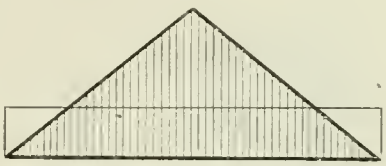
(1) The choice of the most economical position for the power house and transformer houses.
(2) The choice of the most economical voltage.
(3) Various types of cable lay-out.
(4) Means of ensuring that all radial feeders shall be fully loaded.
(5) How to make the fullest use of the current-carrying capacity of cables.
(6) The use of tariffs for obtaining the best use of the distribution system.

(1) Choice of Position for Power House and Transformer Houses.

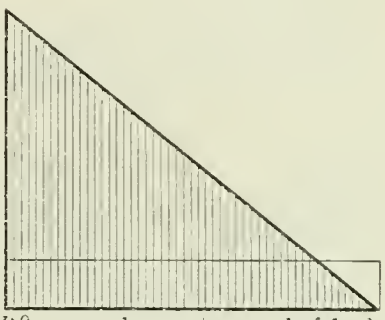
It is well known that the cheapest distribution system is obtained when the power house is nearest to its largest or most concentrated load. For a given voltage-drop the cheapest cable lay-out occurs when the power house is at the centre of gravity of the load. But the cheapest cable lay-out is not necessarily at the centre of gravity of the load when heating of cables determines their size. The cost of E.H.T. distribution when the power house is on the fringe of the supply area is theoretically double that with the power house at the centre for an area with an even load distribution, and three times if the density of demand is greatest at the centre of the supply area and is steadily reduced to zero from the centre to the fringe. In practice the ratio is rather less than that given by these theoretical figures. If two power houses are installed at opposite sides of a town, and each on the fringe of the town, the cost of E.H.T. distribution is obviously the same as for a single power house at the centre if the density of demand is constant throughout the area. It is twice that for a single power house at the centre if the density of demand diminishes towards the fringe. So there is little or no gain in supplying a town from two power houses. If the power houses are to be interconnected the cost of distribution will almost always be greater than if there is only one power house, on account of the cost of the interconnection. Two power houses, both well within the supply area may, however, give a cheaper distribution system than one at the centre.

Figs. 1 to 5 show graphically the relative cost of the distribution system with various positions of the power house for the special case of an area of which the width is small compared with the length. A town like Liverpool following the course of a river would be such an area.

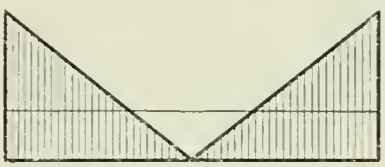
Figs. 1 and 2 illustrate the relative cost of distribution for the cases mentioned above. The ordinates



(*a*) One power house in centre of load



(*b*) One power house at one end of load



(*c*) Two power houses; one at each end of load

Fig. 1.—Cost of E.H.T. distribution with different position of power house, when load is evenly distributed.

of the curves represent the total power flowing across a section of the town, and for rough comparative purposes this may be taken as equal to the total cost per yard of cable across a section of the town, so the total shaded areas represent approximately the total cost of cable. The thin line on each diagram represents the density of demand across a section of the town. If the area in Fig. 1(*a*) is taken as unity, the areas of the remaining figures are as follows :—

Diagram ..	1 (*a*)	1 (*b*)	1 (*c*)	2 (*a*)	2 (*b*)	2 (*c*)
Shaded area	1	2	1	$\frac{2}{3}$	2	$\frac{1}{3}$

The choice of the number and position of power houses is one that will rarely be much affected by considerations of the cost of distribution. The supply

of coal and condensing water will generally be the determining factors. But, if the power house is far enough away, it pays to have a transformer house

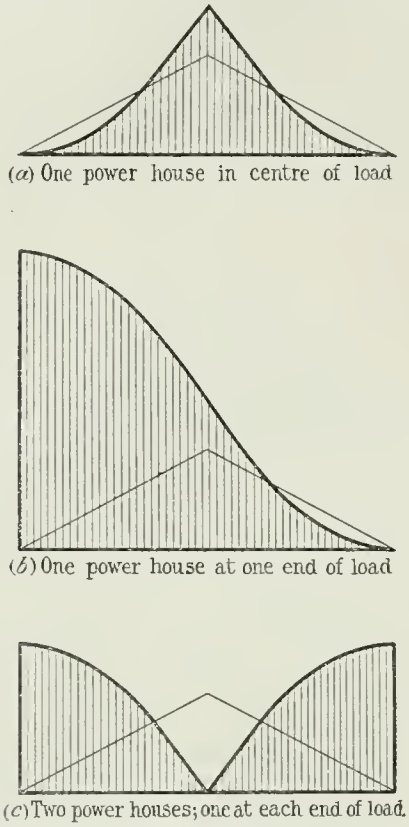


FIG. 2.—Cost of E.H.T. distribution with different positions of power house, when load diminishes evenly from middle to end of supply area.

within the boundaries of the town which is supplied at a high voltage which we will assume to be 30 000 volts, from which the power for the town is stepped

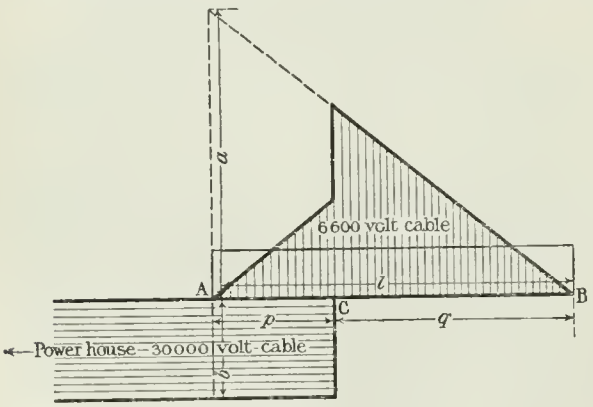


FIG. 3.—Cost of E.H.T. distribution with transformer house at point C and load evenly distributed.

down to some lower voltage which, for the purpose of illustration, we will assume to be 6 600 volts. A question of interest then is to determine the most suitable position for the transformer house. Fig. 3 shows the expenditure on cable theoretically required when the load is evenly distributed over the town.

The limits of the supply area are A and B, and the transformer house is situated at C. The length of the town is l, which is divided in the ratio p/q by the transformer house; a represents the cost per yard of 6 600-volt cable, and b the cost per yard of 30 000-volt cable needed to supply the whole of the town load. The whole of the horizontally and vertically shaded areas between A and B represent the total cost of cable within the limits of the town.

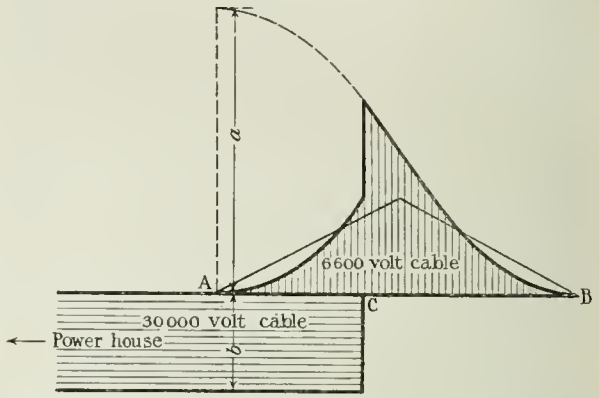


FIG. 4.—Cost of E.H.T. distribution with transformer house, and load diminishing evenly from middle to end of supply area.

This area is :—

$$\frac{a}{2l}p^2 + \frac{a}{2l}(l-p)^2 + bp$$

On differentiating this expression with regard to p it will be found that this area is a minimum when

$$\frac{2p}{l} \times \frac{a}{b} = \frac{a}{b} - 1$$

a/b is the ratio of the cost per yard per kilovolt-ampere of 6 600-volt to 30 000-volt cable. If we put this ratio $= \alpha$ we find that for minimum cost of distribution system

$$\frac{p}{l} = \frac{\alpha - 1}{2\alpha}$$

Fig. 4 shows a similar diagram for a town in which the density of demand is a maximum at the centre and becomes gradually and evenly reduced to zero at the outskirts. It can be shown that, for this figure, the saving in cost of cable is theoretically a maximum when

$$\frac{p}{l} = \frac{1}{2}\sqrt{\frac{a-1}{a}}$$

The ratio of the cost of 6 600-volt to 30 000-volt cable per kilovolt-ampere per mile is about 3 if allowance is made for stand-by cable in both cases, and the greater amount of switchgear required on a 6 600-volt network is taken into account. From this it follows

that p should theoretically be equal to $\frac{1}{3}$ AB if the load is evenly distributed throughout the town as in Fig. 3, and $\frac{1}{2}$ AB$\sqrt{\frac{2}{3}} = 0{\cdot}4$ AB if the load diminishes evenly from the centre to the fringes of the town as in Fig. 4. Various practical factors make these theoretical figures only approximately correct. For the sake of simplifying the mathematics, the town has been treated as if it had length without breadth, and the ratio of cost per kilovolt-ampere of 30 000-volt and 6 600-volt cable has been based on the assumption that the load would be a multiple of the load capacity of one cable. In practice this is rarely the case, and so the full-load capacity of the cable is not made use of. On the 30 000-volt system the effect of this is greater, as the cable unit is larger than on the 6 600-volt system, and so it would be better in designing a transmission scheme to place the transformer houses on sites rather nearer to the fringe of the town than in the positions given by the above theoretical figures.

If the power house itself is on the fringe of the town, and the town is a large one, it may be economical to supply the distant parts at 30 000 volts. The question having been determined, how great an area is to be supplied through the transformer house at the opposite

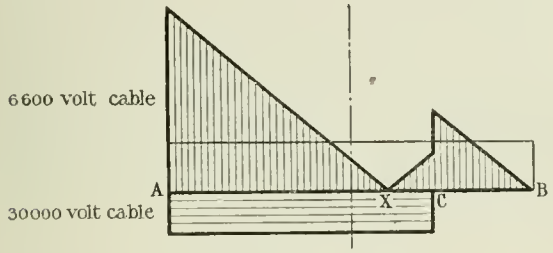


Fig. 5.—Economical position of transformer house on a mixed-voltage system, with load evenly distributed.

side of the town to the power house, the best position of the transformer house within this area is found by the above formula to be about one-third of the distance across the area. Fig. 5 illustrates the cost of cable for such a scheme.

In this case the voltage at the transformer house must be so arranged that the point X in Fig. 5 shall be the point of lowest potential on the system. If the cable lay-out is designed for this condition and in actual working the point of lowest potential does not occur at X, part of the cable system will be overloaded. As this is very liable to happen in practice, it is advisable to install sufficient feeder capacity to meet the occurrence, and to provide a few interlinking cables across the boundary line between the area supplied by the power house direct and that supplied through the transformer house. The simplest way of fixing the transformer house voltage and, therefore, the position of the point X, is by means of transformer tappings. The usual transformer tappings of $2\frac{1}{2}$ and 5 per cent up and down give too coarse a regulation for a mixed-voltage system. To get a good range of voltage in small steps with the least possible number of tappings, the author has adopted a scheme with tappings at different points on the step-up and step-down transformers. If, for instance, the step-up transformers have $1\frac{1}{2}$ per cent tappings up and down, and the step-down transformers $2\frac{1}{2}$ and 5 per cent up and down, the following percentage voltage variations up and down from the normal are possible :—

0, 1, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, 4, 5, and $6\frac{1}{2}$ per cent

That gives us 15 possible ratios.

The above discussion does not take voltage-drop in the feeders or feeder losses into account. For the purpose of the rough generalizations dealt with here, the latter are too small to need considering. Voltage drop on the E.H.T. system is always so small that it need not govern the lay-out as it does in the case of L.T. distribution systems. In the case of E.H.T. cables, heating and not voltage-drop is always the determining factor.

For this reason, as has already been pointed out, the most economical site for the power house is not at the centre of gravity of the load. This fact is worth emphasizing, as it is one about which misapprehension is very general. The most economical site for the power house is roughly such that the sum of the product of each load into its distance shall be a minimum. For the centre of gravity the sum of the product of each load into the *square* of its distance would be a minimum. More accurately we may say that the most economical position of the power house is such that the sum of the product of the cost per yard of cable for supplying each load, into the distance of the load, shall be a minimum. That takes the circumstance into account that the cost per kilovolt-ampere of supplying a small load is greater than that of supplying a large load.

The author has not succeeded in finding an analytical method of determining the most economical position of a power house except for the special case of a long, narrow area dealt with already. An ingenious way of solving the problem has, however, been suggested by Mr. C. G. Carrothers. Imagine a map of the supply area to be pasted on a horizontal board and a hole drilled through the board wherever an important load is shown. Bring a string up through each hole and tie all the strings together in a single knot, or fasten them all to one small ring. To the loose end of each string attach a weight proportional to the cost per yard of cable of supplying the load at the point of the map through which its string passes. On the map one will now see a number of strings meeting at one point, their other ends passing through holes to the weights beneath. The potential energy of the system of strings visible on the map is the sum of the product of the tension in each string into the distance between the knot and the hole in the board. The system will automatically adjust itself so that its potential energy becomes a minimum. That is to say, the knot will take up that position on the map which represents the most economical site for the power house.

If the site of the power house is fixed, and the problem is to determine the most economical position of a step-down transformer house, the same method can be used. A hole must be made through the map where the power house is shown, and the weight on the string

through this hole must be proportional to the cost per yard of supplying the transformer house from the power house at 30 000 volts. The knot will then take up the most economical position for the transformer house. For the sake of easily understanding Mr. Carrothers's method we have been imagining a crude model with string passing through holes in a wooden board. The friction of such a model would be too great to give a precise result. But a model in which the strings were replaced by fine wire and the holes by pulleys of small diameter designed to move with as little friction as possible would be a useful piece of apparatus.

(2) Most Economical Voltage.

The cost of cable per kilovolt-ampere transmitted is less the higher the voltage, and so it would pay in most systems to have the voltage as high as convenient. If increase in the voltage involves the use of transformers, the cost of these and their switchgear must

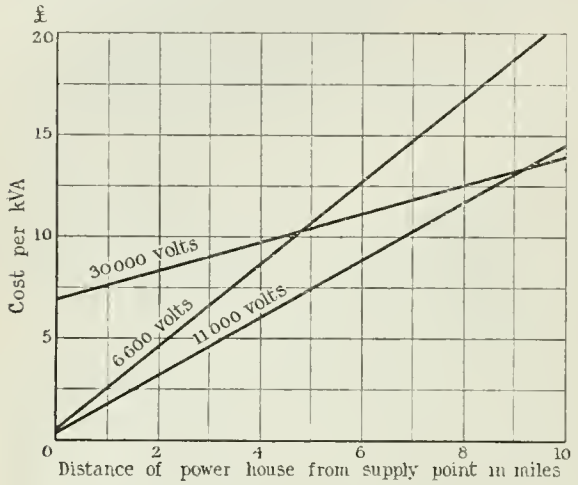


Fig. 6.—Cost of E.H.T. transmission in bulk.

be charged to the E.H.T. distribution, and this makes the cost per kilovolt-ampere greater than that for a lower voltage, unless the transmission distance is great enough to make the saving in cable pay for the cost in transformers and switchgear. Therefore the highest safe voltage for a given set of conditions is the right one to select, and circumstances may call for the selection of one of the following:—

(*a*) The highest voltage at which one can sell power.

(*b*) The highest voltage at which one can generate power.

(*c*) The highest voltage at which one can transmit power.

Other voltages cannot be justified.

The author has assumed these to be 6 600, 11 000, and 30 000 volts respectively. He quite appreciates, however, that 30 000 volts is probably not the ultimate safe limit for underground cables.

Fig. 6 shows the cost of transmission for these three voltages. Everyone who has designed a supply system over a scattered area will know that the actual cost of distribution comes higher than that given by the curves, which merely assume that power in bulk has to be sent from one point, and is to be used concentrated at another point. The curves make due allowance for stand-by feeders and switchgear, but they do not allow for the fact that in most practical cases the load does not happen to be a multiple of the load capacity of one feeder, and therefore the feeders are rarely used to the limit of their capacity. For most practical cases the cost of 30 000-volt transmission would, for this reason, be higher than that shown on the curve. On the other hand, the 30 000-volt scheme might be taken to imply delivery to substations and consumers at 6 600 volts, whereas the 11 000-volt scheme implies delivery at 11 000 volts, which may be considered to be a voltage for which there is less demand. Hence, an exaggerated value must not be attached to theoretical considerations in the choice of voltage, and the point of intersection of the curves shown on Fig. 6 should only be taken as an average above and below which some latitude must be permitted to suit each individual case.

On the whole the author would be inclined to adopt a 30 000-volt transmission with a 6 600-volt delivery for distances somewhat less than that given by the point of intersection of the curves shown. The curves shown in Fig. 6 assume the cables to be laid under average circumstances for heat dissipation and do not allow for the cost of cable protective gear. In the author's opinion it is very doubtful indeed if engineers will decide in future schemes that the advantages of cable protective gear are great enough to justify its cost.

(3) Cable Lay-out.

The problem to be solved in designing a cable lay-out is how to get at least cost the necessary number of cables to each supply point plus one spare cable. Cables are very reliable and one need not consider the possibility of more than one on a large supply system being out of service at a time. The most obvious way of designing a distribution system would be to connect each supply point to the power house by the requisite number of feeders. Such an arrangement would resemble a star. It is an uneconomical method for two reasons. One is, that the load at each point will not always be an exact multiple of the load capacity of a cable unit. In order to avoid overloading the cables it is necessary to choose the next higher whole number to the number given by dividing the load by the load capacity of each cable. The other reason is that the number of spare feeders is needlessly great. In order to reduce as far as possible the waste due to these causes, it is usual to connect the more distant supply points to the power house via other nearer ones, and to take sufficient feeders from the power house to the nearest supply points for their load plus the whole of the load beyond them, with only one spare feeder for the whole of the load along one ray of the star.

A further economy in the superabundant feeder capacity can be effected by interconnecting supply points, which are about equidistant from the power

house, by link feeders. Then the distribution system resembles a star no longer. It is more comparable with a cobweb. The function of each link feeder is to act as a stand-by to two rays of the star. Suppose that in Fig. 7 the capacity of each cable is 3 750 kVA and the load at A is 7 700 kVA and at B is 7 000 kVA. Without the link feeder three cables would be required from the power house to B, and four from the power house to A, if each supply point is to have a stand-by.

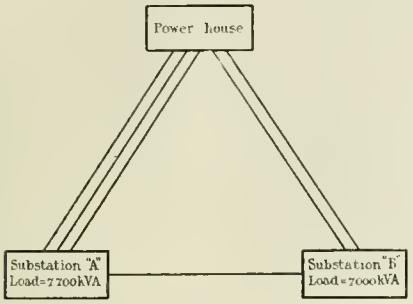


FIG. 7.—Radial and link feeders.

If A and B are close together, a link feeder between A and B would carry enough current in either direction to make up for the failure of a radial cable either to A or to B. If one of the radial feeders to B breaks down, the other one can carry 3 750 kVA, leaving 3 250 kVA to be supplied through the link feeder. The total load on the three radial feeders to A will be 10 950 kVA, which is within their capacity of 11 250 kVA. If, on the other hand, one of the radial feeders to A breaks down, the two remaining feeders can take 7 500 kVA, leaving 200 kVA to be supplied via B through the link. The two radial feeders to B are large enough to take this load in addition to the load at B.

Sometimes a number of supply points are arranged on a system known as a "ring main." Any point can be supplied from the power house, in either a clockwise or a counter-clockwise direction. This is only used for a small number of isolated loads conveniently situated.

Occasionally a main feeder is laid through a district and tappings taken off it to consumers on the right and left. This might be called a "herring-bone" type of lay-out. It is used for low-tension and sometimes for overhead line work, but is not much in favour for underground E.H.T. cable, partly because a failure anywhere means shutting down all consumers connected to the whole system, and partly because the buried tappings are a source of weakness.

Hence the "herring-bone" type of lay-out may be ruled out of court. The "cobweb" type seems best at first sight, but it must be remembered that the full current-carrying capacity of the link feeders can rarely be made use of even as stand-by feeders. For this reason link feeders are only economical if they are short, and a combination of the "star" and "cobweb" types is generally best. The former is employed where loads are far apart, and the latter where they are close together.

A great deal of ingenuity can be expended on designing cable lay-out schemes, but as soon as one tries to make generally applicable rules one finds that the problem is so indefinite and depends so much on individual cases that generalizations do not apply to it. One might as well try to devise a formula for playing chess. However, a few guiding principles can be stated.

(1) The system should be so arranged that failure of any one feeder does not cause any other feeder to become overloaded. In a complicated network one cannot always be sure on this point without calculating in what proportion the feeders share the load.

(2) Unless some device such as that suggested in the following section is adopted, link feeders will only carry a small amount of power compared with their load capacity, as the difference in potential between the ends of a link feeder is not great. Therefore, their usefulness as stand-by cables is frequently illusory.

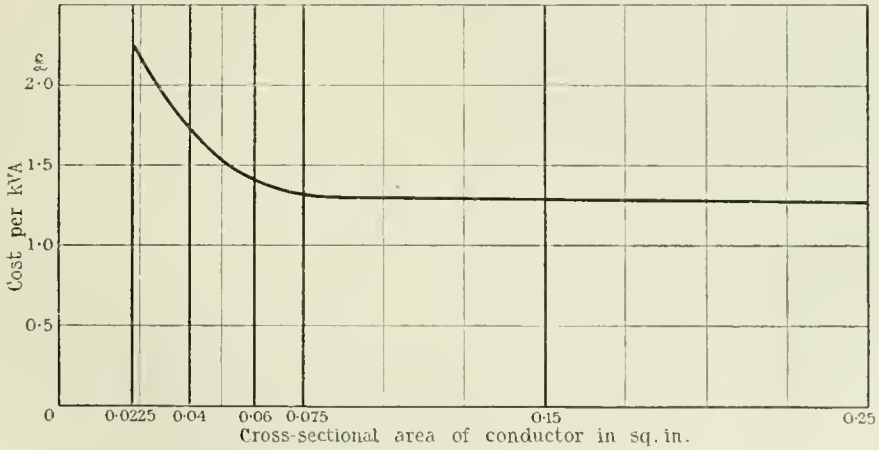


FIG. 8.—Cost per kVA per mile of 6 600-volt cable.

(3) Link feeders must not short-circuit any current-limiting reactances that may be installed between busbar sections at the power house.

(4) If a consumer is situated between the power house and a substation, and is supplied by a cable from the latter, the cost of cable for supplying him is about twice the cost of cable between the substation and the consumer, as the power has to make a double journey. Unless the existing cables to the substations are likely to suffice for both loads in future, it may be more economical to lay a new cable from the power house direct to the new consumer, even though that involves a greater immediate outlay.

(5) If power is tapped off a feeder, the possible future load that may safely be taken off the end of the cable is reduced by the power tapped off nearer the power house. Unless the cross-section is reduced beyond the tapping point, feeders should only be tapped after the probable future growth of the load has been duly considered.

(6) On a ring main the cross-section of copper should be reduced on the feeders furthest from the power house.

(7) When the power has parallel paths through feeders of unequal cross-section of copper it will divide so as to give the same current density in each feeder. Advantage cannot, therefore, be taken of the circumstance that the feeder of smaller cross-section may safely be worked at a higher current density than the other.

(8) Up to about 0·25 sq. in. the cost per kilovolt-ampere per mile of cable is cheaper the greater the cross-sectional area of copper of the cable. On the other hand, the proportional cost of stand-by cable is increased for the heavier cross-sections, and the chance of utilizing each cable to its full-load capacity is reduced. The number of cable boxes required is also increased. Assuming the cable to be laid under the best conditions for heat dissipation, the cost per kilovolt-ampere per mile of 6 600-volt cable is approximately as shown in Fig. 8. When more becomes known concerning the safe loading of cables this curve may have to be somewhat modified, and it may be further modified by fluctuations in prices, but its general shape shows that there is little to be gained by increasing the cross-section above 0·075 sq. in. The author therefore believes that 0·075 sq. in. or some figure near to it will in future become the standard cross-section on 6 600-volt networks, with possibly feeders of some smaller cross-section on the outer fringe of the supply area.

(4) Equalizing the Load Between Feeders.

In a complicated network it is inevitable that the cables do not share the load equally and, as the capacity of the system is determined by the capacity of the most heavily loaded cables, full use can hardly ever be made of all the cables leaving the power house. The matter is further complicated by the need of stand-by cables. If any one of the cables leaving a power house should break down, the distribution of the load among the others will be affected, and the engineer must bear in mind that his total load capacity is limited by the load on that cable which will be most heavily loaded when any of the other cables is out of service.

As was mentioned above, link feeders are used to equalize the load between radial feeders as far as possible, but, under purely ohmic conditions of pressure drop on the system, these link feeders do their work very imperfectly. If, for instance, Fig. 9 shows a portion of the network of a cobweb type lay-out, and the maximum loads at the substations are as indicated in the diagram, power will be carried by the link feeders in accordance with Ohm's law as shown. The load shown for each substation need not necessarily be used at that substation. A part of it might be outgoing power to supply other, more distant, substations.

If the capacity of any single feeder is 3 000 kVA, the feeders to substation B will be nearly fully loaded. The normal growth of the load which would be likely to occur at B, as well as at A and C, would demand an increase in the capacity of the network, or the introduction of some device that would make better use

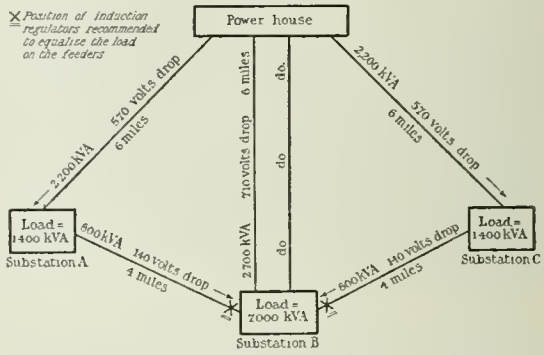


Fig. 9.—Uneven distribution of load between feeders,

of the feeders to A and C which are loaded well below their full-load capacity.

It appears that, for such cases with which every mains engineer is familiar, boosting transformers or induction regulators on the link feeders would most cheaply and efficiently increase the available network capacity. In the example shown in Fig. 9, a boosting transformer capable of giving a boost up or down of about 250 volts, or less than 4 per cent, would be ample for all load conditions likely to arise. With the conditions given in the example, an additional load of 1 000 kVA at B could be supplied through the existing network if two such boosters were installed at B, one on the link feeder to A, and the other on the link feeder to C. Such boosters would have to be set for 230 volts. Without the boosters, additional load would have to be met by a further feeder from the power house to B, the cost of which would be from £15 000 to £20 000 according to the cross-section. The boosters, if of the induction regulator type, would probably not cost more than £1 500 each with all necessary switchgear, so the saving by installing boosters would be considerable. That may not, however, be a fair way of looking at the problem, as the boosters add only 1 000 kVA capacity to the system, whereas

a further cable adds considerably more. The cost per kilovolt-ampere per mile for 6 600-volt feeders works out at roughly £1·25. At this figure, additional feeder capacity can be obtained by a further feeder at a cost of £7·5 per kilovolt-ampere or by induction regulators at a cost of £3 per kilovolt-ampere. The example chosen is a fairly random one and typical of average conditions, except in so far as it is more symmetrical than a practical case, for the sake of clearness. For an actual case worked out by the author on an existing undertaking, the load capacity of the distribution system could be increased by an extra cable at a cost of £10·6 per kilovolt-ampere, or by induction regulators at a cost of £4·2 per kilovolt-ampere.

The example chosen in Fig. 9 does not show a stand-by cable. If it is to be typical of an actual scheme, it must be drawn on the assumption that one cable has broken down somewhere because of the rule that the load capacity of a cable network is its load capacity

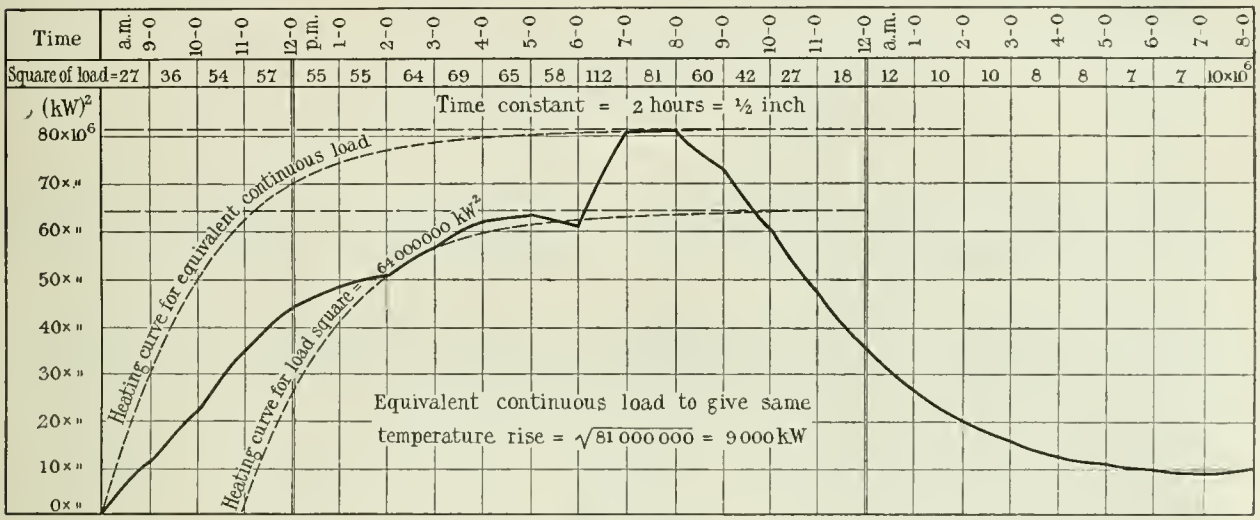

Time
a.m. 9-0 10-0 11-0 12-0 p.m. 1-0 2-0 3-0 4-0 5-0 6-0 7-0 8-0 9-0 10-0 11-0 12-0 a.m. 1-0 2-0 3-0 4-0 5-0 6-0 7-0 8-0
Square of load=27 36 54 57 55 55 64 69 65 58 112 81 60 42 27 18 12 10 10 8 8 7 7 10×10⁶
, (kW)²
80×10⁶
70×"
60×"
50×"
40×"
30×"
20×"
10×"
0×"
Time constant = 2 hours = ½ inch
Heating curve for equivalent continuous load
Heating curve for load square = 64 000 000 kW²
Equivalent continuous load to give same temperature rise = √81 000 000 = 9000 kW

Fig. 10.—Temperature chart of cables on 25th September, 1919.

when any one feeder is out of service. That means that the network would be designed large enough to take the peak load without the use of the induction regulators. The safe working of the system at ordinary times would not depend on this apparatus. The induction regulators would really be a part of the stand-by equipment, though they might be set so as to equalize the load and save copper losses during normal working with all feeders in service. Although automatic induction regulators are on the market, it hardly seems necessary to attempt to obtain automatic working of induction regulators on link feeders. It must be remembered that the apparatus fulfils its functions only at the time of peak load. During light loads it matters very little whether or not the radial feeders share alike. The most attention, therefore, that is likely to be needed is a hand-setting once a day at the time of peak load. Even that amount of regulation may be superfluous, however, for on most systems the peak-load conditions repeat themselves for many days on end, so that one setting would suffice for a long time.

It may be objected that the peak load does not occur at the same moment in the substations at the two ends of a link feeder; therefore, an induction regulator set to help matters at the moment of peak load at substation B would have a harmful effect by exaggerating the load on the radial feeder to A at a moment when substation B was lightly, and substation A heavily, loaded. This argument would apply to an overhead line having low heat-storage capacity. In the case of cables, however, as will be shown further on, load conditions which last for a short time have but a small heating effect and, in consequence, the induction regulators will fulfil their function if set to meet what is called further on the equivalent continuous load, provided the peaks at the two substations occur within a reasonably short time of each other. For cases where the peak load in one substation occurred some hours before that at the other, the induction regulators would be no use without frequent adjustment.

(5) Load Capacity of Cables.

It is not so easy to say when a cable under working conditions is loaded up to the safe limit as might at first sight appear. One may determine by experiments what maximum temperatures are permissible for the various parts of the cable, and one may further determine what steady load gives those temperatures. For such tests, one may bury the cable in the ground under a street as it would be laid in service. In fact, all the working conditions are fairly easy to reproduce experimentally, except those of an intermittent or varying load. In the course of 24 hours, the load on a cable may vary between almost zero and a peak load of high value but short duration. The mains engineer knows what continuous load his cables can stand. The question that interests him is whether the actual varying load is producing at any time during

the day a dangerously high temperature in his cables. He may, and often does, design his feeder system so amply that the maximum peak load on any cable is no higher than the safe continuous load. That method is safe but extravagant. To make a guess and design the system for some value less than the peaks, treating the peaks as a permissible overload, is less extravagant, but it may not be safe. In order to combine safety with economy, the problem must be studied a little more closely. The problem may be formulated thus. What is the continuous load equivalent to a given varying load? Equivalent load in this case means that load which gives the same maximum temperature rise.

In determining the rating of a tramway motor the root-mean-square value of the load is taken. As, in the case of a tramway motor, a complete cycle from peak to zero and back to peak load recurs at frequent intervals, this method is justified. In the case of cables subjected to a daily load curve with slow changes, this method would, however, give too low a value. One must take account of the progressive heating effect of the varying load during 24 hours or more. Fortunately, this is not such a laborious operation as one might think. Let Fig. 10 be a temperature chart of the cable during 24 hours' heating under a varying load. The abscissæ represent time, and the ordinates temperature. The temperature scale is not actually

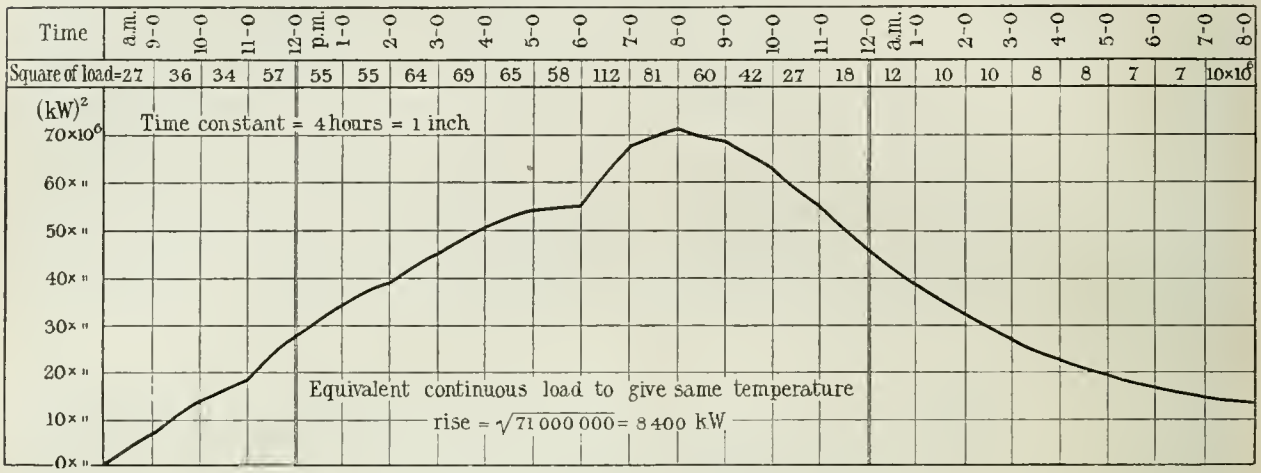


FIG. 11.—Temperature chart of cables on 25th September, 1919.

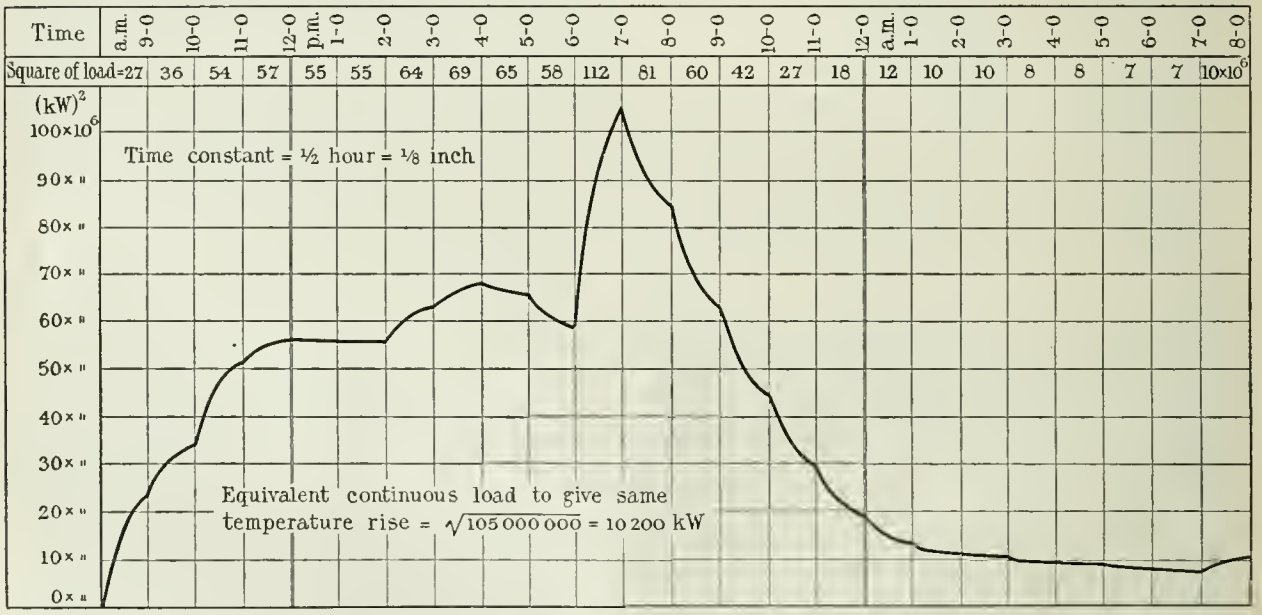


FIG. 12.—Temperature chart of cables on 25th September, 1919.

shown on the diagram. The scale on the left-hand side will be explained later. The smooth logarithmic curve on the same diagram is a heating curve corresponding to that steady load which, if applied long enough, would give the same maximum temperature as the actual varying load. The temperature chart is drawn on the assumption that the load remains constant during the period of one hour. Each part of the temperature chart is therefore a small portion of a heating or cooling curve, similar to the smooth logarithmic curve. One such heating curve is continued with dotted lines to show how it forms a portion of such a logarithmic curve. This shows how the cable temperature would have varied with time if the load had been the same throughout the period as it was between 2 p.m. and 3 p.m. It would then have reached a final temperature, given by its asymptote which is the dotted horizontal line.

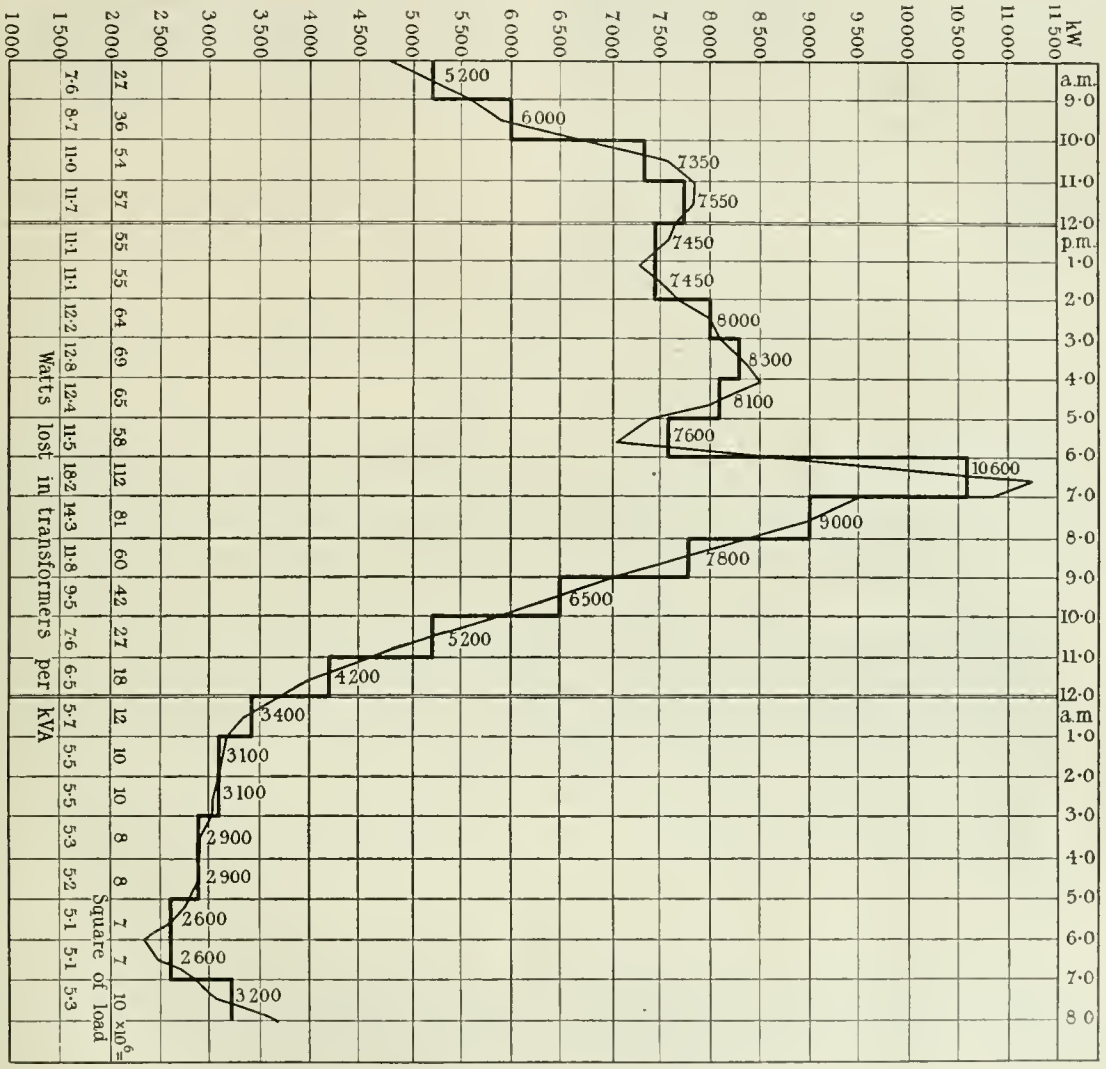


FIG. 13.—Load curve for 25th September, 1919.

Fig. 10 does not show a scale of temperature, and so it only indicates the shape of the temperature chart, and not the actual temperatures reached. A few words of explanation of the vertical scale chosen are necessary. The equation of a heating curve is

$$\frac{y}{T} = 1 - e^{-\frac{x}{C}}$$

where y is the temperature rise after time x, T is the steady temperature the cable will ultimately reach, C is a constant depending on the heat capacity of the cable and the amount of heat it can dissipate in unit time, and e is the base of Napierian logarithms.

It will be seen that x is a function of the ratio of the temperature rise after time x to the final steady temperature rise. In other words, the time it takes to reach a given proportion of the final temperature

rise is independent of the actual temperature rise, so that the shape of the curve is independent of the scale of temperature. We are therefore at liberty to make the scale of temperature such that a unit on the vertical scale shall represent the final steady temperature rise reached when unit load is taken by the cable. We may even go a step further and let a unit on the scale of ordinates represent the final steady temperature rise, which occurs on any of the cables leaving the power house, when the power house output is unity, provided all the cables take the same proportion of the total load at all times. That has been done on the temperature charts shown in this paper, and it will be seen that the shape of the curves is correct, although we do not know how many degrees of temperature are represented by unit length on the scale of ordinates. The final steady temperature being proportional to the square of the load in the case of cables, the scale of ordinates must be proportional to the square of the load.

A temperature chart drawn for 24 or more different loadings looks complicated, and would be a very laborious undertaking were it not that each partial curve can be traced on transparent paper from a template curve with sufficient accuracy for practical engineering purposes. Suppose on Fig. 15 the zero line is at the temperature of the ground, and the load is such that the cable will approach a steady temperature corresponding to T_1 if this load persist for a sufficiently long time. The cable will heat up in accordance with the curve Op_1. Now let the load become such that the final steady temperature would be T_2. The heating of the cable will now follow a heating curve through $p_1 p_2$. The slope of a heating curve at any point is proportional to the vertical distance of that point from the asymptote. For equal distances from the asymptote, the slope is the same wherever the asymptote may be. Hence, if on Fig. 15 p_1' be drawn as far below T_1 as p_1 is below T_2, and $(x_1' - x_2')$ equal to $(x_1 - x_2)$, the curved line $p_1' p_2'$ will be identical with $p_1 p_2$. If a template curve is prepared like the curve drawn asymptotic to T_1, any portion of the temperature chart such as $p_1 p_2$ can be traced through from it on to transparent paper placed in the correct position above the template curve.

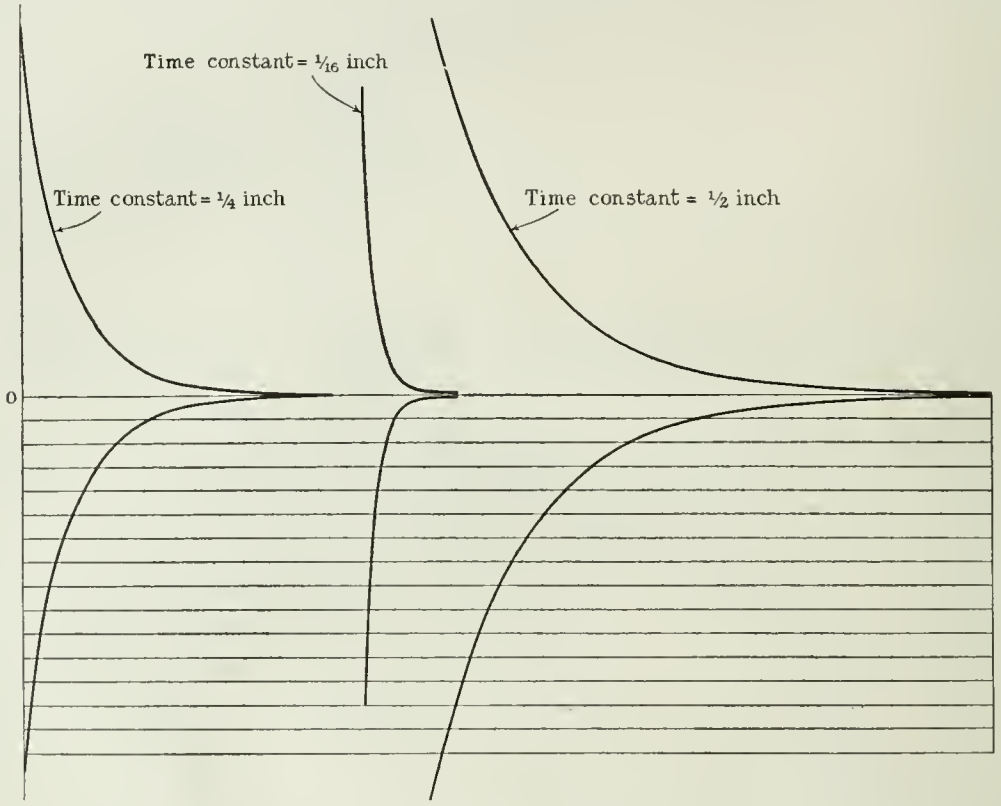


FIG. 14.—Heating and cooling curves.

When the load is such that it would maintain the cable at a constant temperature lower than the temperature of the cable at the time when the load is applied, the cable will cool. The cooling curve is of the same shape as the heating curve, but it approaches the asymptote from above. Fig. 14 shows three sets of heating and cooling curves similar to those used by the author as template curves in the preparation of temperature charts. These curves are drawn to different values of the time-constant C.

It was shown above that it is immaterial to what scale of temperature a temperature chart is drawn.

For the horizontal scale the case is different. This depends on the constant C in the equation. When $x = C$ in the equation

$$\frac{y}{T} = 1 - e^{-\frac{x}{C}}, \quad \text{then} \quad \frac{y}{T} = 0{\cdot}63$$

C can therefore be defined as the time taken by the cable to reach 63 per cent of its final temperature,

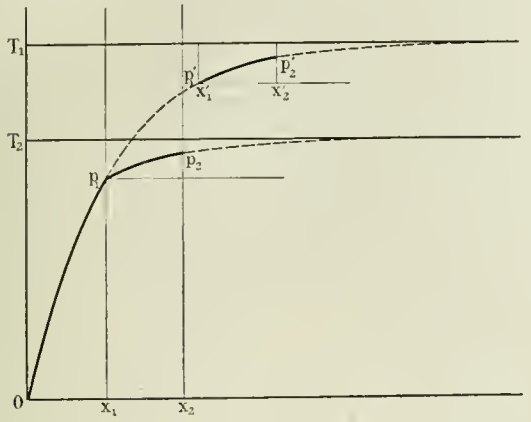


Fig. 15.

and it is called the "time constant." This constant has different values for different cables, and also depends on how the cable is laid. The three template curves, of which Fig. 14 is a reproduction to a smaller scale, are drawn so that this constant is represented by $\frac{1}{4}$ inch, 1 inch and 2 inches respectively. Suppose a temperature chart is to be drawn for a scale having a time constant of $1\frac{1}{2}$ hours, and the template curve having a time constant equivalent to $\frac{1}{2}$ inch is to be used. The scale of time on the tracing paper must be such that $\frac{1}{2}$ inch = $1\frac{1}{2}$ hours. Each time interval of I hour must be represented by $\frac{1}{3}$ inch, and a temperature chart for 24 hours would be 8 inches long. In order to be able to keep temperature charts to a convenient length for various time-constants, it is useful to have three template curves of different sizes, as shown in Fig. 14.

The method of procedure is as follows. Select the most severe daily load curve of the year from the point of view of heating of the cables. This is not necessarily the curve with the highest peak. It is more likely to be the curve for that day on which the greatest number of units were sold, though generally the highest peak and the greatest number of units sold occur on the same day. Divide this load curve into hourly averages and write the square of each average load on the load curve as has been done in Fig. 13. Next prepare a piece of tracing paper in accordance with Fig. 10, where vertical lines have been ruled corresponding to intervals of an hour with a spacing determined by the time-constants in hours of the cable, as described above. Write the square of the load along the top of the paper against each hourly interval and draw a vertical scale of square of load on the left of the paper. This scale can be chosen arbitrarily, but it is convenient to choose it so that the horizontal lines shown on the template curves in Fig. 14 coincide with round numbers on the scale of square of load. Now place the transparent paper over the template curve so that the asymptote of the latter is in a position corresponding to the square of the load for the first hour of the day, and the curve goes through the origin of the chart. Trace the curve in this position for the first hour and move the transparent paper vertically till the asymptote of the lower template curve is on the square of load for the second hour, and move the paper horizontally till the template curve passes through the point reached by the temperature chart at the end of the first hour. In this position the curve can be traced for the second hour, and the process can be thus repeated for each succeeding hour until the complete chart is built up. Care must be taken that the axes of the chart and the template curve are always kept parallel.

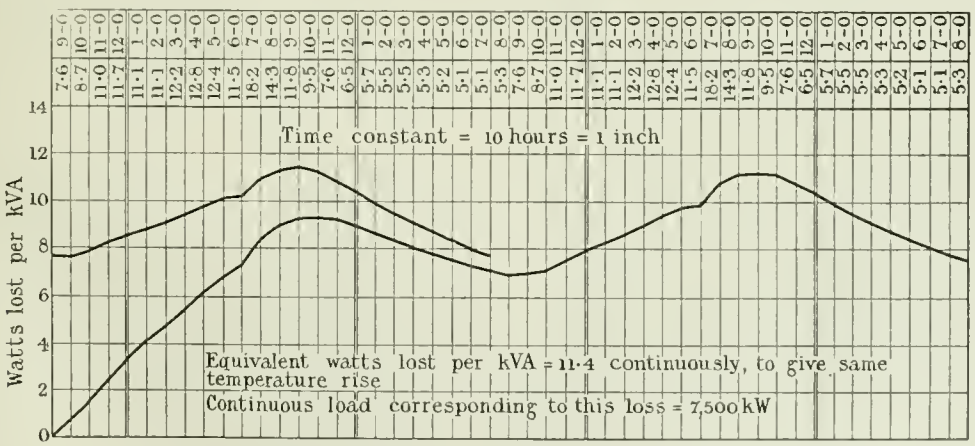

Time constant = 10 hours = 1 inch
Watts lost per kVA
Equivalent watts lost per kVA = 11·4 continuously, to give same temperature rise
Continuous load corresponding to this loss = 7,500 kW

Fig. 16.—Temperature chart of transformers on 25th September, 1919.

The method involves little calculation. The whole temperature chart is traced by the manipulation of

a piece of tracing paper. The most complicated part is the determination of the average load figures and their squares. The remaining work of tracing the charts for various time-constants could be done by an intelligent switchboard attendant, and each chart would take about 10 minutes. Figs. 10 to 12 show a number of temperature charts that have been constructed in this way. They are all based on the load curve shown in Fig. 13. It will be seen that the greater the time-constant of the cable, the smaller is the equivalent continuous load. The average value of the load for the day in question was 5 830 kW, and the "root mean square" is 6 300 kW. Hence the load factor was 50 per cent. The curve shown in Fig. 18 gives the ratio of equivalent continuous load to peak load as a function of the time-constant. The points shown on the curve are those actually obtained by the template method described above, and their consistency indicates that the method may be trusted to give sufficient accuracy for practical purposes.

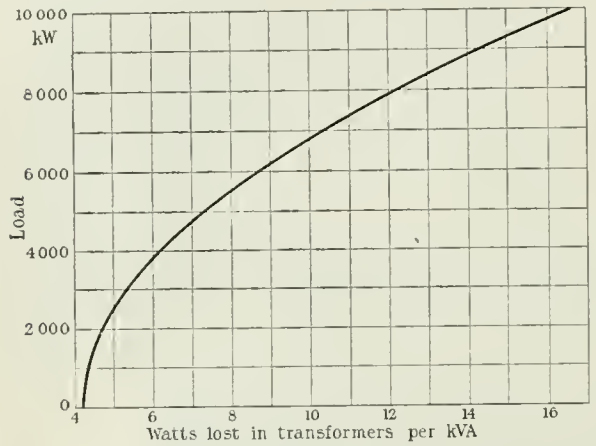


FIG. 17.—Curve of transformer losses.

The example chosen is from an undertaking having a high load factor. In the case of a lower load factor, the difference between the peak load and the equivalent continuous load would be greater.

Fig. 16 shows a temperature chart for transformers assumed to have a very large time-constant, equal to 10 hours. In the case of transformers, the final temperature is not proportional to the square of the load. It is proportional to the losses, which are made up of a copper loss that is proportional to the square of the load, and an iron loss that is independent of the load. Therefore the scale of ordinates is more complicated, and it is best to determine the transformer losses for different loads and let these be the vertical scale. The correspondence between transformer losses and load is shown on the curve in Fig. 17, and from this curve the loss figures can be obtained which should be taken for each portion of the temperature chart. From the loss figure given by the highest point on the temperature chart the equivalent continuous load can be obtained by reference to Fig. 17. The general form of the temperature chart for the transformers is much smoother than that for the cables. This is partly due to the large time-constant, and partly to the fact that changes in the load have a smaller effect than in the case of cables, because the losses producing heating only partly depend on the load. In consequence of this, the transformers show so considerable a temperature rise after 24 hours that they become appreciably hotter during the second day of running than during the first. The temperature chart has been continued for a third day, but it will be seen that the temperature rise is not much greater on the third day than on the second.

For the reasons given above, the equivalent continuous load for transformers is less than that for cables. Therefore, where an E.H.T. feeder and its step-up and step-down transformers are controlled as one unit, their continuous load capacity should not be the same. If the same safety of working is required both for the cables and for the transformers, the former must be rated higher than the latter. The heat capacity of an overhead line is so low that the equivalent continuous load is very near the maximum peak load. Transformers working on overhead transmission might, therefore, safely be rated very much lower than the capacity of the line.

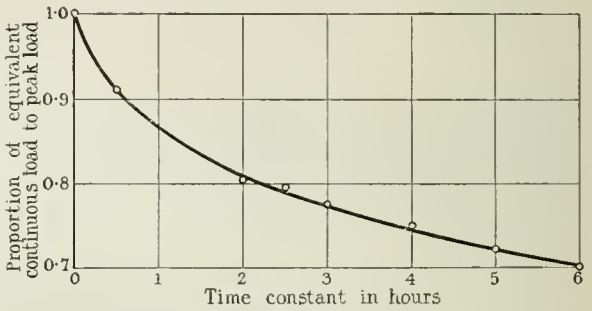


FIG. 18.—Proportion of equivalent continuous load to peak load as a function of the time-constant for load curve on 25th September, 1919.

Since the equivalent continuous load of a given load curve is a function of the heating time-constant of the cable, transformer, or machine, the safe loading under variable load conditions can only be determined if the time-constant is known. For a machine or transformer, the time-constant can be readily determined by a few temperature readings on a load test which does not need to be carried out at full load, nor to be continued until a steady temperature is reached. In the case of a cable, this constant depends both on the heat capacity of the cable and the method of laying. Some useful curves are given by Mr. Beaver in the discussion on Messrs. Melsom and Booth's paper before the Institution.* Apart from these curves, very little published information exists, and it seems worth while, in view of the importance of the subject, for extensive research to be carried out to find out how great the time constant of different types of cable is, under different conditions of laying. The present state of our knowledge about time-constants is too slight to enable us safely to make full use of the method of determining temperature rise described above. Since, however, a scientific

* *Journal I.E.E.*, 1911, vol. 47, p. 747.

consideration of the actual load conditions may result in a saving of 15 to 25 per cent of the cost of the distribution system, money spent on research in this connection would be well invested. The capital charges on this saving might represent a charge per unit that may be many times the saving in the cost per unit due to the use of economizers. It might be comparable with the cost of maintenance on the whole of the power-house plant, and greater than the cost of running all the power house auxiliaries.

If the cable capacity is based on the equivalent continuous load, a certain measure of watchfulness is essential. The author would recommend that temperature charts for various time-constants be taken at each season of maximum demand and whenever the nature of the load curve changes. Separate charts may be worth while for different parts of the distribution system or even individual feeders where the load curve may vary from the station load curve. In most cases the equivalent continuous load of the station load curve will, owing to the diversity factor, be higher than that on individual portions of the system, and outlying feeders working on a bad load factor may possibly be run at a higher peak-loading than feeders leaving the power house. So little work is involved in the preparation of temperature charts that there appears to be no reason why this should not be done at fairly frequent intervals.

(6) Tariffs.

Tariffs designed to obtain the best use from the E.H.T. distribution system will be of greater importance on super-power schemes than they have been in the past. A multitude of tariffs devised with this end in view are in operation, and anyone who collected and analysed the results obtained with them would be doing a most valuable piece of research work. The subject is too big and too important to be properly treated at the end of this paper, and a few general remarks must suffice.

Discriminating tariffs are only justified if they ultimately result in cheapening the cost of current to the average consumer. If they do not have this effect, a flat rate is preferable. For instance, it costs more in capital charges on transmission to supply a consumer far from the power house than one near, but a tariff that made the cost per unit a function of the consumer's distance from the power house would not be likely to reduce the cost of the distribution system and, therefore, it would not be a desirable tariff. On the other hand, the power factor is always under the consumer's control, and also the load factor to some extent. A tariff which encourages the consumer to improve either of these will therefore help to reach one of the electrical engineer's chief aims, which is to cheapen electricity.

For a moderately large system a fair average figure for the cost of generators, transformers, and distribution system may be taken to be £15 per kilovolt-ampere of maximum load. At a power factor of 0·8 this becomes £18·75 per kilowatt. An improvement of the power factor from 0·8 to unity would represent a capital saving of £3·75 per kilowatt of maximum demand. The average cost of apparatus or machinery for improving the power factor would come to very much less than this, and so power-factor improvement would very decidedly be an economic proposition on such a system. Load-factor improvement often costs nothing at all. It merely entails a little care in starting up heavy machinery.

If the distribution system is to have the full benefit of power-factor improvement, the apparatus or machinery must be installed on the consumer's premises. The cheapest method is generally that by which phase advancers are installed on each individual large motor. The drawbacks of divided responsibility and difficulty of supervision make it impracticable for the supply authority to install these, so a tariff discriminating enough to make it worth the consumer's while to improve both his load and his power factor best meets the case. In the author's opinion the tariff of a large supply undertaking ought invariably to discriminate between a good and a bad power factor. For small undertakings a discriminating tariff is rarely worth while. Wherever the nature of an industrial load is such that the consumer can improve his load factor, the tariff should also be based on maximum demand, that is, should discriminate between good and bad load factors.

Such a tariff is, however, of little use unless the consumer knows what to do about it. If it engenders distrust and gives the consumer the idea that somehow or other he is being charged an unfair price, it does positive harm.

Nevertheless, a scheme sound in itself should not be discarded because it is liable to be badly understood. The remedy lies in explaining matters properly to the consumers. A good power factor means cheaper electricity and is ultimately to the advantage of the consumer. It should not be impossible to make him understand that.

The time has come when electric supply undertakings should have publicity departments run by a well-paid staff. These departments should supply readable literature and personal help and advice. Among other things, it would be the duty of such a department to advise consumers how to keep down excessive peaks by adjusting the times of working of heavy machinery, and by exercising care in starting it up. It would also advise them as to the best methods of improving the power factor in each particular case. It is to the interest of the consumer, the supply undertaking, and the industries of the country generally that electricity should be used to the best advantage, and the supply engineer is the man in the best position to help towards this end. The cost of giving free advice to power consumers and generally keeping them informed of the possibilities of reducing their electricity bills would be a small item in the budget of a large supply undertaking, and would ultimately result, not in a reduced but in an increased revenue.

The author's thanks are due to his colleague, Mr. Carrothers, for help and advice in the preparation of this paper, and to Mr. A. E. Tanner, of Messrs. W. T. Glover & Company, for figures concerning the cost and load capacity of cables.

Discussion on the Papers by Mr. Woodhouse and Mr. Kapp before The Institution, 25 November, 1920.

Mr. P. V. Hunter : On page 86, Mr. Woodhouse uses this expression, "The transmission of electricity is, from an economic point of view, not a virtue but an expensive necessity." Now as a cable maker I feel that that is one of those things which might have been expressed differently; in fact, although it is difficult to give a direct negative to the statement, I think I can give quite a different meaning to it. The distribution of electricity is undoubtedly increasing in a remarkable manner, but there is certainly nothing inherently necessary in it. It is being done to help power station designers out of their difficulties. In order to obtain the best results it is found necessary to place power stations on riverside or waterside sites, and to generate power in very large amounts. The result is that electricity has to be transmitted in larger quantities and over longer distances than would otherwise be necessary. It seems to me, therefore, that it is not a fair statement of the case to speak of this increased transmission as an expensive necessity. On the contrary, considered from this point of view, transmission is a very necessary aid to economy, and, unless cable makers and those who are interested in that branch of engineering were able economically to provide the means for such transmission, it would not be possible to take advantage of the economies obtainable by designing power stations in this way. The next point I should like to comment on is the necessity for three or more pressures mentioned by Mr. Woodhouse. I am afraid this is unavoidable. When I was responsible for the design of transmission systems it was always my endeavour to work with two pressures only, that is to say a main pressure for transmission and a distribution pressure for consumers, and thus keep transformation to a minimum. Loads and distances have, however, now reached a point when this procedure is no longer economical, and it is necessary to use a superior transmission pressure for interconnecting power stations, as Mr. Woodhouse has suggested. My experience, however, is that distance and the number of feeding points do not necessarily decide the main transmission pressure. The main transmission pressure is an economic question and should be settled purely on what is the cheapest pressure at which energy can be transmitted per kilovolt-ampere per mile. The only qualification that it is necessary to make to this statement is that extremely high pressures may result in feeders which carry individually too much load for the circumstances of the district, but this is, I think, the only condition that limits one economically in choosing the highest pressures for which cables and other accessories are available. One arrives ultimately at the unexpected conclusion that the correct transmission pressure is settled, not by the distance, but by the density of the load. In every densely populated industrial area, where for instance the individual loads may be of the order of from 15 000 to 25 000 kVA per feeder, then a transmission pressure of 50 000 to 60 000 volts is undoubtedly the right pressure to use, and will show large economies in cost per unit. On the other hand, in a less industrial area, where 15 000 kVA would be the upper limit for an individual feeder, and where 5 000 to 10 000 kVA would be a more common load for a feeder, pressures of a lower order would be more economical. In the case of the lower pressures the question is complicated by the fact that at 10 000 volts step-up transformers are not required, which extends the economic field for this pressure in an abnormal manner, and it is difficult to show that any pressure below 50 000 volts is more economical than 10 000 volts. An argument which I have often heard used in connection with this question of transmission pressure is that, if 30 000 volts is sufficient from the point of view of voltage drop for dealing with the distances met with in this country, then 30 000 volts is correct and should be standardized for this country. I submit that this is entirely wrong and, if it is given effect to even in a limited way, will result in a large amount of money being spent needlessly. Mr. Woodhouse says, "The progress of engineering has generally passed through three phases : first the use of faulty apparatus, next the addition of elaborate protective devices, and finally improved apparatus and the simplification of protective devices." I think, however, that this sentence is liable to be misunderstood as implying that protective gear is no longer necessary, due to the improvement in the manufacture of equipment. It is true that this impression is largely negatived by the succeeding paragraphs recommending a protected transmission system, but it may be well if I deal with the subject a little more in detail. The adoption of protective apparatus as far as I remember it—and I think I was connected with the first protective apparatus used in this country or elsewhere—I am speaking now of discriminating protective apparatus—did not arise because of faulty cables, faulty transformers, faulty machines, or in fact any defects of manufacture. It arose from the fact that an accident occasionally occurred ; a cable or a transformer was damaged, and the results were much too widespread. It was desirable, therefore, to have some device or apparatus which would limit the results of a breakdown, and it was from those considerations alone that protective gear was first adopted. This condition holds to-day, only with added force, because the networks are larger and more complicated. I am very pleased to notice that Mr. Woodhouse points out that an entirely different set of considerations arises in choosing the pressure for overhead lines as compared with cables, and I think it is not sufficiently realized that an overhead line working at the same pressure as a cable is in no way comparable with it from the point of view of service rendered. I am not speaking now of such questions as reliability, but of the question of voltage drop, and although I mentioned earlier that the selection of cables did not depend on distance and voltage drop, I find that in the case of overhead lines, on the other hand, the voltage drop is a very pertinent matter, because,

owing to the reactance voltage of overhead lines, the impedance is excessive, and if they are loaded in the same way as cables the voltage regulation of the system as a whole tends to become unsatisfactory. The trouble cannot be overcome by increasing the section of conductors, as doubling the area of a conductor makes very little difference indeed to the total impedance at 50 cycles. The only way to obtain real relief is to increase the working pressure of the pole line, as this reduces the value of the voltage drop expressed as a percentage. I see, in Fig. 2 of Mr. Woodhouse's paper, that he has again brought forward the question of the variation of the losses with the shape of the load curve, and the fact that they do not vary with the load factor only. I think I am correct in saying that this was first brought to light by Mr. J. R. Beard in his paper on "Design of High-Pressure Distribution Systems" before this Institution,* and it has not had sufficient attention. In fact I have not seen it mentioned since, although it is a very important question in dealing with the losses of networks. I am pleased, therefore, that Mr. Woodhouse has emphasized the point. Mr. Woodhouse also touches on the question of dielectric losses. A great deal can be said on the subject, and it would require a special paper to deal with it adequately. I will say this, however, that from the point of view of their heating effect on the cables, I am satisfied that the losses in the dielectric will or should have little or no effect in limiting the working current. This does not perhaps agree with the guarantees obtained from cable makers for the carrying capacity of cables having similar sectional area, as the pressure increases. A 60 000-volt cable is guaranteed to carry materially less current proportionately than a 20 000- or 30 000-volt cable, but that is not because the dielectric losses heat the cable in one case more than in the other. The dielectric losses are larger, but, even at their worst, they are so small that they lead only to 1 or 2 per cent reduction in the load; in fact, the maximum dielectric losses which I should expect, even in a very high-pressure cable, would not exceed 2 per cent of the copper losses. Therefore, to make such a cable comparable in temperature with the lower pressure, it would merely be necessary to reduce the load loss by about 2 per cent. Cable makers, however, guarantee materially lower working currents for the higher pressure cables, not because of the losses, but because less is known about the working of 60 000-volt cables, and it is wiser therefore in the meantime not to push working loads to the same limits as in the case of, say, 10 000-volt cables about which there is very full knowledge and years of experience.

I should like to emphasize the paragraph on page 99 of Mr. Kapp's paper, as it seems to me to sum up the whole situation very admirably. He says: "A great deal of ingenuity can be expended on designing cable lay-out schemes, but as soon as one tries to make generally applicable rules, one finds that the problem is so indefinite and depends so much on individual cases that generalizations do not apply to it. One might as well try to devise a formula for playing chess." That is entirely my experience. I am satisfied that for many years to come the design of high-pressure distribution and transmission systems will be more of an art than a science, and we cannot therefore discuss it and consider it too often or too freely, because, after all, it is by the exchange of opinions that one arrives at a school in art.

Mr. H. M. Sayers: There are some novelties in Mr. Kapp's paper which are implicit and not explicit. There are one or two figures which seem open to question. The distribution capital for a 40 000-kVA station at the centre of a supply area extending over 5 square miles is given as £4 per kilovolt-ampere, which makes a total expenditure of £160 000. For the same 40 000 kVA, with a supply area extended over 40 square miles, the cable transmission capital or distribution capital is given as £6·5, or a total expenditure of £260 000. Assuming that these areas are circular and with a uniform load density, I should have thought that the cost of distribution would have been nearly proportional to the radii, but, worked out as the cost per kilovolt-ampere per mile, the smaller area is debited with a larger cost. On page 107 Mr. Kapp says: "A fair average figure for the cost of generators, transformers, and distribution system may be taken to be £15 per kilovolt-ampere of maximum load." I suggest that £15 is a rather low figure, though the plant involved excludes turbines, boilers, etc. If it is too low, then correcting it and increasing the figure will strengthen Mr. Kapp's argument for recognizing power factor and load factor in consumers' tariffs. The kernel of Mr. Kapp's paper seems to be the omission of Kelvin's law altogether as applying to E.H.T. distribution. Here is a paper on distribution that does not mention Kelvin's law, and yet throws it over. The overthrow of Kelvin's law is implicit in the paragraph commencing: "The above discussion does not take voltage-drop in the feeder or feeder losses into account. For the purpose of the rough generalizations dealt with here, the latter are too small to need considering"; and Mr. Kapp goes on to say that for this reason the most economical site for the power house is not at the centre of gravity of the load, whereas he implies that in low-tension systems it would be. The limiting factor, according to Mr. Kapp, for E.H.T. cables is heating. If that is so, it is worth while to consider why Kelvin's law does not apply. Kelvin's law says that the greatest economy in distribution is obtained when the capital charges on the copper cost of the cable are equal to the cost of the annual losses in the copper. It follows from that at once that, if the cost of a cable per ton of copper increases, the amount of energy to be lost in it to equalize these two items should also increase. That is, with a dearer cable reckoned as the cost of a ton of copper, the current density should be higher. That is quite true. Mr. Kapp means that for E.H.T. cables for 6 000 volts and upwards, Kelvin's law would give current density higher than is safe. I should have thought 6 000 volts a rather low figure at which to discard Kelvin's law. Kelvin's law gives the most economical current density which is universal all over the system, whatever the lengths of the feeders. A uniform current density is also the result of applying the heating criterion. For a given load, no matter

* *Journal I.E.E.*, 1916, vol. 54, p. 125.

at what distance, the author says he would put in a given cable. That is correct and according to Kelvin, except that the current density is lower. So there is no real difference between essential features of L.T. distribution and E.H.T. distribution. Therefore the best location for the transformer or power station from the point of view of distribution expenditure is the same in both cases. In L.T. distribution systems there is, of course, a rather early limit to the pressure drop allowable. Over 20 years ago I showed in this Institution that when such cases arise it becomes a question whether the current density should be reduced on the long feeders, or whether the long feeders should be individually boosted. Which is the more economical way depends upon circumstances. I then showed two or three practical cases in which one or the other was the better method to employ and why. Mr. Kapp gives a figure showing the price per kilovolt-ampere per mile for 6 600-volt cables of different sections. The smaller sections are naturally the more expensive. If one were applying Kelvin's law to those cables one would necessarily work the smaller sections at a higher current density. The same thing will apply with the temperature limitation, because a smaller cable will heat less for the same current density than a larger cable at the same pressure. Therefore, although there is some limiting point (really that of cost of cable per ton of copper and safe current density) beyond which one cannot apply Kelvin's law, the results are not so very different in form.

(*Communicated*): In the calculation of economical cable loading the item which varies most under different conditions of place, fuel costs, etc., is the cost of the energy dissipated in the cables. There also seems to be a considerable difference of opinion as to what proportion of generating costs is to be taken for this cost. The bare cost of fuel, etc., corresponding to the added units is too low. It is true that a considerable part of such station costs as salaries, wages and stores is not increased by the production of these "added units." On the other hand, since the cable losses are greatest at maximum loads, and are proportional to the square of the load—assuming the delivery pressure to be kept constant—these losses are added to the peak load, reduce the load factor and occupy a real part of the maximum plant capacity used. They should therefore be debited with the capital charges on such proportion of the plant. As a rough approximation it may be taken that these two items, i.e. the generating costs not increased and the share of capital charges, nearly cancel out, so that the cable losses should be priced at the full generating costs. The current density resulting from the Kelvin law varies inversely as the square root of the price per unit, e.g. the current density with the cable losses charged at 0·5d. per unit is 1·226 times the current density with a charge of 0·75d. per unit. It is quite sound to reckon the distribution as buying the lost units from the station at cost price. It is perhaps from taking too low a price that there is a somewhat general impression that the most economical cable loading results in too great a voltage drop, or too great a temperature rise for practical adoption. Mr. Melsom in the discussion remarked that one must take into account cable depreciation due to the loading. It seems to me that any loading which demonstrably shortens the life of the cable is too high and justifies a lower density. This, in fact, marks the dividing line between loading on an economical basis and on a heating basis.

From Fig. 1 of Mr. Woodhouse's paper it appears that the cost of 33 000-volt three-phase cables laid in the ground increases at the rate of £345 per ton of copper between the sizes of 0·05 square inch and 0·3 square inch. Taking capital charges at 10 per cent on this amount and the cost of energy as 0·75d. per unit, the economical loading of such cables is 511 amperes per square inch, which gives a drop of 23 volts per mile per conductor for a continuous load, or 100 per cent load factor. This is 153 amperes for a 0·3 sq. in. cable, and compares with 369 amperes for the safe load of 11 000-volt cable of the same size, from Mr. Woodhouse's formula, $I = 742\sqrt{a} - 37$, presumably with continuous loading. The density stated for 100 per cent load factor corresponds to the R.M.S. density for any actual load. Thus the load diagram (Fig. 13) in Mr. Kapp's paper has an R.M.S. load of 5 958 kW, an average load of 5 837 kW, and a maximum load of 11 250 kW, or a load factor of 51·8 per cent. The current density of 511 amperes per square inch given above will therefore give $511 \times 11\,250 \div 5\,958 = 964$ amperes per square inch at maximum load. If the cost of the wasted energy were 0·5d. per unit the economic loading would be 630 amperes per square inch with a maximum of 1 130 amperes per square inch on the peak of the load. It will be noticed that the equivalent continuous load to give the same temperature rise as the actual is much greater than the continuous load to give the same losses, on the assumptions embodied in Mr. Kapp's Fig. 10, i.e. a time-constant of 2 hours. Whether these R.M.S. and maximum load densities would be unsafe for a 33 000-volt cable will no doubt be elucidated shortly. Evidently the rates at which the safe density diminishes and the price of copper increases with higher voltages have an important bearing on the economy of increasing voltages. It means that there is some limiting pressure beyond which no economy in distribution can be attained with cables.

Major A. M. Taylor: On the question of the transmission pressure I must disagree with No. (7) of Mr. Woodhouse's conclusions, because this seems to be inspired by a clause on page 93, col. 2, which suggests adopting the *lowest* pressure of transmission which will satisfy the requirements of pressure regulation for the period chosen. It is my belief that we must adopt the very highest voltages practicable, even if they seem to be in excess of the demands immediately in view. I wish particularly to emphasize the importance of tie lines for power-transmission purposes between stations at even considerable distances apart. This is being most especially emphasized by the American Institute of Electrical Engineers, and is taking a concrete form in the Washington–Boston transmission line. The author's figures for a 33 000-volt transmission are not materially different from figures which I have obtained from another source, viz. £0·4 per kilovolt-ampere per mile, and I believe that if we could, by

the use of single-phase underground cables or other means, raise the voltage to 100 000, the cost would be brought down to £0·2 per kilovolt-ampere per mile. Working on this figure, the cost of a 60-mile tie line, intended to carry 30 per cent of the output of the station in an emergency, would, on a 35 per cent load-factor basis (of the tie line), work out to about 0·03d. per unit for the standing charge. This opens up great possibilities and, even if the 100 000-volt cable were not forthcoming, a combination of overhead line and underground cable could be adopted which would give even lower figures. There are stations in this country which at the present moment are so handicapped for generating plant that they could probably take on a 30 per cent additional demand if only this were available, that is, a 30 per cent additional revenue would be available for paying for the tie line. Added to this, the effect of running the whole of the generating station plant hopelesly overloaded for months together must unquestionably materially shorten the life of the generating plant, if it does not even precipitate actual breakdown, and therefore an extra depreciation charge on the generating plant must be credited to the transfer line, which would save such a condition. On the question of the use of pressures higher than 33 000 volts, I am almost certain that the makers of three-phase cables could not use 66 000 volts without incurring a maximum potential gradient of at least 45 000 volts per centimetre, and more probably 50 000 volts. Now, Clark's and Shanklin's tests* have shown that the resistivity of the insulation begins to decrease very rapidly when a maximum potential gradient of over 23 000 volts is incurred. It therefore looks as though the cable makers must be taking considerable risks in offering 66 000-volt cables, or else Clark's and Shanklin's tests are wrong. The latter's figures are, however, confirmed by the figures published by Hochstädter in 1915.

Mr. E. T. Painton : My only regret is that Mr. Woodhouse has not dealt in greater detail with the question as to when and where underground cables should be used in preference to overhead lines, and whether in fact E.H.T. underground cables are ever economically justified. Even for low-voltage detail distribution I believe the overhead system is generally considerably more economical, and I understand that for this reason overhead distribution in large towns is now making headway in this country. The removal of the present onerous conditions in securing wayleaves will materially assist this advance. In this connection it would be very interesting to know the ratio of capital invested in generating plant and in power and distribution mains on such a system as Mr. Woodhouse's. In the linking up of generating stations, and for other bulk-supply lines, there is one voltage which gives the most economical arrangement. This applies both to underground cables and to overhead lines, although Mr. Woodhouse states that for overhead lines there is no limit to the voltage which may be employed on the score of capital expenditure. With underground cables it is very often the case that when the length of the line is short the most economical voltage will be the voltage of generation, but with overhead lines it is quite possible that the most economical voltage will be considerably above the generated voltage, or, in other words, that the reduction in the cost of the conductors due to the use of high pressure will greatly overbalance the extra cost of transformers and switchgear in the case of overhead lines, but not in the case of cables. Mr. Woodhouse gives some general expressions showing how the cost of underground cables and overhead lines varies according to the voltage, but it is impossible to compare the expressions without some indication of the relative

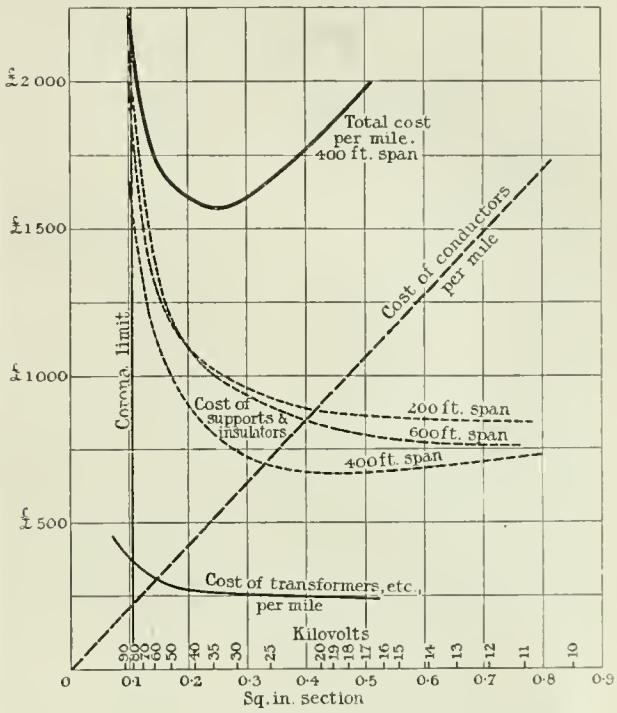


FIG. A.—Overhead lines to transmit 10 000 kW, 30 miles at various voltages. In all cases, two circuits on one line of steel supports, with aluminium conductors and pin-type insulators, have been assumed.

values of the constants A_v, A_0; B_v, B_0, etc. It is well known, however, that while the cost of an underground cable increases very rapidly with the voltage, the cost of an overhead line increases at a very much less rapid rate, and for that reason the most economical voltage for an overhead line is much greater than for an underground cable. In comparing overhead lines with underground cables in such a case, one has to consider the cost of a low-voltage cable with a high-voltage overhead line, and in these circumstances the cost of an overhead line may be a half, a third, or even a quarter of the cost of an insulated cable. Mr. Woodhouse states that one of the factors which limit the voltage in an overhead line is the minimum size of conductor for mechanical strength. It is not too well appreciated that the factor of safety in an overhead line depends on the relationship between the conductor size and the sag, and, provided the sag allowed is large

* *Proceedings of the American Institute of Electrical Engineers*, 1919, vol. 38, p. 663.

enough, a small conductor is quite as safe as a large one. As the sag in an overhead line increases with the span, it follows that to keep it within reasonable limits the span must decrease with the conductor size. In many cases it is more economical to use a small conductor on a small span than a large conductor on a long span, and it follows that the span length has a very material bearing on the total cost of the line and on the most economical voltage. This point is readily brought out by plotting a series of cost curves. By the use of Kelvin's law, as explained by Mr. Woodhouse, it is possible to obtain at once the most economical current density under any particular circumstances and, knowing the power to be transmitted and the load factor, it is evident that there is a definite relationship between the conductor section and the voltage. Choosing any particular span length it is then possible to calculate the cost of a complete line with different conductor sizes, and the appropriate voltage for that size, and, if the figures obtained are plotted against either the conductor area or the voltage, the curve obtained will be found to have a well-defined minimum, indicating at once the most economical voltage and conductor size to be employed. In order to illustrate the principle I have prepared a series of cost curves for a particular case which are shown in Fig. A. It will be seen that the base line represents a scale of sectional areas and to a different scale indicates the voltage to transmit the power at the economic current density. The diagonal line represents the cost of the conductors, which will be practically independent of the span length and increases directly as the sectional area. The three dotted curves represent the cost of supports and insulators for three different span lengths. The rapid rise of these curves as the conductor size decreases is due to the rapid increase in the voltage which accompanies the reduction in the conductor size. It will be noted that the cost with a 400-ft. span is much less than with a 600-ft. span or a 200-ft. span. Next, the cost of transformers and switchgear, which varies according to the voltage, is taken into account in the curve at the bottom of the figure. This curve is obtained by dividing the actual cost of the transformers, etc., by the length of the line in miles in order to get a figure for cost per mile. Finally, the curve at the top of the figure is the total cost of the line per mile for a 400-ft. span (which is apparently the best to use in this case) and is obtained by adding all the individual costs together. It will be noticed that there is a well-defined minimum to this curve, and in this particular case the most economical voltage is shown to be 34 000 volts, corresponding to a conductor section of 0·25 square inch. It may not always be possible to use the most economical voltage as indicated by this method, and it would be necessary to check the regulation of the line before adopting the figures determined. In some cases also the minimum value for the total cost curve may fall within the vertical line marked "Corona limit." This line marks the point where a further reduction in the conductor size accompanied by the appropriate increase in voltage brings the line within the conditions where a corona discharge occurs. In spite of these reasons for adopting some other than the most economical conditions as indicated by the cost curves, the method is very useful as giving a logical basis to work from. It is interesting to note also that if an increase in the power demands necessitates an increase in the power transmitted at some future date, this may be obtained by increasing the transmission voltage, and the same sizes of conductor and span length again give very approximately the most economical conditions. In conclusion I should like to add that Mr. Woodhouse is well known to have had considerable experience with both overhead and underground transmission, and his comments on the points I have raised would be of especial value.

Discussion (continued) on the Papers by Mr. Woodhouse and Mr. Kapp before The Institution, 9 December, 1920.

The President (Mr. Ll. B. Atkinson): Before calling on anybody to continue the discussion, I should like to describe a method of carrying out one of the operations mentioned in Mr. Kapp's paper, i.e. the operation of finding the final temperature of the cable from the diagram (load curve squared). This method is a mechanical one, as opposed to the method of integration by using tracings from curves, described in the paper. The instrument which performs the operation is called an "integraph." If one has a curve which shows the loading at any moment, and if the final temperatures for definite loadings be known, it is possible, by means of an integraph, to draw the actual temperature curve without any tracing or template curves. The integraph exhibited (kindly lent by Prof. A. R. Forsyth, F.R.S.) consists of an oblong frame on which are two carriages, one of which is quite free. The other is connected with a mechanism, the object of which I shall explain in a minute. There are two pencils, one on each of the carriages, which slide on the main frame. If the outline of a curve be followed by the pencil on the lower carriage, the pencil will, as it goes along, move the rods of the parallel motions. Underneath the other carriage there is a metal wheel with a sharp edge, the object of which is to prevent sideways motion; that is, the wheel will move only in the direction of its own plane. Therefore, along whatever curve it is moving there is a tangent along which, at any moment, it is going in a straight line. The wheel is guided by a tiller at the top; this moves it into any position, and when the parallel bars are moved, by means of the lower pencil following a curve, the tiller moves backwards and forwards, and sets the angle of the wheel down below. If a curve be followed with the lower pencil, the top frame begins to move across independently, forced by the wheel underneath, and the pencil guided by the wheel will trace a new curve. The height of this new curve at every point is the integral or sum of the ordinates of the bottom point, each multiplied by the short

distance dx through which it is moved. The result is that if, for instance, the lower curve represented the load on a cable (which was so arranged that no heat could escape) and in which a definite current was flowing, we should have moved the lower frame to a definite height; the top frame would begin to move off in a straight line, and at every point the height of that line would mean the temperature of that cable. We should have to perform no calculations if the zeros and constants were known. But that will not perform the operation when there is an escape of heat, and when the operation depends on what the escape of heat is. A modification of the instrument allows for the case when heat is escaping at a rate proportional to the temperature. That is to say, it will draw one of those heat curves which Mr. Kapp has given us. Nearly all electrical curves depend on the same property, that the angle which the wheel on the second carriage makes with the axis of x is fixed, not by its height from the base, but by the distance from the maximum value. Supposing, for instance, a current is started in a coil; the rate at which the current rises depends on how much electromotive force is not absorbed by the product, current into resistance. The rate at which a condenser charges is determined by the total electromotive force minus the potential to which the condenser has risen. In other words, the law is the same as Mr. Kapp has explained for the heating of a cable. The rate at which the heat is increasing is proportional to the production of heat minus the temperature which has already been reached. Therefore, a machine of that kind, with two floating frames, but with a rather different coupling, will draw those curves which Mr. Kapp wants. That is, if we have given the maximum temperatures for particular currents we can draw the load curve squared which he has given us, put an integraph on it, and draw out what the temperature will be at any hour of the day or night. (A description of this class of integraph will be found in articles by Mr. Ll. B. Atkinson, in the *Electrician*, 1889, vol. 1, pp. 702, 730; and vol. 2, p. 29.)

Mr. C. J. Beaver: I think the subject of distribution has not hitherto received the scientific consideration which it deserves, and I am glad to see that it is now coming into its own under the influence of the super-power schemes. In these two papers we have a combination of broad outlook and ingenious method. My personal interest is less, however, in the broader questions of distribution design than in detail points relating to the means of distribution. In the first paragraph of that section in Mr. Woodhouse's paper which is headed "High Pressure Cables," it is stated that, on account of dielectric stress and so on, a minimum section of 0·05 square inch appears necessary for pressures above 10 000 volts. As a matter of fact, that word "necessary" should be "advisable," because nowadays we do make under the stress of competition cables of smaller sectional area than 0·05 square inch for pressures higher than 10 000 volts. With regard to the third paragraph of the same section, in which the questions of the limiting factors of weight and diameter in relation to maximum size of conductor and the liability of cracking the insulation in large heavily insulated cables are considered, I think there is no doubt that the weight limit will arrive before cracking of the dielectric occurs in well-made cables. With regard to the next paragraph, while it is generally necessary to increase the thickness of the dielectric as the voltage goes up, it is usually possible for given values of working voltage and maximum dielectric stress to reduce the actual thickness of dielectric as the size of conductor is increased. Some succeeding paragraphs deal with the matter of permissible current densities in high-pressure cables. While, perhaps, we have insufficient experience in the matter to standardize the current density on very high-pressure cables, and while it depends somewhat on the methods of laying those cables, my own opinion is that under comparable conditions it will be found possible to increase the current density as the working pressure increases, owing to the relatively greater surface areas involved; it depends very greatly on the thermal properties of the component parts of the cable itself, and of its surroundings. With regard to the effect of dielectric loss on the rating of the cables, it is somewhat interesting to compare American rules on that particular point which are based on a maximum cable temperature of 85° C. with a deduction of 1 degree C. for every 1 000 volts working pressure. It will be seen that the result will be to reduce the permissible temperature of, say, a 30 000-volt cable by 30 degrees. Assuming that dielectric heating is the reason for that particular formula, the comparison with British manufacture is very favourable to the latter, because the temperature allowance necessary under British practice is merely a fraction of that indicated by the American rule, something of the order of one-tenth. That is to say, the extra heating due to dielectric losses in well-made British cables for, say, 30 000 volts working pressure over and above that due to I^2R losses at normal current densities, is only about 3 degrees C., and this under conditions of dielectric stress which American authorities tell us should be fatal. In the eighth paragraph of the same section is the statement that "the dielectric losses are proportional to the product of the working pressure and the charging current." I think that would have been more explicitly stated as the product of the working pressure and of the in-phase portion of the charging current, which corresponds to the irreversible energy that Mr. Addenbrooke has explained to us. Further, with regard to the matter of the dielectric heating, although it is correct that it generally varies as the square of the pressure, I think the more important variation is that with temperature, and it would probably be found that various makes of cable would vary somewhat in their loss/temperature curve. With regard to the maximum size (0·25 square inch) mentioned further on, it may be of interest to know that we are now making 0·3 square inch 30 000-volt cable in large quantities. A point to note in connection with such large sizes is that they should have substantial lead coverings. The tendency to-day is to cut these down to a somewhat dangerous extent. With regard to the mention of single-core cables as an alternative to 3-core cables when very high

pressures are entailed, I would point out that, although they relieve some of the limitations of the 3-core cables, they also entail some special considerations of their own, such as the minimizing of the circulating current in the sheaths of the cables. On page 93 of Mr. Woodhouse's paper there is a reference to the amount of power per cable. "The failure of a main conveying, say, 50 000 kVA is not lightly to be contemplated." It is not so very long ago that there was a Board of Trade Regulation which fixed 1 000 kVA as the maximum power to be conveyed by a cable In connection with the matter of protective gear, which is entailed by these considerations of power, both the authors have referred to the "swing of the pendulum" in connection with the matter of protective gear, and I should like to make the suggestion that, at the time when protective gear came into fashion, and during the period in which it has been very largely developed, practically nothing was known of what was happening inside the cables. To-day we have, or have the facility for getting (by means of the test sheaths now commonly embodied in high-pressure cables), a very different perspective of the whole matter; that is to say, if we can get periodical tests which show the internal condition of the cables, surely the whole matter has taken another aspect, and the requirements of the case have, in my opinion, entirely altered. It is, of course, a matter for the judgment of the operating engineer as to the extent to which this reliable information as to the internal condition of his cables reduces the necessity for costly and elaborate protective gear, but it seems at least common sense to review the whole situation under the present conditions.

In Mr. Kapp's paper, with regard to Fig. 8, the author suggests that 0·075 sq. in. cable is likely to become the standard in future, and he makes comparisons between various sizes of cables. I presume that, in comparing the 0·075 and the 0·25 sq. in. cables on this curve, he has allowed for two cables of the smaller section which would carry about the same current, about 330 amperes. Even so, there is one aspect in this connection which does not appear to be taken into consideration. Taking 600 volts as the maximum permissible drop, the two 0·075 cables would be usable up to about $3\frac{1}{2}$ miles, whereas the 0·25 cable would be usable up to about 6 miles; in other words, the power loss in the two small cables would be about 70 per cent more than in the larger cable. With regard to the last part of Mr. Kapp's paper, at the bottom of page 106 and the top of page 107, I should like to endorse his suggestion with regard to the importance of research on the time-constants of cables for very high pressures and, I would add, the allied subject of dielectric heating. I think that any research work which is to be done should attack the high-pressure end of cable work in preference to the other end, about which we have more information.

Mr. P. Dunsheath: In dealing with load factor and losses on page 89 of his paper, Mr. Woodhouse states that the dielectric losses in a distributing system are approximately constant. Now it is well known that dielectric losses are primarily a function of the dielectric stress, and constant pressure *would* produce a constant loss, but only if the temperature remains constant, as Mr. Beaver has pointed out. It has been shown by many experimenters that dielectric losses are as dependent on temperature as on stress, so that in the case of a cable where the temperature is largely decided by the amount of current flowing, the dielectric loss becomes a function of the current as well as of the pressure. M. Swyngedauw announced in the "Revue Générale de l'Electricité" about six months ago the results of some tests on a 3-core cable which showed that the dielectric losses decreased with rise of temperature, and M. Rennesson has more recently announced in the same journal (1920, vol. 7, p. 579) the results of an interesting series of tests illustrating this point.

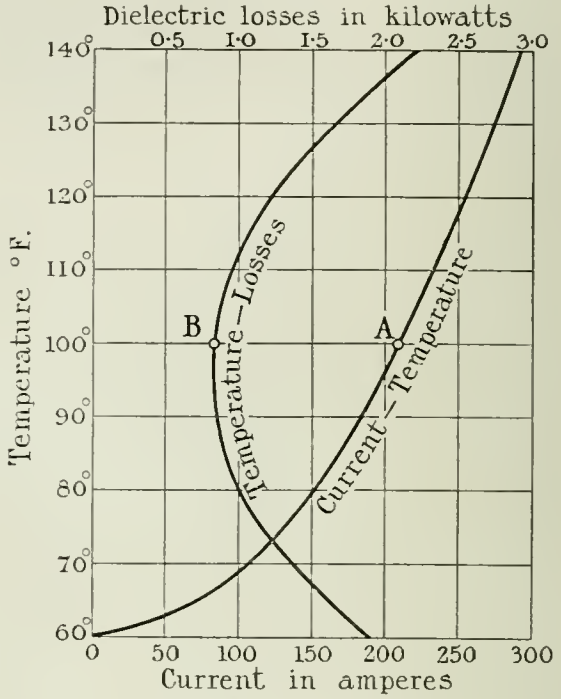


FIG. B.—Curves showing temperature rise for various currents, and the dielectric loss.

The latter investigator has shown that as the temperature rises the losses decrease to a minimum, and then begin to grow again, the lowest point being at about 90° F. In order to connect the value of the current in a buried cable with the change of dielectric loss due to the resulting temperature rise, I have taken some loss/temperature results obtained recently on a 0·2 sq. in. 33 000-volt, 3-core cable, and combined them with heating figures obtained from the formula given in Dr. Russell's book on "Cables and Networks." The results are shown in Figs. B, C and D. One curve in Fig. B gives the calculated temperature rise for various currents, and the second curve shows the dielectric loss at these temperatures. By connecting points on the two curves for separate temperatures, we get Fig. C. It will be seen from Fig. C that, as the current on the cable is increased, the losses fall slowly to a minimum and then rise more rapidly. Curiously, this minimum

point appears to coincide approximately with the current that one would recommend for the cable in question. The curve seems to suggest a rather alarming state of affairs, but it is not as serious as it appears. Fig. D shows the curve from Fig. C reproduced on a different scale to make the dielectric losses comparable with the I^2R figures, and from this it will be seen that they are far from constant as Mr. Woodhouse stated, but over the working range, say from 0 to 260 amperes, their variation amounts to 100 per cent or so. However, they are negligible in comparison with the variation in the total losses.

The method, developed by Mr. Kapp, of dealing with intermittent rating, marks a distinct step forward in a difficult part of the subject of current-carrying capacities, but he has made the problem so simple that there is a danger of our overlooking effects which are by no means negligible. Local factors, such as

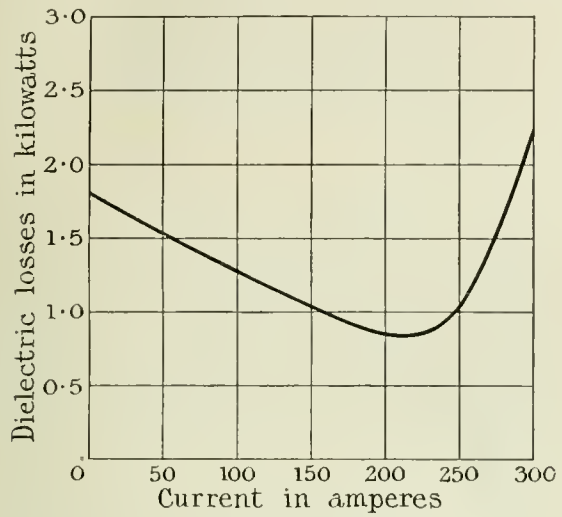


Fig. C.—Curve obtained by compounding the two curves in Fig. B.

variations in depth of laying, and proximity to heat sources, as well as such seasonal factors as ground temperature and moisture of soil, affect the rating enormously. I am afraid the author's statement, that a mains engineer knows what continuous load his cables can stand, is open to question. He may have a figure which he adopts as a standard, but very few have gone to the trouble and expense of fulfilling all the various conditions for scientifically verifying such a figure. Costly experience, of course, sometimes provides the necessary knowledge. Turning to the details of Mr. Kapp's levelling-up method, I think there is a weakness in his adopting a scale of "kilowatts squared" for his ordinates. The method requires a vertical scale of temperature, and the temperature rise of a buried cable is proportional, not to the square of the kilowatts carried, but to the square of the current, and what is also very important, to the actual resistance at any temperature. This means that power factor and copper temperature coefficient must be considered. Neglect of the latter alone in calculating current-carrying capacity may introduce an error of the order of 15 per cent in the result. Mr. Kapp draws a template diagram for maximum loading, and then uses greater or smaller parts of the same diagram for lower values of the loading. In the conditions of lower loads, the copper coefficient has a smaller effect, so that the actual current, or as Mr. Kapp prefers to put it, the kilowatts for a given temperature, will be higher than given by the diagram. The effect is to introduce an error which would be cumulative from left to right across the diagram, and, in order to avoid this error, it will apparently be necessary either to work with a tempera-

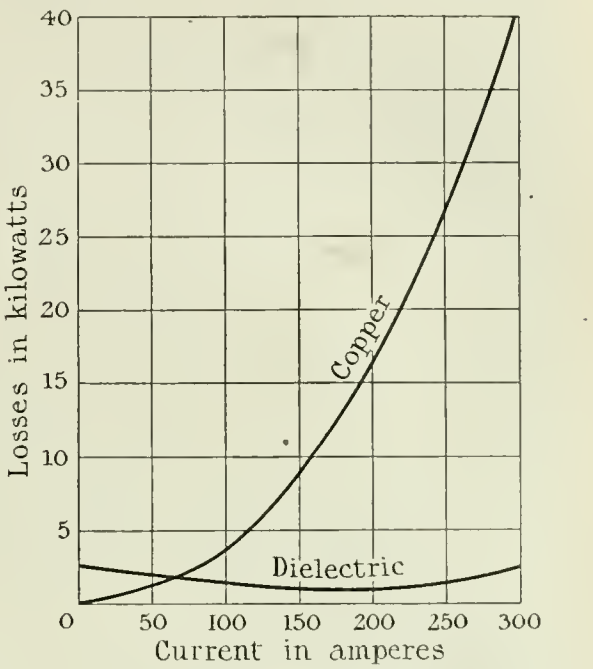


Fig. D.—Curve in Fig. C reproduced to make the dielectric losses comparable with the I^2R losses.

ture scale on the ordinates or to employ a series of templates for different parts of the diagram calculated for different copper-coefficient effects.

Mr. P. Rosling: Mr. Kapp refers to the 30 000-volt scheme, and also to 6 600 and 11 000 volts. It would be well if pressures could be standardized. Is it necessary always to have a 10 per cent increase over the real supply pressure, or could it be taken as 1 000 volts over the round figure of 20 000, 30 000, 40 000 or 50 000? Referring to Mr. Hunter's remarks as to super-tension cables in which he intimated that the cable makers would only give a reduced figure for the super-tension cables for current-carrying capacity because they did not know much about them, this is not correct in all cases. We can say under known circumstances what temperature rise a cable will have, but we do not know what all the circumstances are in which the cables will be used, and, knowing the losses due to hysteresis at the higher temperatures, it is only reasonable that makers should be more conservative for super-tension cables than for ordinary cables.

I think it practically resolves itself into this. We can give the users a definite temperature which should not be exceeded, but we cannot tell them what current will give this temperature unless users can give us all particulars of how the cable is to be laid and used. At central stations there are instruments for measuring everything in connection with the station, but nothing for measuring the temperature rise of the cables. I think there should be thermo-couples or resistance coils at different points throughout the length of the cable, connected to the switchboard, so that at any time the switchboard attendant can see what temperature a cable has at any particular point. Cases have been known where cables were laid over a baker's oven, or over condensing water pipes. With super-tension cables that might have a very serious effect. As Mr. Dunsheath has pointed out, there are the differences in local conditions; the dryness of the soil, the difference in temperature in winter and summer; in some places the cable has to be near the surface; and there may be the sun temperature to be considered. If four or five places on a run of perhaps 10 miles were connected to the station, the engineer could load his cable up to the maximum, which might vary with the time of the year, and so get the full advantage of his super-tension cables. Mr. Woodhouse mentions the question of bending. I think cable makers should take a stand upon that matter. The ordinary bending test should not be enforced on the super-tension cables; it is too severe, and a much less severe test will serve the purpose and more correctly represent what the cable will have to stand in practice.

Mr. S. W. Melsom: Previous speakers have already pointed out certain limitations of Mr. Kapp's theoretical treatment of the case of the problem of cable heating. Dealing first, however, with Fig. 14, which is based on the assumption that the heating and cooling curves are identical, I have some information which bears out Mr. Kapp's assumption. Fig. E shows the actual heating and cooling curves obtained with a 0·3 sq. in. concentric cable laid direct in the ground. The actual heating and cooling of the cable, as observed, is shown in the full-line curves. The cooling curve is again shown in the upper part of Fig. E, in such a position that the distance above the heating curve is twice that of the cooling curve from the datum line. Thus, the dotted equidistant line becomes the asymptote of the two curves. In addition, the upper dotted line is reflected over to show its close agreement with the heating curve. The curves therefore bear out Mr. Kapp's theory, that the time of cooling is approximately identical with the time of heating. I was particularly interested in the method of integration of the load curve of a cable used by Mr. Kapp, and the integrator shown by the President. Both are ingenious, and will undoubtedly furnish a fair indication of what is happening inside a cable. There are, of course, limitations to the application of either of the methods, some of which have been pointed out by other speakers, but the most difficult is the difference due to the heat being radiated from other cables in the immediate vicinity, which may be affecting the particular cable the temperature of which is being estimated. Such an effect would not be shown by the load curve, and would be most serious where a number of cables are laid up together. It seems certain, in view of the great and increasing importance of the whole question of the heating of cables, that instruments must be evolved which will enable an engineer to obtain a curve showing the actual heating of any particular cable. Some two or three years ago I adapted a Callender recorder to the purpose of measuring the actual temperature rise of a cable carrying direct current, and the method worked very well. The general principle of the Callender recorder is well known, and it is not necessary for me to describe it here. The modifications to suit the case of a cable core are shown in Fig. F, where, as will be seen, a shunt is put in series with the cable, the shunt and the cable being two arms of a resistance bridge. The other arms are made up of the recorder slide-wire

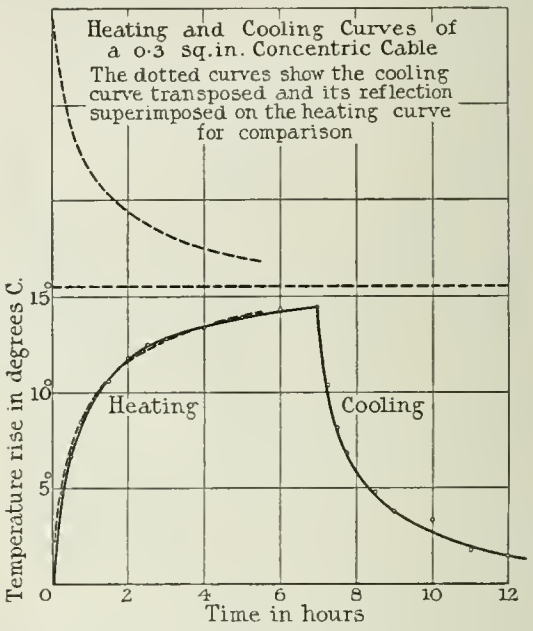


FIG. E.—Actual heating and cooling curves obtained with a 0·3 sq. in. concentric cable laid direct in the ground.

with resistance R suitable to the cable, and r so adjusted that each division on the chart corresponded to a change of temperature of 1 degree C. An actual record obtained from a cable in service is shown in Fig. G. The load of the cable is plotted on the chart; had the instrument been designed for the purpose, there is no doubt that it could have been arranged to record both current and temperature. The recorder when used in this way is not absolutely accurate, being subject to an error, owing to the conditions of use, of plus or minus 5 per cent. Such an instrument could, however, be used on any direct-current cable having a pilot wire, and will give much better results than any method of integrating a load curve would give. The question of obtaining actual temperature curves for 3-core cables, using alternating current, is much more difficult, but it is being tackled in more quarters than one, with fair prospects of success. Other methods that have been

evolved as a result of the Research on the Heating of Buried Cables, such as obtaining the temperature of the core of the cable by measuring the temperature of the lead sheath and allowing for the temperature gradient inside the cable, would be quite satisfactory for continuous and fairly constant loading,

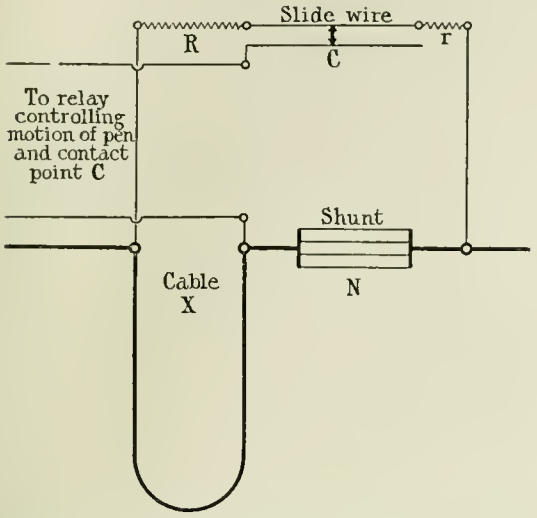


FIG. F.—Method of arranging a Callender recorder to suit the case of a cable core.

but the time-constant of the cable rules out such a method for any other conditions of loading.

Mr. Woodhouse, on page 92 of his paper, gives a formula for the safe rating of an 11 000-volt cable. I should be glad if he could let us know upon what temperature or other conditions that formula is based. Is it based on Kelvin's law, or on some increase of temperature which Mr. Woodhouse may be able to give us, and which will help in checking results we have obtained in other ways? Mr. Sayers referred to Kelvin's law in his remarks, and seemed concerned because he thought that it had been thrown overboard. I think the law is quite sound, and that Mr. Kapp is perfectly right in his use of it. However, I think that Mr. Woodhouse, as he has stated it, has not put sufficient weight on the depreciation factor of a cable. It is probably quite true that, if one takes the first cost of the cable and the annual capital charges, and compares them with the watt loss, one may arrive at the most economical size of cable, that is, if depreciation be omitted. I think, however, if one includes a factor which Kelvin included, namely, depreciation, it will be found, as Mr. Kapp has apparently discovered, that temperature is the only factor which need be taken into account, and therefore the comparison of the actual watts with the first charge of the cable is largely swamped, in the case of high-tension cable, by the question of depreciation.

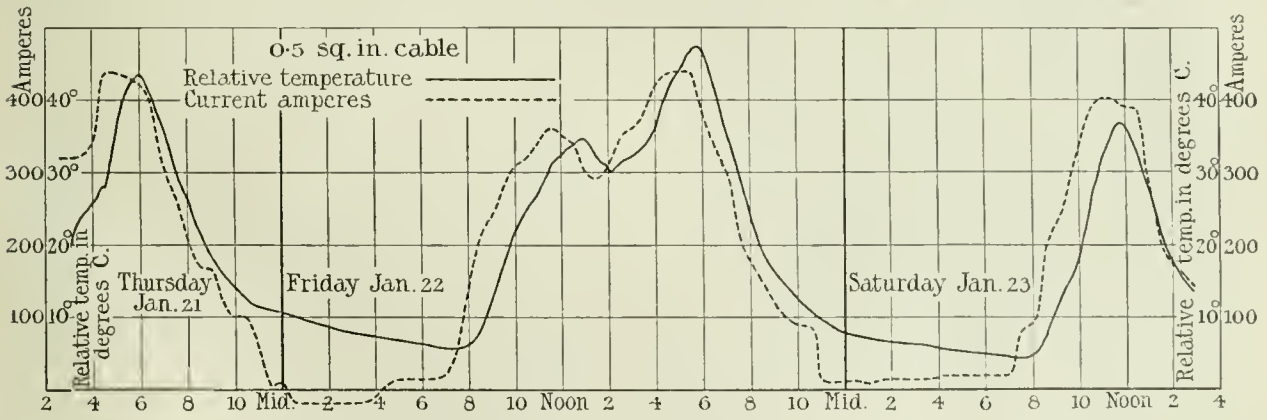


FIG. G.—Temperature and load record of a 0·5 sq. in. cable.

Mr. G. L. Addenbrooke: When I prepared my lectures on "Dielectrics" two years ago I found it absolutely necessary to make an historical survey, beginning with the work of Cavendish. For immediate purposes there is a book which seems now to be forgotten, but which is the best book on the subject so far as my knowledge goes in our own or any other language up to the present day, and that is Gordon's "Electricity and Magnetism." The circumstances under which it was written were that Gordon was one of that small band of enthusiastic pupils of Clerk Maxwell who seized the idea which grew out of his work, connecting the square of the refractive index with specific inductive capacity of a substance, and tried to see whether this suggestion of Maxwell held true or not. Chiefly in England, but also on the Continent, the work of Maxwell attracted attention amongst a very small band of workers, and the first thing to do was to get the correct dielectric constants of substances in order to see whether the fundamental conception was correct. The latest edition of Gordon's book gives the history of practically all that was done up to 1891. As regard the work of Cavendish, he was the first to measure dielectric constants with accuracy. About 1776 he made a series of measurements of the dielectric capacity of certain glasses. Those measurements are given in his own words in Gordon's book. At that time the very term "specific inductive capacity" was not known, but Cavendish gave with great precision sufficient details for Gordon to be able to work out

his results into modern figures. Cavendish made great discoveries in other lines and, as he must have been puzzled by these results, his account of them was put on one side as he probably thought he would have time to make some more experiments and verify his results before he published them; but, as in so many cases, he did not publish them, and they remained unpublished at his death. Amongst his other papers they ultimately found their way to Cambridge. Maxwell, working in other directions at these very problems that we are now at work on, while searching in Cavendish's papers, found those papers, but they had remained unknown for nearly 80 years, when Maxwell found them. In the meantime, in 1837, when overhead telegraph and submarine cable work was just beginning, and the capacity question cropped up, Faraday was making experiments on the connection between static electricity and current electricity and, as the result of some brilliant experiments which are illustrated in all the textbooks, he furnished the first rough list of dielectric constants; but as a matter of fact I do not think these are so accurate as those of Cavendish 80 years before. The first Continental observer, as far as I know, to take up Maxwell's discovery, and to go into the question of dielectric constants, was Boltzmann. His work is also more or less quoted in the textbooks, especially his work on gases, which is the standard now. He announced his discoveries in general terms at the Vienna Academy, but not very much was said about them. Gordon, knowing Boltzmann's ability and work when preparing his book, wrote to Boltzmann who was good enough to send him a special memorandum showing how he arrived at his measurements of capacity, and this is included in Gordon's book. Those measurements were made by a method which has only lately been revived in another form, that is, by hanging spheres of dielectrics and noting the attraction on another charged sphere. In Gordon's book there is what has always appeared to me to be one of the most beautiful pieces of reasoning that I have ever come across, the memoir showing how Boltzmann deduced from such measurements the dielectric constants or specific inductive capacities of a number of materials, and also at the same time obtained clear evidence of the other actions in dielectrics which are now beginning to attract so much attention in connection with the later advances in electric transmission in nearly all its different aspects.

Mr. H. Brazil: In any scheme, the cost of the E.H.T. cables is a considerable proportion of the total cost, and it is essential that the full value for this expenditure be obtained. The carrying capacity of these cables becomes, therefore, of very great importance, and we cannot afford to run on the old basis of 1 000 amperes per square inch, nor in the case of existing cables—where the alternative to running them higher is the laying of fresh cables—can we even stop at the higher figure given by the I.E.E. Wiring Rules. Let us take, for example, a 0·1 sq. in. paper-insulated, 3-core, 10 000-volt cable. The figures given in the I.E.E. Rules are for single cables, and the current there given must be reduced in the ratio of 1 to 0·88 for 3-core cables. This gives a figure of 168 amperes as the safe carrying capacity for a 0·1 sq. in. cable, the temperature rise allowed being 50 degrees F. Mr. Woodhouse on page 92 gives a formula for an 11 000-volt three-phase cable, and the safe carrying capacity calculated from this formula is 198 amperes. Again Mr. R. W. Atkinson in the *Journal* of the American Institute of Electrical Engineers (1920, vol. 39, pp. 831 and 839) gives as a safe maximum operating temperature 85° C.—E, where E is the R.M.S. voltage in kilovolts between conductors. For a 10 000-volt cable this works out at 167° F., and if we assume the temperature of the earth to be 50° F., this means a rise of 117 degrees F., which would permit of a higher current even than that which Mr. Woodhouse's formula gives. There is still some confusion as to whether it is right to take the rise of temperature or the maximum temperature when determining the carrying capacity of a cable, but there is no doubt in my mind that the maximum temperature is the correct figure to take. I would suggest, with cables laid in the ground, the temperature of which cannot vary much, and is lowest in the winter when the demand is greatest, that we should fix a maximum temperature to which the cables should be allowed to attain, and rate the cables at the current which would produce this temperature. Whether this temperature shall be taken as 167° F., the figure given in the American *Journal* for 10 000-volt cables, is a matter for discussion, but judging by the maximum temperature at which the E.H.T. windings of machines are run without apparent injury, I do not think this figure is prohibitive. It may be argued that the joints would give trouble, but I suggest that, if well made, a joint with a given current through it will rise in temperature very much less than the cable itself. A test I have recently made with a short length of 10 000-volt, 0·1 sq. in., 3-core paper-insulated cable with a wax-filled joint gave the following results: Final constant temperature after three hours 135° F. on the cable itself; 106° F. on the joint, air temperature 80° F. Another test with 280 amperes gave cable temperature 203° F., joint temperature 130° F., air temperature 80° F. These tests were taken in air, but they are instructive as showing the small temperature rise on the joint as compared with that on the cable. Finally I should like to ask Mr. Woodhouse how his formula is obtained, and whether he has any record of the maximum temperature attained by 10 000-volt 3-core cables laid in the ground, when carrying a maximum current as determined by this formula.

Mr. W. M. Selvey: There are some points in these papers which might perhaps be emphasized a little more than the authors have done. Mr. Woodhouse says on page 87: "For example, the 2 000 volts and 10 000 volts which the author has adopted in Yorkshire." I want to point out the extreme value to the industry of that pressure of 2 000 volts. Electrical engineers cannot get away from the question of cost, and it is a fact to-day, with the improvement in our electrical apparatus, especially in motors and switchgear, that there are very few industrial jobs, except the smaller ones, that cannot adequately be carried out entirely, with the exception of a little lighting, on a pressure

of 2 000 volts. Without calculation one does not realize the benefit which Mr. Woodhouse has conferred on Yorkshire by supplying generally at 2 000 volts. One can take a standard motor, say 50 h.p., which is a very good size, and wind it for 2 000 volts, and it will easily stand a flashing test with 8 000 volts. It is the most reliable piece of apparatus obtainable ; the current is low, and the pressure is high. That, I think, is the direction in which improvement will be obtained. I am buying very largely common ironclad switchgear for controlling this kind of installation, and a week or so ago the makers of an industrial installation did actually put 20 000 volts on a 2 000-volt switch. That, combined with the fact that a company or undertaking is willing to offer 2 000 volts, is a most material fact in our progress. A friend of mine who was concerned in putting in a big industrial installation, which ought to have gone on the public mains but which did not do so, told me he was installing cables and motors throughout for 500 volts, and, on my asking why 500 volts and not 400 volts, which would be the pressure of supply in his district, he replied that the saving on cables alone would amount to several thousands of pounds. The saving is immensely greater with 2 000 volts. I suggest that that is a direction in which the industry can do a great deal more. Electrical engineers are great readers, and I want to suggest to them for consideration that, when they are thinking about these big new subjects, they should read again what they have no doubt read many times—that is, Edgar Allan Poe's " Tales of Mystery and Imagination "—and consider very carefully the arguments there put forward as to the way in which one can lose the big view by meticulous care in considering the details. Mr. Woodhouse says on page 87 : " In such a case, however, it would seem inadvisable to interconnect the higher pressure transmission mains of the system, because of the complications which might arise. The solution most favoured is the division of the power distribution network into a number of sections, each of 20 000 kVA or more, and fed from an interconnected transmission system." I submit that that is a most profound statement, if it be followed to its logical conclusion. Later on he says : " A number of interconnected stations being assumed, the problem of distribution is much simplified, because the transmission mains may be utilized to provide additional feeding points on their routes between stations." Now the expression " transmission mains " is quite wrong, and yet it is inevitable. A transmission main is one that transmits, and cannot be used for anything else but transmission, and a new technical term for the main transmission is needed. I suggest that the transmission and the apparatus which is used for transmission are totally and entirely distinct from that used for distribution, and need an entirely distinct set of technical terms. I think that it is most important that we should have a glossary of these technical terms, because the transmission mains that we are going to see are something entirely different from the distribution mains which Mr. Woodhouse is discussing.

Mr. Kapp, on page 99 of his paper, says : " A great deal of ingenuity can be expended on designing cable lay-out schemes, but as soon as one tries to make generally applicable rules one finds that the problem is so indefinite and depends so much on individual cases, that generalizations do not apply to it." Then he goes on to write a most valuable paper based on certain generalizations that are convenient. He is perfectly right in so doing, but I contend in this connection that, as regards the main transmission system to which we are looking forward in the future, the main transmission and the problems belonging to it are comparatively simple, because they will have to be built up on more or less existing conditions as to distribution. There are not many of these trunk feeders or main transmission mains ; they will have to be considered extremely carefully. They can never possibly be interconnected except through certain definite apparatus ; they must be laid on some theory which admits of spare copper ; there can never be any question of 100 per cent. I doubt if we can get more than the time-constant of 63 per cent, but I suggest something like that. From the point of view of the Institution, as mentioned by Mr. Brazil and others, if one takes those two diagrams, Fig. I of Mr. Woodhouse's paper and Fig. 8 of Mr. Kapp's paper, it will be seen that both of those curves, while telling us something, are also to some extent incomplete, and yet they are the very crux of the whole argument that has been going forward in both papers. If to the matter contained in Mr. Woodhouse's curves could be added by Mr. Kapp the rate at which one has to depart from Kelvin's law with a large cable, due to heating, and from this plot a series of curves for all these voltages which Mr. Woodhouse has taken, we should have in one sheet of curves a complete record of the state of our knowledge of cable economics at the present time.

Mr. J. R. Beard : Dealing first with Mr. Woodhouse's paper, it will be noticed that he emphasizes the necessity for step-by-step development. This is a point which is continually cropping up in designing distribution networks, as there is frequently such a large difference between the cost of laying a larger cable immediately to provide for future requirements, and the cost of re-opening the ground to lay another cable when the requirements actually call for it. Fig. H gives some simple curves which may be of assistance in settling whether or not such expenditure for the future is justified in individual cases. These are based on the assumption that the compound interest and depreciation on such immediately non-productive provision should properly be considered as debitable to capital and added to the present extra cost over the non-productive period. The total thus obtained should obviously not exceed the capital cost which would be incurred by making the additional provision at the time it is actually required. I am glad to note that Mr. Woodhouse emphasizes the importance of considering the load factor of the losses. His results agree very well with those I obtained in my own paper, viz. that on an average commercial 50 per cent load factor the load factor of the losses is about 35 per cent. It is, however, very difficult to convince many practical engineers that it is worth putting in copper to save losses, and still more difficult to convince them that

the cost of the losses is really greater than the cost of the average units supplied to consumers. I should be glad to know if Mr. Woodhouse, on his own undertaking, feels himself justified in spending capital to save distribution losses, and if so on what basis. On page 88 Mr. Woodhouse gives a formula for the cost per mile of cable, from which it will be noted that he assumes that the factor dependent on the area of the core is a constant for any voltage of cable. This he shows graphically in Fig. 1 by the various lines being parallel to each other. His figures for the lower voltages are stated to be pre-war prices, and, if the cost of cables is assumed to have doubled during the war, and his figures are re-plotted on this basis, it will be noted that the lines of the lower voltage cables are converging towards the lines for the higher voltage ones, which is contrary to what one would expect. However, I am not inclined to place very much reliance on the figures for the very high-voltage cables, as these are not yet manufactured in sufficient quantity to put their design and price on a commercial basis. In my own work I have found that, for voltages up to 11 000, it is correct to state that the curves showing the relation of cost with size of conductor are practically parallel lines, but for 20 000 volts I found there is a very appreciable divergence. Fig. J shows these figures based on present-day prices, and it will be noticed that the increment cost of 20 000-volt cables (the constant B_c in Mr. Woodhouse's paper) is 50 per cent greater than that for 6 600-volt cables. If this figure is applied to his equation for current density on page 91 it will be found that it economically pays to run 20 000-volt cables at $\sqrt{(150/100)}$ times or 22·5 per cent greater current density than 6 600-volt cables, whereas Mr. Woodhouse states that this density is independent of the pressure of working. I might perhaps mention here that the economic densities which obtained in pre-war times were 910 amperes per square inch for cables up to 11 000 volts, and 1 190 amperes per square inch for 20 000-volt cables. It will be noted that the difference between these was about 30 per cent.

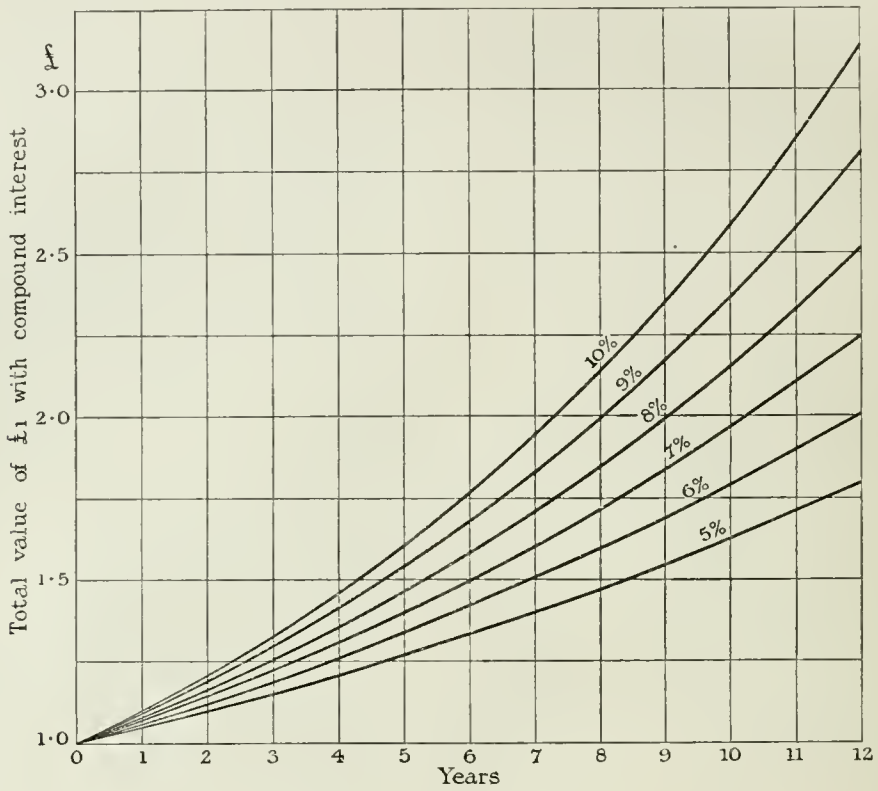


Fig. H.—Compound interest curves.

Where a considerable expenditure at some future date can be avoided by the immediate expenditure of a smaller sum the latter alternative should only be chosen if the sum of the immediate expenditure and the compound interest on it up to the date when the larger expenditure would have become necessary, is less than such larger expenditure. The curves show the total reached in any number of years, up to 12, by £1 with compound interest at various rates.

Mr. Kapp seems to make the assumption that the size of cable to be installed is settled entirely by the heating limit. Until one gets above 11 000 volts, my own experience has been that the economical loading of the cable is sufficiently far below the heating limit to allow ample margin for emergency conditions, but at higher voltages I agree that this is not always the case. It is possible that there is some confusion between : (*a*) The most economical main for a given load, and (*b*) the most economical loading of a given main. The distinction between these two divisions of the problem is that a main rated according to (*b*) would be run at the maximum economy for the particular

size of main, but for the current carried the absolute overall economy would not be so good as would be given by a larger main running at a correspondingly reduced current density as given by (*a*). I have dealt with this question in some detail on pages 365 to 367 of the latest edition of Whittaker's "Electrical Engineers' Pocket Book."

Mr. Kapp states on page 98: "it is doubtful if the advantages of cable protective gear are great enough to justify its cost." Any engineer who has, as he should have, the commercial as well as the scientific sense, will be anxious to avoid seemingly non-productive expenditure of this kind whether on protective gear or on switchgear. In practice, however, I think all operating engineers of large interconnected systems agree that such systems could not be operated to give satisfactory service without proper protective gear, and I think it was proved quite clearly in my own paper that, even allowing for the cost of the present forms of protective gear, there is a very appreciable overall saving in extensively interconnecting networks. These forms of protection are a peculiarly British development, and it is of interest to note that engineers in America and on the Continent are becoming increasingly alive to the advantages to be gained from their use. We are, however, all hoping that, in the

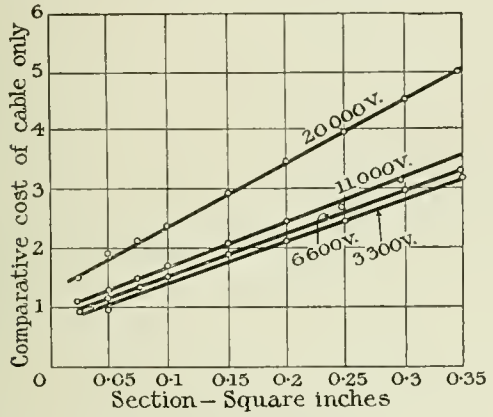


FIG. J.—Comparative variation of cost with section of three-phase cables of various voltages.

future, methods may be devised which will enable as satisfactory operating results to be obtained with considerably less capital expenditure. Possibly what Mr. Kapp has in mind is that there may be occasionally a tendency to over-do a good thing and to install too elaborate protective arrangements on simple networks where the advantages do not justify the cost, although probably in many cases the responsible engineers have had in mind that these networks will later form part of much larger ones. In Fig. K I have worked out some curves to check Mr. Kapp's Fig. 8. These would seem to indicate that in preparing his curve Mr. Kapp has taken the cost of the cable only, whereas the correct procedure is to take the cost of the cable on site, complete with all telephone or pilot auxiliaries and switchgear. If this is done it will be seen that on the heating basis the cost per kilovolt-ampere per mile is falling fairly rapidly up to 0·15 square inch and continues to fall rather more slowly up to 0·25 square inch. After 0·25 square inch very little is gained by increasing the section. Using my figures, but taking the cost of the cable only, I get results more in line with Mr. Kapp's. If the cable be loaded on the basis of maximum economy instead of heating, the advantages of the larger sections are increased. In view of Fig. K, I would suggest that further investigation is necessary before accepting Mr. Kapp's suggestion that a 0·075-sq. in. 6 600-volt cable is the largest size which it is sound to use, whether on the basis of carrying capacity or economic

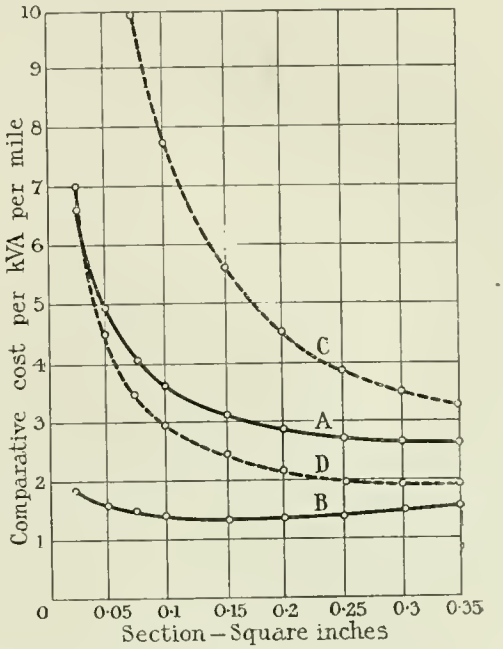


FIG. K.—Comparative cost per kilovolt-ampere per mile, for various sizes of three-phase, 6 600-volt cable.

Full-line curves:—Cable rated on heating current for a 50 degree C. rise.
- Curve A: Cable laid complete with pilot and telephone.
- Curve B: Cable only, inlaid.

Dotted-line curves:—Cable rated at economic current density of 1 000 amperes per square inch.
- Curve C: Cable laid complete with pilot and telephone.
- Curve D: Cable only, inlaid.

loading. Sections (4) and (5) of the paper are to my mind exceedingly useful, and deal with important matters which have often been neglected in the past. To-day many undertakings are operating with overloaded networks and are attempting to increase their capacity by distributing the load between the various feeders and interconnectors in a more symmetrical manner. One regrets that English manufacturers have not seen their way to devote more attention to the question of regulators for feeder purposes. At present the design of these is far from satisfactory, particularly for the higher voltages, and the cost is usually very high. On the North-East coast a number of step-switch regulators of Continental manufacture for 20 000-volt working have been in very satisfactory use for some 9 or 10 years, and the cost of these was only of the order of about one-half that of the equivalent induction regulator. It may be of interest to note

that in America a form of regulator has been introduced in which the induction and step principles are combined by the use of a small capacity induction regulator to bridge the gaps between steps, thus avoiding special arrangements for preventing sparking in changing from step to step. In many cases the cost of a variable regulator can be avoided by the installation of fixed boosting transformers which keep the distribution of load correct at the peak, at the expense of a certain amount of circulating current at other times. Since, however, the loss due to these circulating currents is off the peak, their cost is relatively small. Section (5) on the load capacity of cables is exceedingly valuable at the present time, and will be of great interest to the Committee of the British Electrical and Allied Industries Research Association, which is considering this question. I suggest that a useful addition to this section would be a set of curves similar to Fig. 2 of Mr. Woodhouse's paper giving for different time-constants the two extreme carrying capacities of cables at different load factors with the two limiting forms of load curve.

Mr. J. St. Vincent Pletts (*communicated*): I was very interested in the solution given by Mr. Kapp of the problem of finding the most economical site from which to supply different amounts of power to a number of given points. It does not seem to me by any means obvious that the point which makes the sum of the products of the distances into the cost per length of the necessary cable a minimum, is also the point at which forces proportional to the cost per length of the necessary cable and passing through the same places are in equilibrium. The realization that the two problems were identical was, I think, an achievement for which Mr. Carrothers deserves the highest praise. Not very long ago I had to deal with the latter problem in an entirely different connection. Like Mr. Kapp, I was unable to find an analytical method of treatment and, not having the paraphernalia of pulleys and weights, I devised another method which may perhaps be of interest to him. I set out on a drawing board the points through which the forces were to pass, and marked each with its respective force. Then I selected the point at which I thought the forces would be in equilibrium and drew the polygon of forces, making each side parallel to an imaginary string and of a length proportional to the force. The resultant gave me the direction and magnitude of the additional force I should have to apply to the knot to bring it to the point I had selected. The problem therefore resolved itself into finding how the knot would move if I were to cut the additional string. It is obvious that the removal of this additional force would cause the knot to move in approximately the opposite direction, through a distance depending to some extent upon the relative magnitude of that force and all the other forces. The direction and distance are only approximate because, as soon as the knot begins to move, the force moving it changes in both direction and magnitude, but the resultant nevertheless gives a valuable indication of the direction and distance of the true point of equilibrium from the point originally selected. From the new point so indicated I drew a new polygon of forces, giving a much smaller resultant, which in turn indicated the position of the true point with much greater precision, thus enabling the process to be repeated until the polygon closed upon itself, giving the true point of equilibrium. This may not sound a particularly rapid method, but as a matter of fact it is possible with a very little practice to find the point after drawing three or four polygons, and the polygons may be so rapidly and accurately drawn with the aid of a couple of set squares and a measure, that I think the point can be found in less time than it would take to set up the apparatus, even if one had the necessary strings, pulleys and weights. Moreover, the result is quite as accurate, and the accuracy can be checked by the simple process of drawing the sides of the polygon in a different order and seeing how nearly the same point is reached.

Dr. A. Russell (*communicated*): I was much interested in Mr. Kapp's theorem on the most economical site for a power station and in Mr. Carrothers's model for finding the position of this site. As the theorem is novel it will be useful to consider it mathematically. In the usual discussion of this problem it is customary to assume that the fractional loss of the energy distributed by each feeder due to heating is the same for each feeder. It follows that the site of the power station which makes the volume of the copper used in the feeder mains a minimum is the centre of gravity of the load. It follows also that the voltage drop from the power station to each distributing centre is the same. Consequently the current density in the long feeders will be much smaller than in the short feeders. In order, however, to find the absolute minimum value of the copper in the feeders for a given efficiency of distribution, it is necessary to remove the restriction that the efficiency of distribution of every feeder is the same, and this is what Mr. Kapp has done. The theorem given below shows that the copper required for the feeder mains for a given power loss is a minimum when the *current density* in each feeder is the same. It also shows that the minimum value of the copper in the feeders is proportional to $(l_1I_1 + l_2I_2 + \ldots)^2$, where l_1 is the length of the feeder which carries the current I_1, l_2 is the length of the feeder which carries the current I_2, etc. We have therefore to find the site of the power station for which $l_1I_1 + l_2I_2 + \ldots$ is a minimum. Finally, we have to choose the value of the power lost in the mains so that the annual interest and depreciation plus the annual cost of the power expended in the mains may be a minimum. The investigation gives the actual values of the cross-sections of the feeder mains which make the distribution the most economical. It will be seen that there are three mathematical theorems on minimum values which have to be solved. In the first theorem we assume any site for the power station and find the cross-sections of the mains that will, for a given power loss, make the copper required a minimum. From the result obtained we find the best site. Finally, we apply a theorem similar to Kelvin's, to find the most economical power loss. This investigation gives us the best site and the best sizes for the mains. Let the distance of the generating station G from the distributing centres

A_1, A_2, . . . be l_1, l_2, . . . and let S_1, S_2, . . . be the cross-sections of the feeder mains. Then if V be the volume of the copper required, we have

$$\tfrac{1}{2}V = l_1S_1 + l_2S_2 + \ldots \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Let p_1 be the voltage drop from G to A_1, p_2 the voltage drop from G to A_2, . . . and let P be the total power loss in the feeders. Then if I_1, I_2, etc., be the currents taken at the distributing centres

$$P = p_1I_1 + p_2I_2 + \ldots \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$p_1 = 2(\rho l_1/S_1)I_1, \qquad p_2 = 2(\rho l_2/S_2)I_2 \quad . \quad . \quad (3)$$

where ρ is the volume resistivity of the copper, and from (1)

$$\frac{V}{2} = 2\rho\left\{\frac{l_1^2}{p_1}I_1 + \frac{l_2^2}{p_2}I_2 + \ldots\right\} \quad . \quad . \quad . \quad (4)$$

Now for a maximum or a minimum value of $V/2$ obtained by varying p_1, p_2, . . . we get by Lagrange's method

$$0 = I_1\delta p_1 + I_2\delta p_2 + \ldots$$

and

$$0 = -2\rho\left\{I_1\frac{l_1^2}{p_1^2}\delta p_1 + I_2\frac{l_2^2}{p_2^2}\delta p_2 + \ldots\right\}$$

Hence, if λ be an undetermined multiplier, we have

$$\frac{l_1^2}{p_1^2} - \lambda = 0, \qquad \frac{l_2^2}{p_2^2} - \lambda = 0,$$

and, therefore,

$$\frac{p_1}{l_1} = \frac{p_2}{l_2} = \ldots = \frac{P}{l_1I_1 + l_2I_2 + \ldots} \quad . \quad . \quad (5)$$

It can be shown that this solution gives V a minimum value. In this case

$$\left.\begin{aligned} S_1 &= 2\frac{\rho I_1}{P}(l_1I_1 + l_2I_2 + \ldots) \\ S_2 &= 2\frac{\rho I_2}{P}(l_1I_1 + l_2I_2 + \ldots) \end{aligned}\right\} \quad . \quad . \quad . \quad (6)$$

and

$$V = \frac{4\rho(l_1I_1 + l_2I_2 + \ldots)^2}{P} \quad . \quad . \quad . \quad (7)$$

We see from (6) that when V is a minimum

$$\frac{I_1}{S_1} = \frac{I_2}{S_2} = \ldots = \frac{P}{2\rho(l_1I_1 + l_2I_2 + \ldots)} \quad . \quad (8)$$

For a given site, therefore, the volume of the copper required for the feeder mains is a minimum when their current densities are all equal and are determined by (8). If this value of the current density be excessive we must reduce the value of P, the power lost in the mains. In accordance with (7) we have now to find the position of G so that $l_1I_1 + l_2I_2 + \ldots$ is a minimum. In practice, as there are only a finite number of sites possible, this might be done by trial and error, and I think that this method would generally be best. We shall, however, try to solve it mathematically. Assuming that there are no hills, rivers, valleys, etc., and that all the mains are straight lines, let the co-ordinates of A_1, A_2, . . . and G be (a_1, b_1) (a_2, b_2) . . . and (x, y) respectively. Then

$$\Sigma(lI) = I_1\left\{(x-a_1)^2 + (y-b_1)^2\right\}^{\frac{1}{2}} + I_2\left\{(x-a_2)^2 + (y-b_2)^2\right\}^{\frac{1}{2}} + \ldots$$

Differentiating this expression with regard to x, and equating to zero, we get

$$0 = I_1\frac{(x-a_1)}{l_1} + I_2\frac{(x-a_2)}{l_2} + \ldots = I_1\cos\alpha_1 + I_2\cos\alpha_2 + \ldots$$

Similarly, by differentiating with regard to y, we get

$$0 = I_1\sin\alpha_1 + I_2\sin\alpha_2 + \ldots$$

These equations show that if forces I_1, I_2, . . . act at the required position of G in the directions of l_1, l_2, . . . they will be in equilibrium. This proves the theorem on which Mr. Carrothers's model is founded. A somewhat similar piece of apparatus is used in many physical laboratories for verifying the polygon of forces. Little aluminium pulleys mounted on ball bearings are used, and with five or six strings the apparatus is found to be very accurate. From the mathematical point of view the theorem is subject to limitations, but these do not affect its practical use. Suppose, for instance, that there are only three centres, A_1, A_2 and A_3, and that each takes the same load. Then, if each angle of the triangle is less than 120°, G will be the point inside the triangle at which the sides subtend angles of 120°. If the angle $A_1A_2A_3$ be 120° or greater, G will be at A_2. Again, if we have four centres A_1, A_2, A_3 and A_4 in a straight line, and if the sum of the loads at A_1 and A_2 equals the sum of the loads at A_3 and A_4, G may be anywhere on the line joining A_2 and A_3. For a given value (7) of V the possible sites of the power stations lie on the curve :—

$$l_1I_1 + l_2I_2 + \ldots = \text{constant}.$$

For instance, if there are only two equal centres A_1 and A_2, the sites will lie on an ellipse which has A_1 and A_2 for foci ; the minimum value of V occurs when the site lies at any point on A_1A_2. If the loads on the two stations are unequal, the locus of the sites for a given value of V is $l_1I_1 + l_2I_2 = \text{constant}$, which gives us Cartesian ovals. The best site in this case would be the greater of the two centres. Having found the position of G which makes the volume of the copper required for the feeders an absolute minimum, we can compute this value by (7), and the required cross-sections of the mains by (6).

Let us now suppose that the mains are loaded for H hours per annum, that the price of generating per kilowatt-hour is q pence, that the cost of a cubic cm. of copper is r pence, and that interest plus depreciation is to be taken as 10 per cent. If M denote the total annual cost, we have

$$M = \frac{qHP}{1\,000} + \frac{r}{10}V_{min.} = \frac{qHP}{1\,000} + \frac{r}{10}\cdot\frac{4\rho(l_1I_1 + l_2I_2 + \ldots)^2}{P}.$$

and, if P varies, this is a minimum when

$$P^2 = 400(l_1 I_1 + l_2 I_2 + \ldots)^2 \frac{r\rho}{qH}$$

Substituting this value of P in (6) we get

$$S_1 = \frac{I_1}{10}\sqrt{\left(\frac{qH\rho}{r}\right)}; \quad S_2 = \frac{I_2}{10}\sqrt{\left(\frac{qH\rho}{r}\right)}$$

and

$$\frac{I_1}{S_1} = \frac{I_2}{S_2} = \ldots = 10\sqrt{\left(\frac{r}{qH\rho}\right)}$$

If this value of the current density be too great for the thickest main, we must reduce the power loss until the current density becomes admissible.

Mr. G. V. Twiss (*communicated*): Mr. Woodhouse makes the statement that transmission is an expensive necessity. This was controverted by a previous speaker, but I should like to add that, in my opinion, transmission, rather than being an expensive necessity, is really a means whereby the advantages and savings of centralized generation can be achieved, and, if intelligently applied, transmission and distribution will result in savings far exceeding its cost. I notice a difference of opinion between the two papers before us. Mr. Woodhouse advocates the adoption of the lowest pressure of transmission, whereas Mr. Kapp advocates the highest. This would point to the desirability of the two authors being respectively invited to discuss each other's papers. I find myself in disagreement with the advocacy of low voltage. Mr. Hunter emphasized it still more by a direct comparison, that, because of reactance, 33 kilovolts in an overhead line are equal to only 22 kilovolts in an underground cable. There is evidently something wrong here. In what respect are they equal? Of course, reactance is greater in an overhead line than in an underground cable, due to the fact that, in an overhead line, conductors are wider apart, and air (with a specific inductive capacity of 1) is the dielectric, whereas with an underground cable the conductors are close together, and impregnated paper (with a specific inductive capacity of from $3\frac{1}{2}$ to 4) is the dielectric. In designing an overhead line, however, that is all allowed for in the calculations, so that for a given amount of power to be transmitted, and a given drop, a 33-kV line is considerably cheaper than a 22-kV underground cable and, notwithstanding the comparison, a 33-kV overhead line will do the same work as a 22-kV underground cable at much less cost. In the case of overhead lines, to increase the voltage reduces the reactance and increases the leading component of current, and consequently the copper can be run at a greater current density for a given drop. Extra insulation, being merely air except for the points of support, or say one-thousandth part of the length, adds but little to the cost; consequently the cost per kilowatt transmitted diminishes, and the economic voltage is on the high side. With underground cables, the opposite is the case. Increased voltage increases the dielectric loss and reduces the permissible current density. In addition, the cable being insulated throughout its length extra insulation costs a great deal more, and the economic voltage for a cable is therefore lower than for an overhead line. But that is no argument against overhead lines, as Mr. Hunter's remarks would seem to imply; they are, rather, an argument for them. Low voltages indicate small dielectric problems but large currents, and large currents are always an objectionable feature and the cause of most difficulties. High voltages, on the other hand, indicate small currents, but proper regard must be paid to dielectric problems. However, with the better knowledge of the dielectric field now available, there should be no real difficulty in providing insulation for the highest voltages, and thus achieving the handling of large amounts of power with small currents. Any arguments which tend to advocate low voltages either directly or indirectly are, in my opinion, wrong. The new age before electrical engineering is one of high voltages, and air-insulated high-voltage overhead lines will have their place in it. There is a view implied by previous speakers, and held by many, that there is a conflict of interests between overhead lines and underground cables. I do not admit this. Each system has its sphere, where its use must be to the advantage of the other system. Consequently, I was glad to hear of the possibility of 60- and 100-kV underground cables. On the other hand, knowledge of the dielectric field, together with chemical and thermal knowledge, based upon experience, is being thoroughly applied to overhead lines, their insulation and apparatus, resulting in greater reliability and security. I heartily welcome all developments in connection with underground cables; and underground cable interests, if they think as I do, will welcome all developments in connection with overhead lines.

Mr. R. O. Kapp (*in reply*): My figures of £4 and £6·5 respectively for 40 000-kVA transformers were obtained from different schemes, and I am glad that Mr. Sayers has pointed out the inconsistency between these figures, which I had not noticed. I agree that the cost should roughly be proportional to the radius for a given load. This could most clearly be seen from a reference to Fig. 1 (*a*) in the paper. Varying the radius for a given load on this figure would alter the base of the triangle without altering its height, so that the area would be proportional to the radius. The same would apply to Fig. 2 (*a*), where the shaded area would also be proportional to the base for a given height of the figure. I have no doubt that £6·5 is too low a figure for an average case. Mr. Sayers and subsequent speakers have referred to Kelvin's law, and have suggested that this law may give safe current densities for cables working at 6 600 volts or even higher. It must be remembered that a network must be so designed that the safe current density is not exceeded if one cable is out of service so as to allow for a stand-by. Therefore, the permissible current density for safe working with all cables in use is lower than the maximum with 6 600-volt cable, or the permissible current density for working is lower than the economical current density given by Kelvin's law. It must, however, be admitted that calculations of this sort are based on a number of figures which vary from case to case, and are moreover at the present time in a state of flux. These are such figures as the generating cost

per unit, the cost of cables, the value to be given to capital charges, the conditions of heat dissipation, and similar considerations. Where, for instance, it might have been sufficient some years ago to put the capital charges equal to 7 per cent of the capital cost, it would be sounder now to assume $12\frac{1}{2}$ per cent.

A further consideration which may lead to different results in the application of Kelvin's law is that pointed out by Mr. Beard. If the problem is to determine the cross-section of a single cable, only a portion of the total cost of the cable, namely that which varies with the cross-section of copper, enters into the formula. If, however, the cross-section is given and the problem is to determine the number of cables of the given cross-section, or the problem is to consider whether or not to lay extension feeders parallel to existing ones, then the whole cost of the cable, and most if not the whole of the cost of laying it, enters into the formula, giving a current density which is always greatly in excess of the safe figure.

As Mr. Dunsheath suggests, it would be better to base temperature charts on kilovolt-amperes than on kilowatts. The only reason why Fig. 13 is a kilowatt curve is that I had no characteristic kilovolt-ampere curve available when preparing the paper, most station records being in kilowatts. As Mr. Dunsheath says, the temperature coefficient undoubtedly introduces a factor which makes the temperature charts drawn by my method inaccurate. Unfortunately, the error is such as to make the true equivalent continuous load higher than that given by my method. I fear that to take account of temperature coefficient would complicate the method too much for practical purposes. I would suggest that a factor of safety be always assumed to take account of this source of inaccuracy.

If we could use Mr. Rosling's suggestion of using thermo-couples instead of the template method, we should get over the difficulty, and I am glad to see that Mr. Melsom agrees with Mr. Rosling that it would be of great value if instruments could be evolved which would enable an engineer to obtain a curve showing the actual heating of any particular cable. Measured results, if they could be obtained, are always better than such calculated ones as are given by the template method.

Mr. Beard has challenged me concerning my opinion of feeder protective gear. I think that such gear is worth installing in certain cases, such as on important feeders and ring mains, but that the tendency has lately been to overdo feeder protection. I think that in most cases money is better spent on insulation of the cables than on protective gear. Mr. Beard's curves (Fig. K) are most interesting, though I am surprised that he gets such a high cost per kilovolt-ampere. I admit that my own curve (Fig. 8) was based on good conditions of heat dissipation, and therefore high current densities, but it did include the cost of laying although not the cost of pilot wires or telephone cables. On account of the instability of prices referred to above of all quantities on which such curves have to be based, I should not like too much weight to be given to Fig. 8, but, as Mr. Beard says, the subject deserves further investigation. I would, however, point out that the curve might slope considerably between 0·075 and 0·25 square inch, and yet, on account of the smaller proportional cost of spare feeders with the smaller cross-section, it would often pay to standardize the use of 0·075 sq. in. feeders.

The instrument demonstrated by the President by which temperature charts could be traced is a beautiful piece of apparatus. An integraph would be a great improvement on the more cumbersome template method.

(**Mr. W. B. Woodhouse's** reply to this discussion will be published later.)

Discussion before The Liverpool Sub-Centre, at Liverpool, 13 December, 1920.

Mr. B. Welbourn : At the present moment when such large schemes are being developed and such large amounts are being expended on those schemes, whether overhead or underground, the economics of electricity supply are most important, seeing that one-half of the capital invested in electricity supply goes in the outside work. I will first take Mr. Kapp's paper. I notice that he takes as his basis 33 000 volts for transmission, and 6 600 volts for detail distribution. Mr. Woodhouse, on the other hand, takes 11 000 volts for his detail distribution, and I think both authors should be invited to discuss what the detail distribution should be. Mr. Kapp is not in favour of 11 000 volts, whereas Mr. Woodhouse has used that pressure for over 15 years, and is satisfied with it. I have come to the conclusion that the authors had specific cases in their minds rather than a general case, and that they are recommending what they found the right pressure for their particular case. There are two points on page 98 of Mr. Kapp's paper with which I am not in agreement. At the beginning of section (3) he lays it down as fundamental that there must be one spare cable, but surely that cannot be a general rule. If there is a cable supply into a consumer's premises, and one spare cable, it gives a duplicate supply, but if a supply to a substation required six or eight cables it surely could not be right to have any spare cable. With the modern 6 600-volt cable, the overload capacity of the remainder would be quite sufficient during the time one broken-down cable was out of commission. Mr. Kapp said : " It is very doubtful indeed if engineers will decide in future schemes that advantages of cable protective gear are great enough to justify its cost," but that seems to me to be quite a fundamental difference, and Mr. Woodhouse's paper advocates wholeheartedly the necessity for some protective gear (see page 87), especially in those cases where there is an interconnected network. On some of the large networks in this country it has been found an absolute *sine qua non* that they should have an instantaneous protective gear, so that in the event of a fault between phases, or of a fault to earth, the line should be automatically disconnected

before any synchronous motor could fall out of step. Mr. Kapp's paper will be most helpful in connection with the Research on the Heating of Buried Cables. On page 87 Mr. Woodhouse says, "A part independent of the cross-sections of the conductors and of the working pressure, being principally the cost of excavating trenches, laying and jointing the cable," and he also says that that is a quantity which is independent of the working pressure. That point requires a little further consideration. Joints may cost anything from £10 to £100 each, according to the pressure. On page 92 there is an expression for the current-carrying capacity of an 11 000-volt armoured cable laid direct in the ground. That is an entirely arbitrary expression and does not convey very much unless we are told the continuous copper temperature that is being considered. From figures which I have taken out I assume it was between 55° and 60° C. One of the first things that the Heating of Buried Cables Committee hopes to do before finishing its work is to prepare tables of safe current-carrying capacities for different pressures, and for cables laid under different conditions, whether armoured, laid direct in the earth, or cables in conduits or laid on the solid system. Mr. Woodhouse gives a word of warning about the danger of going beyond 33 000 volts with underground cables at present, because experience, even of that pressure, is limited. Possibly a word of caution on that question is needed, in view of the discussion which has taken place regarding that question of cable pressures. The position seems to me to be this, that there is something like 15 years' very satisfactory experience of 20 000-volt cables, and entirely satisfactory experience with 20 000-volt joints for about 8 years. The total operating experience of the world with 33 000 volts is, I believe, limited to some 50 miles of cable, but I am confident that 3-core cables for that pressure are a sound engineering proposition. It seems to me rather unwise and opposed to British conservative methods to talk of changing everything from 33 000 to 66 000 volts without having a great deal more experience, and that can only come from extended knowledge of the behaviour of compound dielectrics under continued stress over a period of years under operating conditions. The chief engineer of one of the best-known constructional engineering companies, in a discussion on 66 000-volt developments, advised me to go slowly, as makers' troubles with plant do not vary as the pressure, but as the square of the pressure.

Mr. J. A. Morton : Dealing with Mr. Kapp's paper first, the author says on the first page, "The capital charges of the E.H.T. distribution system of a super-power scheme will be comparable with the coal bill and may even exceed it." I should like to know how he arrived at that conclusion. I do not think such capital charges could ever come near the cost of the coal bill ; if the figure was one-third it would be nearer the mark. Further on he says, "The average charge per unit may be as low as about 0·05d. or as high as about 0·22d. to cover the capital charges on E.H.T. distribution." From figures which I have got out from time to time, I think the capital charges ought really to be about half those mentioned. On page 97 he mentions that he has not taken into account voltage drop in the feeders or the feeder losses ; and it is this which makes one feel that his figures must be accepted with caution. Mr. Woodhouse points out that those losses are a very important part of the transmission of energy, just as important as the capital cost which Mr. Kapp deals with in his paper, and Mr. Kapp will probably find, as I will try to show, that if those losses are taken into account he will come to some slightly different conclusions on several points. On the same page he describes the most economical position for a substation. There is another method which is sometimes used. Let L_1, L_2, L_3, etc., represent the individual loads, and x_1, x_2, x_3, etc., y_1, y_2, y_3, etc., the distances of these loads from any two fixed lines OX and OY, at right angles to each other. Then the position for the substation is at that point which is at a distance

$$\frac{x_1L_1 + x_2L_2 + x_3L_3 +, \text{etc.}}{L_1 + L_2 + L_3 +, \text{etc.}} \text{ from OX}$$

and

$$\frac{y_1L_1 + y_2L_2 + y_3L_3 +, \text{etc.}}{L_1 + L_2 + L_3 +, \text{etc.}} \text{ from OY}$$

Personally, I think that method is near enough, and I only mention it because Mr. Kapp said he had not met with any other method of doing it except by the ingenious string method. On page 98 the author mentions that he decides the economical voltage purely from the point of view of capital cost. If he had taken the *annual* total costs into consideration he would probably have found that it would pay to use 30 000 volts at a less distance. I think Mr. Kapp had an inkling of that, because he mentioned that he would be inclined to adopt that pressure for less distance. On page 100 there is a rather extraordinary statement. Mr. Kapp says he believes that 0·075 sq. in. or some figure near to it will in future become the standard cross-section on 6 600-volt networks, with possibly feeders of some smaller cross-section on the outer fringe of the supply area. I do not know how he arrived at that, and I do not agree with the curve in Fig. 8. I have taken out some actual figures comparing two 0·075 sq. in., 6 600-volt armoured 3-core cables carrying 3 300 kVA, with one 0·25 sq. in. cable carrying 3 500 kVA with the same temperature rise, and my figures come out very different from those of the author. I do not mean only that they come out actually different from the curves, but that the comparison is very different. For instance, there is a distinct saving in capital cost by the use of one 0·25 sq. in. cable instead of two 0·075 sq. in. cables ; in one case it is £1·045 per kVA per mile laid, and in the other £0·93. If the annual I^2R losses had also been taken into account and added to the capital cost, the comparison would be still more in favour of the one 0·25 sq. in. cable. My experience with cables suggests that wherever possible one can use one cable up to 0·3 sq. in. for economy, rather than two smaller ones such as suggested by Mr. Kapp. Mr. Woodhouse mentioned * that the load factor of the losses was less than that of the load, and that allowance ought to be made

* On p. 6 of the advance copies of the paper (since deleted).

in the cost of the waste energy on account of this. Another similar point arises here, and that is that the annual charges on the increased size of main to transmit the losses ought also to be taken into account. If these were taken into account one might get the cost of wasted energy in a cable 30 per cent more than the actual cost at the power station. In regard to the economic pressure on page 91 I do not think there are any simple formulæ of the Kelvin law type which can deal with that question in a simple straightforward way, especially when one is dealing with extra-high-tension transmission. For overhead lines Professor Still gives a rough formula for the voltage for lines over 20 miles long, and the economic voltage (kilovolts) is $5 \cdot 5\sqrt{(\text{miles} \times \text{kW} \div 100)}$. I have found that formula useful for overhead work, but not for underground cables. On page 91 Mr. Woodhouse states that the extra cost involved by an increase in size of the conductor forming part of a cable laid direct in the ground does vary in strict proportion to the weight of metal. I do not think that is so. For instance, taking a 0·04 low-tension 3-core and a 0·25 low-tension 3-core cable the cost of metal varies as 1 : 6·25, while the cost of insulation and lead, etc., varies as 1 : 2·65, and with the same sizes of 6 600-volt armoured cable the cost of insulation, etc., varies as 1 : 1·3. It is such things which make it difficult to use simple formulæ for cable work. Mr. Woodhouse, in fact, makes many reservations on page 93, and he mentions many modifying influences which spoil the simple beauty of the formulæ. They take no account of the dielectric losses which go on all the time whether a cable is carrying a load or not. Those losses might even be the governing factor, being very considerable in some cases, such as in, say, a 10- or 20-mile transmission of 12 000 kW as between 30 000 and 60 000 volts, because in that case the dielectric loss in a 60 000-volt cable at 40 per cent load factor would be more than the I^2R loss, and although the dielectric losses cost less than the I^2R losses, having 100 per cent load factor, yet when they are taken into account they put a different aspect on matters in the case of bulk transmission where heavy loads have to be taken over a considerable distance. Another point is that the power factor of such E.H.T. cables rises with the pressure, so that the dielectric losses do not vary as the square of the pressure but as something more than this. The power factor of a 30 000-volt cable would be less than one for 60 000 volts, though no one knows much about 60 000-volt cable yet. These are points which have to be taken into account, and they make an important difference in arriving at the most economical size and voltage. I have worked out several cases for bulk supply and I find, working on dielectric losses which a cable maker might be expected to get, and taking costs of switch-gear, etc., into account, that generally speaking, if the load is not too great for one cable to carry, a 30 000-volt cable is as economical as, if not more economical than, a 60 000-volt cable up to 20 miles' run, and the risk is less. One could lose much money in dielectric losses. The question will have to be gone into much more carefully before we start running 66 000-volt cables everywhere, as some people are expecting. It is a great pity that one has to bother with these losses, but under certain conditions they cannot be ignored. I wish personally that, instead of having to qualify all these equations like this, someone would invent a high-voltage direct-current system so that we could apply Kelvin's law in its pristine simplicity. Mr. Woodhouse suggests that by his equations one can ascertain economic size and pressure, but my experience is that this is impossible ; it is usually only by laborious processes tried upon different cables that one can arrive at the economic pressure. When one comes to really high pressures one has to consider other matters besides I^2R losses and capital charges ; one has the transformer losses to consider, and the extra risk, a very important thing which cannot be expressed in money. The question finally resolves itself into one of using formulæ very carefully, in conjunction with common sense.

Mr. G. H. Nisbett : Both these papers are particularly appropriate because they deal with a problem that is very much before the profession at the present time. A fashion has set in for the concentration of large amounts of power into one central station, with consequent increase of the area supplied from one source, and in my opinion it is being rather run to death without counting the cost, or even without proper investigation as to the cheapest way in which the district can be supplied. As a cable maker I have nothing to complain of if supply engineers spend 0·25d. per unit on capital charges on cables in order to save 0·1d. per unit on coal, but it is not good engineering. Moreover, in order partly to justify such schemes and to cut the capital charges, engineers are calling on the cable makers for higher and higher pressures and are willing to accept super-tension cables with a very much lower factor of safety than any yet employed. I believe this country to be far ahead of any other in the art of cable-making, and it has always led the way in safety and reliability of the distribution system, with consequent low maintenance and depreciation costs and their reflection in the price per unit. Now, however, a huge experiment is being tried, and dielectric stresses are being imposed vastly in excess of those of which we have any experience. For instance, taking for comparison a 0·1 sq. in. 3-core cable built to B.E.S.A. specification we find that

3 300-volt cables have a maximum stress of 834 volts per mm.

6 600-volt cables have a maximum stress of 1 348 volts per mm.

11 000-volt cables have a maximum stress of 1 753 volts per mm.

Now in the case of super-tension cables, not standardized but built to the dielectric thickness commonly employed, we find that

22 000-volt cables have a maximum stress of 2 950 volts per mm.

33 000-volt cables have a maximum stress of 3 880 volts per mm.

66 000-volt cables have a maximum stress of 6 150 volts per mm.

The art of cable-making has immensely improved during the past few years, and it may be that with good

luck these highly stressed cables will be all right, but only years of use can prove if that is so or not. Meantime, in any comparison in cost between cables working at one pressure or another, this difference in the safety provided must be kept in mind. Mr. Woodhouse gives a set of cable cost curves in Fig. 1, but as it stands it is totally misleading. In the first place, he has admittedly used some 1907 prices, whilst others are up-to-date; for even an approximate comparison the three lower curves should be lifted from 200 to 250 per cent in cost. Moreover, there is some obvious error in the calculation of them, as the higher the pressure the more the cost goes up in proportion with increase of section, so that the lines should diverge from the left of the diagram. And there is a further error, in that the curves cannot be straight lines, as the cost of laying is included in the price, and this is practically a constant quantity whatever the working pressure or size of the cables. All this is to be regretted, as the cost of the cable at a particular pressure is fundamental to all other considerations. As bearing on the preparation of a corrected table of relative costs, we have to remember the difference in relative stresses in the dielectric referred to, and to make an allowance for added cost of maintenance and depreciation in the case of super-tension cables. If engineers are willing to work these lower-tension cables at the dielectric stress now commonly employed in super-tension cables at, say, 33 000 volts, then we see that only mechanical reasons determine the thickness of the dielectric for all pressures up to 11 000 volts. Assuming 0·08 inch to be the minimum safe insulation thickness, then exactly the same cable can be supplied and used for any working pressure up to 11 000 volts, and this is so, even allowing for possible surges of a higher percentage above the normal in the case of the lower-tension cables than in those for super-tension. It is not suggested that these reduced thicknesses should be worked to in practice; money spent on providing a reasonable modicum of safety is money well spent, but it does seem that the B.E.S.A. standards are at present unnecessarily generous in the matter of insulation thicknesses. As to tariffs, I think that, if these proposed super-power stations serving wide areas are to come into being, then in fairness a universal tariff must no longer be used, but a rate per unit increasing with the distance of the consumer from the source of supply. Mr. Kapp has pointed out that the transmission capital costs alone may amount to as much as 0·22d. per unit, and the losses have to be added to that. Some differentiation would appear to be fair, on the principle that all consumers must pay directly or indirectly the freight on the goods they buy, and a tariff could be framed to the advantage of the supply authority in obtaining their load at a less average distance from the power station than would otherwise come about. Mr. Woodhouse devotes a considerable portion of his paper to consideration of the effect of load factor on losses, and rightly points out that the resistance losses cannot be determined from the load factor alone, but such refinement is unnecessary because, when the size of cable is being determined upon, both the load and the load factor are quite unknown and can only be guessed at, and when the cable is at work the losses can be measured. Similarly Mr. Kapp goes at considerable length into the question of the exact determination of cable temperature with a varying load, but such niceties are not of much use while we have variable factors entering into the problem, such as the thermal resistivity of the dielectric, the temperature and humidity of the soil and the number of cables in a trench. And in this connection it is well to point out that the limiting load on a long feeder cable may be determined by quite a short stretch of soil that has a high heat resistance or low absorption. It is also evident that, if cables are to be worked fully up to their maximum allowable temperature, it is necessary to join up lengths of cable of much larger section than the normal at points where the feeder enters a power station or substation, because there the conditions are not so favourable for the dissipation of heat, and the surrounding temperature is much higher. In conclusion I should like to point out how necessary these two papers are from the fact that Mr Woodhouse in conclusion (7) says, "The transmission pressure is primarily determined by a consideration of pressure-drop and total power," whereas Mr. Kapp says, "In the case of E.H.T. cables heating and not voltage-drop is always the determining factor."

Mr. W. Lang: On page 86 Mr. Woodhouse refers to transmission as an "expensive necessity," but I hardly think that in this instance the phrase is a very happy one.

Mr. W. Holttum: These two papers put before us very clearly the complexity of the general question of economical distribution, but one point which does not seem to me to be very fully considered, is the question of how an installation could be planned from the outset in order to be able to expand with a minimum amount of scrapping and re-organization, and to make the succeeding parts of the system fit in with those already existing. Mr. Woodhouse on page 86 suggests that this could not be done very well. I imagine it would be very useful indeed if rules could be laid down as to the way a system should start and expand with this object in view. Would it not be possible, for example, to settle on the final distribution system and pressure for any particular case and maintain them throughout, going into higher pressures for generation and transmission when necessary, as the system expands? Thus, the first power station might at a later stage be rearranged as a substation, and so on. Both the authors limit themselves to alternating-current transmission. I do not know whether there is any real reason why this should be. Mr. Morton has mentioned that it would be a very good thing if one had a high-tension direct-current system, and so get rid of such disadvantages as dielectric loss and power factor, which are inseparable from alternating-current systems. There is the Thury system, for example, of which not much is heard, and I should like to ask the authors whether they consider it to be quite out of the running for general transmission in this country, and if so, for what reason—whether it has inherent defects, or whether it is because we are already so heavily committed to alternating-current working. I should have been glad if both authors could have said something more about super-power stations,

and I should like to ask, in connection with the remark on page 94 of Mr. Kapp's paper, whether the statement he makes with regard to the position of the power house is an argument against super-power houses, and whether it is considered that undue weight would be given to considerations of water and coaling facilities, and so in the end a more expensive system be put into operation. Mr. Carrothers's method of using weights and strings, given on page 97 for finding the most economical position of a power station, is very good. It is a pity that a theoretical solution cannot be found, but I also think that the problem cannot be solved theoretically. The solution given by Mr. Morton earlier in the discussion would not give the result aimed at. Mr. Kapp's method for determining the temperature to which a cable rises, while it can, as the author states, be very easily applied in practice, seems to have some practical objections. There is first the question of cumulative error. Each element of the heating curve commences where the last one leaves off, and successive errors in the same direction will mount up. Then there is the question of the initial temperature of the cable, which would appear to require special determination for the starting point of each curve. There is also the question of variation of the quality of the ground, which Mr. Nisbett has mentioned, and finally the lack of knowledge of the heating time-constant. It is very remarkable that there is no information available as to the heating time-constants of cables. No figures for time-constants are given in the recent Report of the British Electrical and Allied Industries Research Association on the Heating of Buried Cables. As mentioned by Professor Marchant, some heating curves are given from which time-constants could easily be approximately determined. There is, however, one very noticeable point about those curves, namely, that the shape is obviously not that of an exponential curve, and the time-constants obtained would therefore be to a certain extent an approximation. Another point to be borne in mind is that the way in which a cable heats up depends not only on the load at the moment and on the time-constant, but also on the earlier part of the heating curve. For example, suppose the temperature of the cable rose 10 degrees C., the rate at which the immediate subsequent heating takes place will depend on whether the initial rise occurred under heavy load or light load, for in the former case the ground near by would have had time to warm only slightly, and in the latter case the ground would be considerably warmed and subsequent heating would consequently be more rapid. If the progressive heating of a cable takes place by alternately rising and falling load, any error due to this cause will probably average out, but as in the majority of cases the load continuously increases step by step up to the peak, there will be an accumulation of error. It seems as though some kind of integrating apparatus would be required to record the temperature to which the cable had arrived, but this would no doubt be very complicated. I suppose that the author's method would only be applicable in cases where the time-constant is fairly long. The curves in the Report referred to give time-constants the longest of which is between 1½ and 2 hours, and it would be interesting to know what the author considers to be the smallest time-constant to permit of the application of his method.

Mr. T. D. Clothier : Mr. Kapp states at the commencement of his paper that the necessity for super-stations is due to the need to conserve coal ; this may be so, and at least it puts the matter on firmer ground than is generally chosen. The next material point is to sell electricity at the consumers' premises at the lowest cost ; it yet remains to be shown that this can be achieved by aggregating the supply required in a large area and concentrating plant in one or more super-stations. I am of opinion that a strong case can yet be made for a number of moderate sized stations under one control. Mr. Kapp quotes a distribution cost of 0·22d. per unit over a distance of less than 10 miles ; this cost increases with increasing distance against the cost of transmitting an equivalent weight of coal by rail which, at 2·24 lb. per unit, is only 0·029d. per 10 miles, or 0·058d. per 10 miles at 4·48 lb. per unit, and this decreases with increasing mileage. I doubt the strict accuracy of Fig. 3. The cost of the intermediate-pressure mains required to supply the hypothetical uniform supply, represented by a rectangle, would surely require some cost at points A and B. Should not this figure be raised upon a rectangular base ? This figure illustrates a matter which I could wish had received more prominence, i.e. the economical limits to the size of substations at various pressures. Assuming the reduction from 30 000 volts to the low-tension system could be made without cost, by direct reduction from the 30 000-volt cables, the lower rectangle would become a triangle, the intermediate system would disappear, and the area representing cost would be reduced to exactly one-third of the expenditure shown in Fig. 3. Then again, by using three substations instead of one, the area can be reduced by more than 30 per cent, so that the economical size of a substation is a matter of first-rate importance. Mr. Kapp suggests the use of smaller cross-sections of copper on a ring main on the feeders furthest from the power house. This practice would hardly be generally commendable, for, if the ring main is to be used as a duplicate means of supply to all consumers on the ring, it must be contemplated that the section between the power house and the first consumer either way may be disconnected, in which event all consumers between the furthest point and the first consumer will be supplied through the section which it is suggested should be reduced. It is notable that the temperature charts carefully avoid giving any actual temperatures. There is an unfortunate degree of obscurity on this subject, on which definite information is overdue. I imagine that a device which could be made for attachment at selected points to underground cables, for the purpose of enabling the temperature of the lead sheath or armour to be kept under observation, might be of considerable practical use. However crude the result, and however approximate the temperature arrived at in this way would be to the core and insulation temperature, it would be better than no information at all, and, after all, it is more important to know the highest local temperature than the general average temperature. Mr. Woodhouse gives a diagram of cost of E.H.T. cables. I think the

prices for E.H.T. work do not include any provision for telephone and pilot cables, and must be adjusted for open country work; they would require modification for urban work, especially if laid in roadways with reinstatement charges. The addition of a curve representing the cost of the copper, which for the 0·05 sq. in. cable is about £170 and for the 0·3 sq. in. cable about £1 000, indicates a large margin which merits careful examination. Mr. Woodhouse's conclusion that the economical limit of cable capacity at the present high generating cost is well below the safe limit, is satisfactory, in that cables laid to-day may safely carry excess development until supplementary provision may be convenient. I doubt, however, whether anyone has failed on account of providing underground copper too generously for subsequent developments, or that there is any special danger of this happening. I should like to ask both authors what are the objections to the use of 6-core cables for 30 000-volt underground work. It has been stated that these cables are so used in America, and for uninterrupted trunk feeders this type of cable and the protective devices which can be used with it seem eminently suitable. The smaller conductor section appears to give a larger carrying capacity per equivalent cable to offset the 60 per cent or so increase in the bulk of the dielectric. In conclusion, I should like to mention the question of tariffs; neither author considers the factor of the alternative and local cost of producing power. In practice this factor has to receive very particular attention and, merely because schemes may be large, it will not be displaced, as it will largely determine the success or failure of such schemes.

Professor E. W. Marchant: With reference to the statement on page 91 of Mr. Woodhouse's paper, Kelvin's law has always been held to be the fundamental law governing the economic conditions under which to run a cable. Kelvin's law was originally laid down by Lord Kelvin to decide which was the most economical size of cable to use for transmitting a given amount of energy from one place to another, and his law was based on the fact that it was required to make the total cost of transmitting energy from one place to another a minimum; actually, that is not the right basis to take in calculating the most economical working current for a cable already laid. If a cable be laid in the ground and it is required to determine the most economical current to send through it, it is not possible to take as a basis that the total cost of transmitting energy should be a minimum. The right basis to take is that the ratio of the amount of revenue earned by the energy transmitted to the total cost of sending it along the cable should be a maximum. If this basis be taken, the result is not in agreement with Kelvin's law. The most economical section is that for which the cost of energy wasted is equal to the charge for interest and depreciation on the total cost of laying the cable, not to the part of the cost which is proportional to the area of the conductor, and that, I maintain, is the real and proper economic basis on which the most economical current density should be decided. I think that point is not fully appreciated. It was shown many years ago by Mr. S. Russell in a textbook, but I have seen little further reference to it. It is rather an important point, because Kelvin's law, very largely because of Kelvin's name, has been looked upon as something beyond dispute. The formula for current-carrying capacity which Mr. Woodhouse gives on page 92 for cables of varying cross-section, corresponds very closely to the figures that have been obtained in the experiments made on the heating of buried cables. With low-tension cables it has been found that the current through a cable which produces a given temperature rise is very nearly proportional to the square root of its area. This seems to lead to the conclusion that it would be more economical to transmit power with a number of small cables than with a small number of large cables. I am interested in Mr. Morton's calculations, but I should like to ask whether the currents given there were actual currents that had been found by measurement to give the same amount of heating on cables. [**Mr. Morton:** No, but the figures are near enough, from experiments that have been made.] It is important that there is a section of cable which can be calculated as the most economical for a given transmission. That is from the point of view of reducing the cost of transmission to a minimum, apart from the advantage of obtaining greater security by having an alternative source of supply. I hope we may be able to make some experiments in the university laboratory to find out a little more about dielectric losses. It is a very live subject. With reference to Mr. Kapp's paper I fully agree with the stress laid on the importance of an economical network lay-out. Mr. Carrothers is an old Liverpool student, and I am glad to find that he has proved himself so useful in connection with Mr. Kapp's work. I note in Fig. 6 of Mr. Kapp's paper on page 98 that, in the straight lines drawn, the crossing points of the curves for 6 600 and 30 000 volts, and the crossing point of 11 000 and 30 000 volts, correspond fairly closely to the old rule that was once used, i.e. that the most economical pressure for transmitting electrical energy a given distance is about 1 000 volts per mile. I suppose it is a coincidence, but it is an interesting one. I am not clear what Mr. Kapp means when he says on page 100, "Up to about 0·025 sq. in. the cost per kilovolt-ampere per mile of cable is cheaper the greater the cross-sectional area of copper of the cable." Is that for the same heating? Mr. Kapp remarks at the end of his paper that very little is known about the time-constants of cables. I hope the figures and curves given in the Report on the Heating of Buried Cables will supply that want. There are numerous curves given there which were put in with the intention of enabling the effect of intermittent loads to be estimated from the curves, and they will provide Mr. Kapp with some of the information he requires.

Mr. R. O. Kapp (*in reply*): I am interested to note that Mr. Welbourn thinks cables could safely take an overload of from 15 to 20 per cent during the time a defective cable was being repaired. I do not know whether many mains engineers would be prepared to do without spares on this account, as Mr. Welbourn suggests, but I hope that information on this subject will be given us in due course by the Research Committee dealing with the heating of buried cables. Mr. Welbourn appears to attach more importance to protective gear

than I should, but I note that he bases his arguments on the need for protecting the machinery rather than the cable, and in this I fully agree with him.

Mr. Morton thinks that my figures for the capital charges on feeders are too high. Such figures are necessarily based on assumptions which cannot be taken as too definite at the present time of fluctuating prices, and are further based on assumptions as to the safe loading of cables, on which we have very little information as yet. Therefore the quantitative statements in my paper were all included with a view to their being challenged, as that is the best way of reaching some common conclusions. I think, however, that at the present time capital charges have to be put rather high, and I have assumed $12\frac{1}{2}$ per cent. On a pre-war basis the capital charges would certainly be less than the coal bill, even for very large undertakings. Mr. Morton's method of determining the economical position of a power house would give the centre of gravity of the load, and therefore not the result aimed at. The curves in Fig. 6 include losses in the feeders and, therefore, do take the annual total costs into consideration. The curve shown in Fig. 8, on the other hand, does not include the losses, and I agree with Mr. Morton that, if this curve did include losses, it would slope more, and that it is therefore not conclusively proved that 0·075 square inch is a more economical cross-section than 0·15 square inch, but, as spares are cheaper with the smaller cross-section, it may still be so and the subject deserves further investigation.

Mr. Nisbett has suggested that such niceties as are gone into in my temperature charts are premature, as there are so many unknown factors which necessitate ample cross-section. It is quite true that these unknown conditions necessitate an ample factor of safety, which has sometimes been defined as a factor of ignorance. I think, however, that in so far as we can find out conditions to which cables are subjected, we ought to do so if we would avoid the charge that our factors of safety are really factors of laziness. I am interested in Mr. Nisbett's view that the tariffs on each undertaking will have to take account of the distance of the consumer from the power station, and I fully agree with his opinion that the cross-section of any part of a feeder with bad heat dissipation should be increased to give a uniform load capacity throughout the feeder.

Mr. Holttum has asked whether I think too much value is attached to the advantages of super-power stations. I consider that the tendency has been in that direction, and I think that probably the cheapest electricity will be procurable from power stations having a total installed capacity of about 100 000 kW. Mr. Holttum has pointed out some of the drawbacks of my method of basing cable rating on temperature charts, and I think these ought to be considered very seriously, as they undoubtedly introduce sources of inaccuracy. Some factor of safety ought to be introduced to take account of these inaccuracies after the theoretical figure has been determined from the temperature chart. With regard to the question as to the smallest time-constant for which this method would be applicable, I would refer Mr. Holttum to Fig. 18 in the paper, from which he will see that one is not likely to gain much if the time-constant is much less than one half-hour.

In reply to Mr. Clothier, the reason why the cross-section of copper on a ring main should be reduced at parts furthest from the generating station is that at these parts the main need never carry more power than the demand between it and the generating station on that side where the load is greatest, whereas the cables leaving the generating station must be able to carry the whole of the requirements of the ring main.

In reply to Professor Marchant, the curve in Fig. 8 assumes equal heating of different cross-sections of cable. With regard to his remark that a number of heating curves are given in the Report on Buried Cables from which the time-constant can be deduced, I should like to take this opportunity of drawing attention to a letter from Mr. Carrothers in the *Electrician* of the 17th December, 1920, which gives a simple method of analysing a given heating curve and determining its time-constant. As the practical curves attained and given in the Report are not true exponential curves, but only approximations, it becomes necessary to assume a true exponential curve approximating to these, but worse from the heating point of view than the actual curves obtained, in order to be on the safe side. That is to say, the assumed curve should envelop the actual curve in such a way that the assumed time-constant is certainly less than the actual one.

(**Mr. W. B. Woodhouse's** reply to this discussion will be published later.)

THE TWELFTH KELVIN LECTURE.

ELECTRONS.

By Sir William Bragg, K.B.E., F.R.S.

(*Lecture delivered before* The Institution, 13 *January*, 1921.)

It is at the wish of your Council that I am speaking to you this evening on the subject of Electrons.

Without this explanation you may well be amazed at my attempt to deal with so large a subject, on which Sir Oliver Lodge and others have spoken to you on many different occasions. But of course I set myself certain limits, and propose to speak mainly on two lines of recent research, referring to the part played by the electron in radiation processes and to the part it plays in atomic structure. And first, with your permission, I will put in a background of general facts.

In recent years the results of experimental research on the properties of electrons have accumulated with startling rapidity. As knowledge grows, the importance of the part played by the electron in the mechanics of the world becomes ever clearer. There are all the right signs that progress is being made along a road that really leads somewhere; we are continually finding that, through some electron action, phenomena are linked together between which we had hitherto seen no connection. Precision is given to our views: we find ourselves able to express, quantitatively and with confidence, laws and relations which have been matters of vague surmise. Every experiment that is finished suggests others that are promising. The whole world of experimental physics is full of new life, and of the consciousness that after a period of hesitation the tide of discovery is sweeping on again. I hope I may be allowed to claim that when an advance is made in the realm of physics all other natural sciences feel the impulse sooner or later.

While knowledge grows by experiment, theory is also busy. The attempts to co-ordinate the new discoveries are of singular interest because of their daring, their width and their strength: because they are so often fruitful in prediction: and, not least, perhaps, because they seem so often to be irreconcilable with each other.

Altogether, then, a lengthy course of lectures might well be devoted to the subject of Electrons, as indeed is the case in our universities. But your Council has asked me to confine myself—and I assure you I am very thankful for the restriction—to a brief description of the main features of a great inquiry, and of the direction in which that inquiry is moving.

It helps to a right appreciation of the position as regards the electron if we observe its strong resemblance to the older state of things when first the atomic theory of matter was clearly defined. Just as chemistry has grown and prospered on its recognition of the unit of matter, so electrical science has already begun a new life, and to all seeming, a most vigorous one, based on the understanding of nature's unit of electricity. There are many different atoms of matter, nearly a hundred are distinguishable by their different chemical reactions: but the number of different kinds of electrical atoms is very much more limited. We have for some years been clear as to the existence of the electron, nature's unit of negative electricity. More recently the work of Rutherford and of Aston indicates that the nucleus of the hydrogen atom is to be regarded as the positive counterpart.

If the chemist has found so much profit in his recognition of the fact that nature has just so many ways, and no more, of doing up parcels of matter, the electrician will surely gain in the same way, when he grasps the fact that electricity is not merely measurable in quantity, but that there is already a unit of nature's choice, possibly no more than one unit. We may say with justice that the most wonderful advances in modern physics are the reward for our appreciation of this truth, and we may hope with equal justice that we are yet far from reaping the full benefit.

The first suggestion of the atomic character of electric charge came, it is well known, from observation of the laws of electrolysis. Since the movement of atoms or atom clusters or ions across the electrolytic cell was accompanied by a simultaneous transfer of electricity, in which each ion, of whatever nature, bore always the same charge or at least a simple multiple of it, there was a clear indication that this division of electricity into parcels of constant magnitude implied the existence of some natural unit charge. No progress, however, was made or could be made so long as the charge could only be observed as an attachment to an ion: it was not even clear that it could ever have a separate existence. In the long series of researches which finally led to the isolation of the electron and the determination of its properties, there were certain that marked definite stages in the forward movement. Crookes examined the electric discharge in bulbs exhausted to a high degree by the new air pumps which he had succeeded in making; and he observed the so-called cathode rays, streaming away from the negative electrode. He showed that they possessed the properties to be expected from a stream of particles projected across the bulb, and carrying negative electricity with them; for, on the one hand, they could heat up bodies on which they fell, and on the other, they were deflected in crossing a magnetic field. Crookes spoke of a fourth state of matter and defended his view against the opposing hypothesis, held largely on the Continent, that the stream consisted of electromagnetic waves in some form

or other. Hertz showed that the rays could pass through thin sheets of matter such as aluminium leaf, and Lenard took advantage of this to coax them outside the bulb and display their effects in the air outside.

In the later years of the last century came the great experiments of Wiechert, Thomson, and many other well-known observers who weighed the electron and measured its charge, and showed that there was only the one electron though it was to be found everywhere and in every body. Since then, the measurements of these quantities have been repeated many times with increasing skill and understanding. They have reached their present high-water mark perhaps in the experiments of Millikan at Chicago, who gives for the value of the charge in electromagnetic units $e = 1{\cdot}591 \times 10^{-20}$, the mass being $0{\cdot}900 \times 10^{-27}$ gramme or 1/1 850 of the mass of the hydrogen atom.

So we arrive finally at an accurate comparison of these unique and fundamental units of nature with the units which we ourselves have chosen for our convenience, and without, of course, any consideration of the former. We infer from experiments such as those of Kaufmann and Bucherer, that the energy of the moving electron may be considered to exist wholly in the form of electromagnetic energy such as is necessarily present when an electrical charge is in motion; and that its mass is in this way perfectly accounted for. But this conclusion sets a limit to the size of the electron and we must assume that its radius, if its form is spherical, is very small compared with the radius of any atom. Also as the velocity of the electron approaches that of light, its mass increases; imperceptibly at first, but at the end very rapidly.

Why, we may well ask, have these measurements of charge and mass never been made before? The electron is everywhere: the transfer of electricity from place to place consists always in the transfer of electrons. The electric current is a hurrying stream of electrons: all our electrical machinery concerns itself with setting them in motion, giving them energy, and again withdrawing it. In the processes of electrolysis, the electrons are handed to and fro. Everywhere they fill the stage; why have we not noticed hitherto their qualities, which so far can be expressed so simply?

The answer is that we have never, until recently, been able to make them move fast enough in spaces sufficiently empty of air or other gases. It is only when an electron has a sufficient speed that it can escape absorption in the atoms which it must be continually meeting. Unless an electron has a speed exceeding about a three-hundredth of the velocity of light, that is to say, such a speed as it acquires in falling through a potential of a few volts, it sticks to the next atom it runs up against: even with 10 times that speed it can only move a fraction of a millimetre through air at ordinary pressure before it loses its velocity and, therefore, its power of going through the atoms. When Crookes first saw the cathode-ray stream in full course, it was because he had reduced the number of gas molecules in his bulb to such an extent that an electron could fly in a straight line from end to end of the bulb without going through more than a hundred atoms or so, and the induction coil had given it quite enough speed to do that without turning out of its course; no matter what sort of atoms they were. Incidentally, since atoms can be traversed in this way, we naturally think of an atom as a very empty affair.

Electrons flying still faster than in the discharge tube are found to constitute a part of the radiation from radioactive substances. Some of the β-rays have velocities nearly equal to that of light, and can pass through millions of atoms before their energy is spent. In open air a β-ray may have a course of metres in length, though it is generally broken, by encounters with traversed atoms, into a path full of corners and irregularities.

It is speed which gives separate existence to the moving electron: and speed which also betrays its presence to us. For, on its way, the electron here and there chips away another electron from an atom which it is crossing and leaves behind it a separation of electricities which may subsequently influence chemical action as in the case of the phosphorescent screen or photographic plate, or provide a current for the ionization chamber. We do not know exactly how this removal of electrons is effected; nor why some atoms part with electrons more easily than others so that the flying electron loses less energy as it goes through: there is much that is obscure in the whole process. But it gives us a ready means of observation, without which indeed our knowledge of the electron would be far less than it is.

These electrons which are so made manifest by speed form but a minute fraction of the whole number in existence. They are to be found in every body, and in every atom of every body. They form one of the elements of construction of the atom; and it is one of the most immediate aims of present research to find in what way they are built into atomic structure. In every atom there are certain electrons of which one can be removed at the cost of an amount of energy of the order of 10^{-11} ergs. The potential through which an electron must fall so that it acquires this energy is of the order of a few volts. There are other electrons within the atom which are intrinsically far more difficult to remove. On the other hand some atoms, for example those of a metal in the solid or liquid condition, have each one or more electrons which are little more than hangers on, and are indeed removed with very little trouble. A block of pure metal is full of such loosely bound electrons, so that if an electric potential difference is maintained across the block an electron flow or electric current is produced. The metal "conducts."

At sufficiently high temperatures all bodies become conductors; we must imagine that the violent thermal agitation shakes electrons free from their ties to the atoms even when at low temperature the bonds ordinarily remain unbroken. At high temperature too, the electrons acquire high velocities as they move to and fro with their proper share of heat energy. At the surface of the hot body the electrons may break away; and hence the "thermionic emission" investigated by O. W. Richardson. So copious is this supply of electrons at the surface of a hot body that if the latter is made negative in potential relative to its surroundings there is a current discharge which may sometimes be measurable in

amperes. Of course, such a current can only pass one way, negatively from the hot body, or positively towards it. So we get the basic principle of the "valve": and so Coolidge provides the electrons for projection against the target in the X-ray bulb which he has designed. At this point we find already the adaptation of our new knowledge of electrons to apparatus of extraordinarily great use to mankind.

If now we plunge a little deeper into our subject we come to certain most fascinating regions of it, where exploration is still in full progress. In one of these we find the most remarkable connection between moving electrons and electromagnetic waves. The one, it seems, can always call up the other: and the action obeys certain precise numerical laws.

Let us take as an example the production of X-rays in a Coolidge bulb. A plentiful supply of electrons is provided at the cathode by heating a fine spiral of tungsten wire to a high temperature. A high potential difference between cathode and target is provided by some appropriate means, and the electrons are hurled at the target, each possessing an amount of energy equal to the product of the electron charge and the applied potential. Where the electrons strike, some of their energy is converted into electromagnetic waves of very high frequency, the so-called X-rays. Suppose that we measure the energy supplied to each electron—not an easy matter with the usual arrangements, but very easily done if, as in certain experiments of Duane and Hunt, at Harvard University, the potential is derived from a great storage battery of 40 000 volts. Suppose, further, that we analyse by the X-ray spectrometer the X-ray radiation that issues from the target. We find that the frequencies of the emitted rays may have a wide range of values, but that the upper limit of the frequencies is always proportional to the energy of the electron and, therefore, to the potential imposed on the tube. This ratio remains the same no matter what is the intensity of the electron discharge, and no matter what the nature of the target. This ratio of electron energy to maximum frequency is a number which has turned up in previous cases where the emission of radiation energy has been measured: it is known as Planck's constant and is denoted by "h." Its value is $6{\cdot}55 \times 10^{-27}$. Although the constant has been met with before, there is probably no instance where the transformation of energy which it governs is so simply displayed or so easily measured as in the case just described.

In certain measurements made by Duane and illustrated in Fig. 1, the X-ray spectrometer was set to observe the presence of a certain frequency as soon as it appeared. The potential on the tube was then increased by degrees. The rays of the given frequency appeared as soon as the energy supplied to the electron was equal to the frequency multiplied by h. As the potential was increased still further these rays increased in intensity, as the figure shows.

It is to be observed that the production of X-rays is no aggregate of individual efforts by separate electrons: each electron produces its own train of X-rays when it strikes the target. There is no sign of any combined action, as indeed is evident from the fact that the intensity of the cathode-ray stream is without influence on the frequencies of the X-rays produced.

The crucial point is that when the energy of an electron is handed over in whole or in part, the frequency of the X-ray waves that take over the energy is determined by the quantity of energy handed over. This explains why there is a limit to the frequency of the X-rays: it is because there are some electrons, though only a fraction of the whole number, which give up all their energy to the formation of X-rays at the moment of striking, before they have lost energy in collisions. The rest of the rays, all those which have lesser frequencies, will come from electrons that have lost speed in this way, or possibly transfer only part of their energy. The atom of the target is playing the part of a transformer, and does not determine the frequency, so far as these effects are concerned.

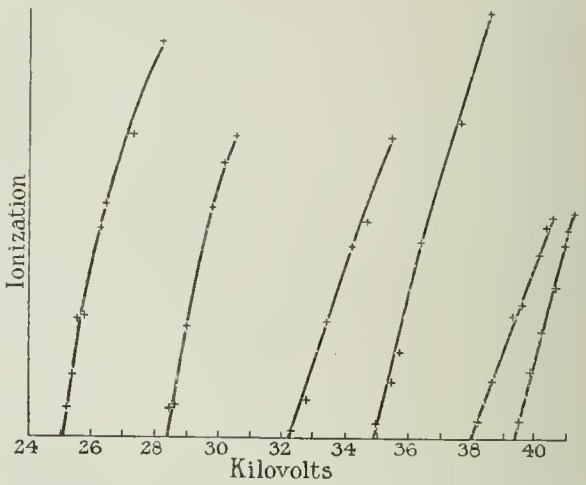


FIG. 1.—Each curve shows the rise of intensity of X-rays of one particular wave-length as the volts on the bulb are increased. The wave-lengths are (left to right) 0·488, 0·424, 0·377, 0·345, 0·318, 0·308, all in Ångström units (10^{-8} cm.).

[From DUANE and HUNT, *Physical Review* 1915, p. 166.]

All this is wonderful enough; but the marvel is greatly increased by the discovery that the effect is reciprocal. Just as the swiftly moving electrons excite X-rays, so X-rays when they strike any substance lose their energy, which now appears as the energy of moving electrons. And, again, we find the same variation in the result and the same limit to that variation. Among the electrons so set in motion we find, examining them as soon as possible after their motion has begun, every variety of energy-content up to a certain critical value which is equal to the frequency of the X-rays multiplied by the same constant h. It is to be observed that we cannot measure all the electron velocities as soon as they exist because some of the motions begin in the body of the substance, into which the X-rays have penetrated, and speed is lost on the way out. Again, therefore, there is nothing against the hypothesis that the energy of every electron set going by waves of given frequency is originally the same, and is determined by the standard condition already given.

Not only in the case of X-rays are these effects observed, but also in the case of light. The only difference is that the frequencies of light vibrations are some 10 000 times less than those of X-rays, and the electron energies are correspondingly smaller. When the light waves produce the electrons we have what is known as the photo-electric effect. The production of light by electrons has been much studied recently in experiments to find "resonance-potentials," that is to say, the magnitudes of potentials which must act on electrons so as to give them enough energy to excite certain particular radiation from atoms on which they fall.

Exactly how this strange transfer of energy from one form to another takes place, we do not know: the question is full of puzzles. The magnitudes involved are hard to realize; it helps if we alter their scale of presentment. Suppose that the target of the X-ray bulb were magnified in size until it was as great as the moon's disc, that is to say, about a hundred million times The atoms would then be spheres a centimetre or so in diameter. But the electrons would still be invisible to the naked eye. The distance from earth to moon would correspond roughly to the distance that ordinarily separates the bulb from an observer or his apparatus. We now shoot the enlarged electrons at the moon with a certain velocity: let us say that in every second each square yard or square foot or square inch, it does not matter which, receives an electron. A radiation now starts away from the moon which immediately manifests itself (there is no other manifestation whatever) by causing electrons to spring out of bodies on which it falls. They leap out from the earth, here one and there one; from each square mile of sea or land, one a second or thereabouts. They may have various speeds; but none exceed, though some will just reach, the velocity of the original electrons that were fired at the moon. That, reduced again to normal size, is the process that goes on in and about the X-ray bulb: which is part of a universal natural process going on wherever radiation, electron or wave, falls on matter, and which is clearly one of the most important and most fundamental operations in the material world.

Keeping these results in mind we can now appreciate a very remarkable development of electron theory which has been made in the last few years. Spectrum analysis has long been occupied with the extraordinary complications of the light radiation emitted by the various atoms. As a result it appears that the frequencies of the lines in a spectrum often display curious and exact numerical relations, in the form generally involving differences of frequencies of similar lines or groups of lines. For instance, the famous Balmer equation :—

$$\text{Frequency} = \nu = N\left(\frac{1}{n_1^2} - \frac{1}{n_2^2}\right) \text{ where } N = 3{\cdot}290 \times 10^{15}$$

gives the frequencies of series of lines in the hydrogen spectrum. When n_1 is put equal to 2, and n_2 to 3, 4, 5 in succession, the series of values for ν represent the frequencies of the lines in the visible spectrum. If $n_1 = 3$ and $n_2 = 4, 5, 6, \ldots$, in succession, we have the frequencies of lines in the infra-red (Paschen) and if $n_1 = 1$, $n_2 = 2, 3, 4, \ldots$, we have the frequencies recently shown by Lyman to exist in the ultra-violet.

Now there is nothing in our older conception of the origin of radiation within the atom to give us a clue as to why differences of frequencies should come into these empirical though most useful formulæ. We have pictured to ourselves vibrating systems, mechanical or electric, and waves arising therefrom. But what connection between masses or electricities gives us in any simple way equations involving the addition or subtraction of frequencies? We are in a blind alley. Let us, therefore, abandon our preconceptions as to the origin of those lines which we find in the light spectrum and suppose that here also they arise in the same fashion as we actually know that they arise in the cases we have considered above. Suppose that the energy of an emission of radiation is derived from the energy of an electron. It may be the only way in which radiation ever does arise, but it is not necessary to suppose so much at present. It is enough that we carry into the atom the whole process which in X-rays and the photo-electric effect we have observed to take place in part outside. Suppose that within the atom there are certain positions or conditions in which electrons *may* be, each postulating a certain energy associated with the electron: and suppose that sometimes an electron slips from one position to another of lower energy, and that the difference in energies is transformed into wave radiation according to the same law as before, i.e. energy transferred $= h \times$ frequency. Let the energy in these states be $Nh/1^2$; $Nh/2^2$; $Nh/3^2$; etc., and so on. Then all the series yielded by the Balmer formula are accounted for at the same time.

What may these states be? Why not, as Bohr suggests, so many different orbits in which electrons can move round the central positive nucleus in the atom, the nucleus whose sure existence Rutherford has established? At one time if we had presumed the existence of these orbits we should have been inclined to connect them with the direct emission of radiation, and the frequency of that radiation would be the number of revolutions in a second. But now we assume these orbits to persist without radiation, and that radiation arises when the electron steps from one orbit to another: moreover, the frequency of the issuing radiation is determined by the simple rule, frequency is equal to change of electron energy divided by h. We are not picturing any new process here, or evolving new ideas to fit awkward facts; we are supposing a process to exist in one place which we already know to exist in another.

It is a very remarkable fact that the number N is equal to $2\pi^2me^4/h^3$ within small errors of experiment. Spectrum measurements show that N is equal to $3{\cdot}29033 \times 10^{15}$ and $2\pi^2me^4/h^3$ is equal, taking the most recent determination of m, e and h, to $3{\cdot}289 \times 10^{15}$.

Imagine an electron revolving in a circle about the positive nucleus of the hydrogen atom with kinetic energy $2\pi^2me^4/n^2h^2 = Nh/n^2$. Its velocity, $v = 2\pi e^2/hn$; the radius, r, of the circular orbit is found by putting $mv^2/r = e^2/r^2$, and is equal to $n^2h^2/4\pi^2me^2$. The angular momentum is $mvr = nh/2\pi$. If the electron changes its orbit from $n = n_2$ to $n = n_1$, where n_2 is greater

than n_1, its kinetic energy in the new orbit is *greater* than in the old by $Nh(1/n_1^2 - 1/n_2^2)$. But an amount of potential energy has been set free equal to $e^2(1/r_1 - 1/r_2)$, and this is equal to twice the change in kinetic energy, as is easily seen by substituting for the r's their values as found above. Consequently the right amount of energy is available for radiation.

We can, therefore, following Bohr, define the necessary separate states as those of motion in circular orbits in which the angular momentum is an integral multiple of $h/2\pi$. The simplicity of these expressions is very attractive. But the matter is far from ending here. During the last few years Bohr and Sommerfeld have led an inquiry into the possibilities of this theory which has furnished very remarkable results. These are due to a slight modification in the original conception. The different circular orbits which Bohr first pictured have become groups of orbits fixed by laws which are somewhat arbitrary, but not without foundation. A group contains a limited number of orbits in which the electrons may move, and each group corresponds to

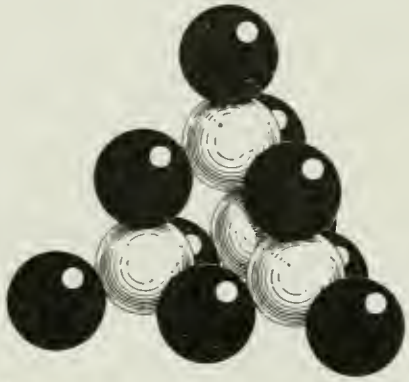


FIG. 2.—Model of the arrangement of carbon atoms in the diamond. All the atoms are alike, but those represented by light spheres differ in orientation from those represented by dark spheres.

one of the original circular orbits. Some of the orbits in each group are elliptical. It appears that the energy of the electron would be the same in all the orbits of any one group were it not that when an electron moves in an ellipse its velocity is not always the same. Now a fast-moving electron shows a variation in mass when its speed alters, and this does affect slightly the energy of the orbit. Consequently, the electron that steps from an orbit belonging to one group to an orbit belonging to another group may part with an amount of energy which is not always exactly the same. The frequency of the consequent radiation may, therefore, have two or more values differing slightly from each other: the single spectrum line is doubled or trebled. This is what Sommerfeld calls the "fine structure" of the lines.

Now there is far more than mere speculation in this. The formula which Sommerfeld gives as the result of an analysis which is as reasonable as can be expected, does more than account for known effects; it has predicted the existence of numerous lines, and even their intensities, and the predictions have been verified by experiment in the most remarkable way The story is told in Sommerfeld's work on "Atom-building": a story of the work of himself, Bohr and others during the last six years or so.

We see that in this fundamental inquiry into the nature and properties of radiation, the electron plays a very direct and important part. Our eyes are designed to detect waves, not electrons, and so our first attention is directed to radiation in wave form. But we now find that radiation energy may alternatively be carried by electrons, and that many things become clearer when we appreciate this fact. We can only make further progress in our understanding of radiation, and indeed of our understanding of the electron, by getting to know more about the reciprocal conversion of one form of energy into the other, since evidently it is one of the most frequent and most fundamental operations in nature.

So far our conception of the structure of an atom would consist of a positive nucleus, and electrons attached thereto in some way: with the further idea that the energy attached to these electrons can only have certain definite values. Bohr assumes that

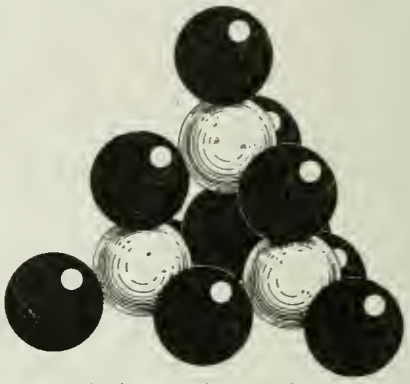


FIG. 3.—An extra ball can be inserted in the model without disturbing the others.

they have these values because they can move round the core in certain orbits only; and Sommerfeld enlarges this idea as already explained. But, of course, this can only be a partial picture of the whole atomic structure. The atom so conceived cannot fill the part required of it in the building of molecules and crystals.

When we come to examine these structures we find atoms attaching themselves to each other through the action of forces which cannot always be considered as acting from centre to centre. For instance, the arrangement of the carbon atoms in a diamond, as recently determined by X-ray methods, is such that every atom is at the centre of gravity of four others, arranged round it in tetrahedral fashion, as shown in Fig. 2. The representation of an atom by a smooth sphere and nothing more would be in agreement with the idea that the properties of the atom in any one radial direction are exactly the same as in any other radial direction, and that any forces between two atoms are between centre and centre. But if that were the case, the carbon atoms would pack themselves together more closely than they do. As a matter of fact, if the top of this model is lifted, another carbon atom can be inserted and the

top replaced exactly as it was (Fig. 3). If a more extensive model were employed it would be seen that just twice as many atoms could be packed into any volume as there are already there. We must conclude that there are definite sub-centres of force on the outskirts of the atom: and that in the carbon atoms of which the diamond is composed there are four such sub-centres arranged symmetrically, that is to say, in tetrahedron fashion round the core.

Must not these sub-centres be electrons? And if so, must we not take them to be circulating in small orbits about a local centre? Or, perhaps, as Parson has suggested, the electron is ring shaped, the electricity revolving round the axis of the ring. In this way we should have electromagnetic forces to link the atoms together.

It is very interesting to observe that in any case the carbon atoms in the diamond are not all oriented in exactly the same way. Taking a cleavage or tetrahedral plane as that of reference, half the atoms will be pointing towards the plane and their neighbours will be pointing away. This ought to make a difference to the X-ray spectra: and it has been looked for at various times, but without success. Lately, however, the improvement in the X-ray spectrometer has been considerable, and I now have no difficulty in finding the expected effect.* It is clear, I think, that the carbon atom in the diamond is to be represented as to its properties by a tetrahedron: and that the atom has different properties in different directions, or, as the chemist would say, has directed valencies. There can be little doubt that this is so in all atoms. The suggestion is that some of the electrons in an atom forming part of a crystal are tied down to certain regions on the surface, and that not all, if indeed any of them, are at all times revolving round the central core.

When atom joins up to atom it is these sub-centres that are at work; and since atom to atom and again atom to atom make in the end the crystal; and since the crystalline structure is the basis of all solid structure, and is fundamentally concerned with the strength of materials and their temper and all their physical properties, it is easy to see how great is this minute study of the electron.

If this conception of fixed electrons seems to clash with the orbital motions of Bohr and Sommerfeld, we must remember that the clash is between two pictures both of which are, we know, imperfect. We may expect that on the next occasion when a lecturer tries to tell you what advance has been made in the study of electrons some of these contradictions will have disappeared. Whether this is so or not, I am sure of this, that in the attempt to realize the properties of nature's unit, the electron, we are working in the true direction towards an understanding of the great problems of radiation, and of material structure.

* There is in fact a small second order spectrum in the reflection of X-rays by the tetrahedral plane.

A PRECISION FORM OF THE KELVIN ELECTROSTATIC VOLTMETER.*

By E. H. Rayner, Sc.D., Member.

[From the National Physical Laboratory.]

(*Paper received* 8 *June*, 1920.)

Summary.

The paper describes a form of electrostatic voltmeter for precision measurements. The instrument follows the general arrangement of the Kelvin type. The chief modifications are: (1) Very considerable reduction in weight, permitting (2) the use of a bifilar suspension to overcome the effect of elastic fatigue: (3) a modification of the shape of the needle to produce a more satisfactory "scale law" for both small and large deflections: and (4) the provision of various mechanical details to facilitate construction and adjustment.

Importance and Utility of the Electrostatic Type of Voltmeter.

In a previous paper † read before the Institution the methods and apparatus used at the National Physical Laboratory for the measurements of alternating currents have been described. In the paper it was shown how an electrostatic voltmeter is used not only for the measurement of high and low voltages, but also as a necessary link in the calibration of the standard wattmeter. It is also used for precise measurements of currents up to about 20 amperes, by passing the current through resistances of known value which are capable of giving a potential drop of from 50 to 100 volts. For larger currents resistances giving a drop of about 2 volts are used, across which the primary of a transformer of 1 : 50 or 1 : 100 ratio is connected. The secondary, giving from 50 to 100 volts, is connected to the voltmeter. By either means observations to within 1 or 2 parts in 10 000 can be made. The electrostatic type of instrument has other advantages over those of the dynamometer type. Among these are its independence of ordinary commercial frequencies, and the fact that it takes a very small capacity current which can nearly always be neglected. A further point is that, the moving part being snspended, it is practically frictionless, and great magnification of the motion of the moving part can be usefully employed.

Past Experience.

For many years there have been in use at the National Physical Laboratory Kelvin multicellular voltmeters made by Messrs. Kelvin and White, to which concave mirrors having a radius of 2 metres have been fitted. The readings are taken on a curved scale 8 metres long instead of that under the pointer of the instrument which is but 10 cm. long. By this means the indications of the instrument working up to 150 volts can be read to 0·01 volt between 50 and 120 volts. Such a great magnification is only practicable because the instrument is suspended by a torsion strip and is frictionless, which would not be the case if it were necessary to use pivots.

Under these conditions, however, certain imperfections become appreciable which are quite negligible when the instrument is used in the manner for which it is designed. The most important trouble is due to the elastic fatigue of the suspension. It may take several hours after the application of 100 volts for the deflection to become steady to 0·01 volt, and if, after this steadiness has been obtained, a lower voltage is applied, the "creep" comes out to some extent and the deflection falls steadily for some time. At 100 volts this effect may amount to 0·1 volt or more, and when accurate readings are required time is taken in calibrating which would not be necessary if an instrument with greater steadiness of deflection were available. Another disadvantage of the ordinary type of instrument is that the moving part is unnecessarily heavy for an instrument which is not intended to be portable, and its time of swing may be shortened by using a lighter moving system. In fact the time of coming to rest after the application of 100 volts has been reduced from about 50 seconds to 15. These are matters of great importance in practice, as the instrument is used for the testing of all commercial voltmeters, wattmeters and watt-hour meters, and is frequently in use for the whole of the working day.

Preliminary experiments showed that it was practicable to reduce the weight sufficiently to enable a bifilar suspension to be used, and thereby to reduce greatly the effect of the elastic fatigue. As the weight of the moving part supplies most of the controlling force in a bifilar suspension, it is necessary to reduce it as much as possible, if the distance between the supporting wires is not to be too small for satisfactory operation.

Description of Instrument.

Two instruments have been built, reading up to about 130 volts on a scale 8 metres long with a bifilar suspension 400 mm. long and 3·5 mm. apart (see Fig. 1). For the sake of comparison and possible interchangeability of parts, the vertical distance between the needles was made the same, i.e. 8·7 mm. (11/32 inch), as in the older Kelvin and White instru-

* The Papers Committee invite written communications (with a view to publication in the *Journal* if approved by the Committee) on papers published in the *Journal* without being read at a meeting. Communications should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

† C. C. Paterson, E. H. Rayner and A. Kinnes: "The Use of the Electrostatic Method for the Measurement of Power" *Journal I.E.E.*, 1913, vol. 51, p. 294.

ments. The number of needles for which it is desirable to arrange is a matter for compromise. In spite of the considerable reduction in weight of the moving part it is amply strong enough to carry a large number

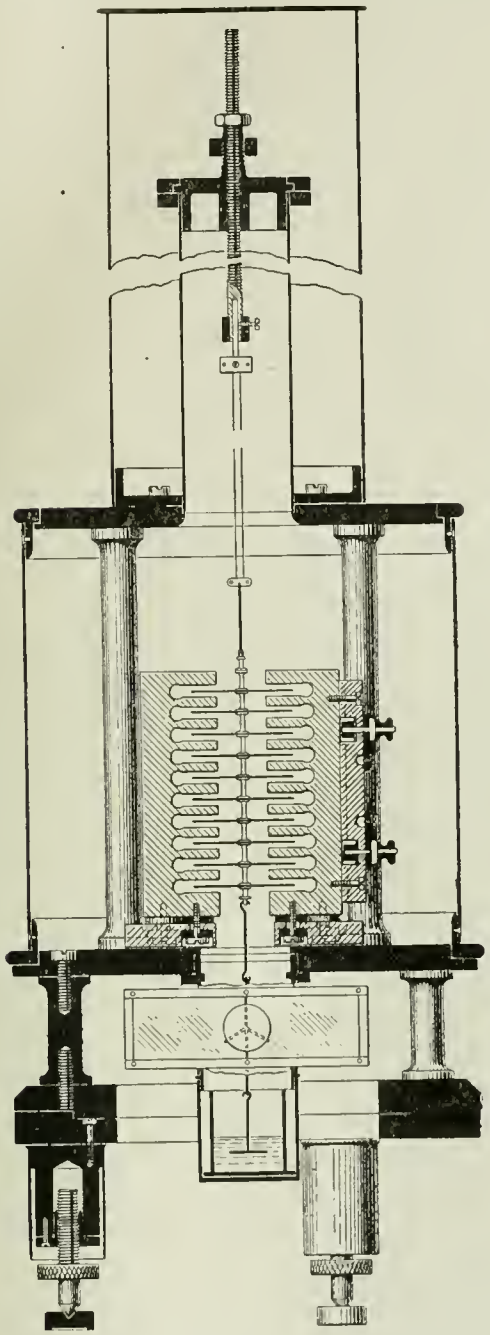


Fig. 1.—Sectional elevation of the voltmeter.

of needles. The total weight does not increase as fast as the number of needles, as there are the end fittings to be taken into account and a large mirror 20 mm. in diameter is used. It is also desirable to have a considerable number of needles to provide sufficient motive power to move it quickly. The only disadvantage in using a large number of needles is the increase in capacity of the instrument, but as a good deal of this is extraneous to the moving system, being due to the capacity between inductors and case and to subsidiary apparatus and connecting wires, the capacity does not increase as fast as the number of needles. The number fixed upon is 10. They are made of special aluminium-copper-manganese rolled alloy and are about 0·02 mm. thick. They are threaded on to a wire screwed at both ends. Spindle-shaped distance pieces of aluminium alloy keep them at the correct distance apart. The nuts at the ends have holes through them, by the top one of which the needle system is hung by a hook of wire of square section to the bridge-piece clamping the suspension wires, and to the bottom one the mirror and damping disc are suspended. The disc is immersed in dashpot oil as usually supplied with Kelvin voltmeters. The diameter of the disc is adjusted until

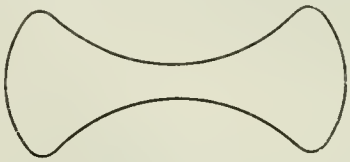


Fig. 2.—Type of needle usually adopted in the Kelvin and White voltmeters.

suitable damping is obtained. The square section of hooks and parts connected to them provides a four-point contact which prevents relative rotation about a vertical axis.

Suspending wires.—The suspension wires are of tungsten, and are about 0·02 mm. in diameter. This material was chosen as being very strong and easily obtainable in such a small size. It is the finest ordinarily used in vacuum lamps, and was specially annealed by the makers, the Osram-Robertson Lamp Company. The breaking strength of the wires is such that the mechanical factor of safety is well over 100.

Shape of the needle.—The shape of the needle is of great importance in determining the length of scale per volt at various parts of the scale. For the first part of the scale this type of instrument follows approximately a "square" law, the increase of deflection per volt being nearly proportional to the voltage applied.

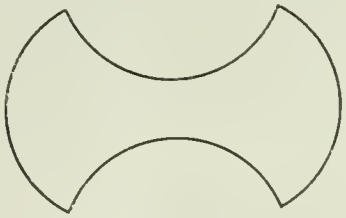


Fig. 3.—Form of needle adopted by the author.

Above half-scale deflection the effect is reversed, due partly to the needle being largely inside the inductors and partly to the presence of the guard plate. Needles of the shape usually provided by Kelvin and White (see Fig. 2) and others of different shapes, both symmetrical and non-symmetrical, were tried. The shape

shown in Fig. 3 was finally adopted. The object of these trials was to open out the deflections of the instrument at the top and bottom of the scale, and to keep the rate of change of scale-length per volt small at 100 volts, at which point the instrument is chiefly used. These qualities and other minor desiderata are to some extent antagonistic, and a compromise has to be adopted. Certain commercial electrostatic instruments are made with unsymmetrical needles with the object of greatly increasing the deflections for a certain range of pressure. This can be done by increasing the rate of change of capacity over such ranges. The indications of an instrument of this nature are liable to vary considerably with small changes in the initial position of the needle between the inductors. Since permanency in this respect is of first importance, and a symmetrical needle was found to satisfy practical requirements, it was adopted as being simpler to make.

It was found that a needle of the form shown in Fig. 3 would give twice as much deflection per volt over the lower part of the scale as that given by needles of the shape usually provided by Messrs. Kelvin and White. At the same time the openness at the top of the scale is also doubled.

Inductors.—The inductors are similar in plan to those of the ordinary Kelvin instrument, but, the metal being thicker, the air space is less. This was done so that experiments could be made, if desired, on the effect of cutting the metal fins thinner, but, satisfactory results having been obtained with the proportions as first made, they have not been altered. The inductors are of cast aluminium alloy and are mounted on sliding bases from which they are insulated by amberite discs. These bases slide between guides fixed on the base of the instrument, and a millimetre scale is provided so that they can be set accurately at any desired distance apart, and can be easily separated when it is desired to remove the needle. Two of the guides have holding-down screws with knurled heads for clamping the slides when in position. An ebonite block is mounted on the back of one of the inductors for the support of a fine wire fuse. The current to the inductors passes through a terminal in the base of the instrument which is bushed with amberite, and then through the fuse, the purpose of which is to limit the current in case of an accidental excess of pressure being applied to the instrument. In such cases the needles may be attracted on to the inductors, and so cause a short-circuit. The provision of the fuse will help to prevent the damage to the needles which might occur in such circumstances. A spring support is provided for taking the weight of the needle system at the bottom while the instrument is being assembled, and behind the inductor on the right is a pillar for supporting two horizontal strips of brass. These can embrace the top of the needle system by a scissor motion, limited by a stop. These two details, not shown in the drawing, enable the needle to be disconnected from the suspension without trouble.

Subsidiary details.—The lower part of the base of the instrument consists of a ring supported by three ebonite pillars fitted with levelling screws. The ebonite is protected from light by a metal sleeve. The instrument proper is supported by three metal pillars on a second ring which can rotate on the lower part of the base. No locking screws are required, as the instrument is not intended to be portable. The three metal pillars are not arranged at 120°, but one of the spaces between them is larger to prevent obstruction of the light reflected from the mirror, which sweeps over an angle of about 120°. The mica window is very thin and of large radius, since even a thin one, bent to sharper curvature, seriously impairs the definition on the scale. Nearly the whole of the instrument, with the exception of tubular parts such as the outer casing and dust cover, is made from aluminium alloy, partly for the sake of lightness and partly because it is an excellent alloy for machining. The main plate has four vertical pillars fixed to it, and these support the upper plate to which the long tube carrying the upper part of the suspension is fixed.

A considerable range of vertical adjustment for different lengths of suspending wires is provided by the screwed spindle, which has a key-way cut in it, and a feather in its housing prevents rotation. The housing can be rotated when it is desired to vary the starting position of the needle in relation to the inductors, and it is clamped by three knurled screws passing through the outer ring at the top of the tube. A split tube and a conical nut clamp the housing tightly round the screwed spindle to ensure rigidity.

An outer brass tube acts as a dust cover and protection for the top of the suspension.

Source of light.—A Nernst lamp, or a gas-filled lamp of 2 to 3 amperes at 10 volts, with an achromatic condensing lens 2 inches in diameter and about 8 inch focus, having a fine wire stretched vertically across the lens, is used as a source of light. The projected and reflected beams are as nearly as practicable in the same horizontal plane. If there is much divergence between them, the motion of the spot of light on the scale will move considerably in a vertical direction towards the ends of the scale. The shape of the scale is such that the ends are much nearer to the instrument than the centre, an arrangement necessary to obtain good definition over the whole length of the scale.*

Contact difference of potential.—Preliminary experiments with an instrument of Messrs. Kelvin and White, provided with inductors made of sheet aluminium, gave rise to the expectation that with inductors and needle of alloys of very similar composition the difference on reversal of a continuous potential of 100 volts would not exceed a few hundredths of a volt. This was at first by no means the case, the difference on reversal of 100 volts being found to correspond to 2·6 volts, the needle being made of rolled alloy and the inductors of cast alloy. As the usual value for an instrument with aluminium needle and brass inductors is about 0·35 volt, the result was that the difference, when using similar metals, was nearly eight times that of an instrument having dissimilar metals.

After various experiments it was found that by boiling the inductors for some time in a solution of soap and washing soda this difference on reversal was very much diminished. It may take several weeks

* Paterson, Rayner and Kinnes, *loc. cit.*, p. 324. E. H. Rayner, *Proceedings of the Optical Convention*, 1912, and also *Proceedings of the Physical Society*, 1911–1913, vol. 24, p. 449

to get steady and the difference may be expected to settle down to the order of 0·1 volt. In one case the difference died slowly down to zero, giving rise to the hope that it would stay there, but it went on further and became steady at a small negative value.

The reason appears to be that the spaces in the inductors were milled out with a cutter which was kept oiled, and the surface of the metal was never fully exposed to the air, so as to become what one may term "normalized." The removal of the oil by boiling with alkali allowed the natural air-metal interface to be formed, producing a condition of surface much more similar to that of the moving system.

In the case of the two voltmeters which have now been in use for some six years the value is about 0·04 volt for the one and about four times as much for the other. It is therefore impossible to say beforehand what the value will be, and experiment may be required if it does not come out, at the first attempt, as small in value as is desired. To obtain the smallest possible difference on reversal it might be the best course, since the needles have to be made of very ductile metal, to build up the inductors of sheet metal of the same composition.

Construction and manufacture.—The instruments have been made in the Laboratory and the author is greatly indebted to Mr. Murfitt, the head of the instrument shop, for the excellence of their construction.

The Director of the National Physical Laboratory is prepared to consider requests for the provision of working drawings of the instrument.

The following appendixes give details as to weight and inertia of the moving parts, method of making the needles, and a device for mounting the suspending wires.

APPENDIX 1.

Details of the Moving System.

The details of the suspended system are as follows :—

Bridge piece clamping suspension wires	0·16 gramme
Needles (10)	0·30 ,,
Distance pieces (10)	0·80 ,,
Spindle and end nuts	0·33 ,,
Mirror alone	0·60 ,,
Mirror support	0·16 ,,
Hook, short	0·07 ,,
Hook, long	0·11 ,,
Damper and hook	0·07 ,,
Total	2·60 grammes

Moment of inertia determined by comparing time of swing with that of a mass of known moment	0·59
Radius of gyration	0·47
Diameter of suspension wires	0·002 cm.
Length	40 cm.
Separation	0·35 cm.
Time of swing (undamped)	4·5 secs.

APPENDIX 2.

Method of Making the Needles.

The needles have been made by means of a template of the shape required, made out of brass $\frac{1}{8}$ inch thick. The aluminium foil is placed between the template and a larger brass plate, and is cut out by a sharp knife, using the template as a guide. A more highly-finished edge might be produced by using a die-punch, but this would be very expensive. The clearance between needle and inductors is relatively so large that any slight unevenness in the edge is negligibly small in comparison.

To form the template, three holes 7 mm. apart in a line were drilled in it. The outer ones were used for centring it in turn on the face-plate of a lathe by a pump-centre. They formed the centres of the convex parts. The lathe headstock was rotated backwards and forwards about half a turn, the cutting tool being held in the slide-rest. The concave parts were formed as in an ordinary boring operation. The edges have all the same curvature, i.e. 15 mm.

APPENDIX 3.

Assembling the Parts of the Moving System.

The ideal way of arranging for the assembly of the moving system would be to provide some registering arrangement so that all the needles would come together with their axes of symmetry in the same vertical plane. This could be done by having two vertical pins in one flange of each distance piece, fitting into corresponding holes in the needle and into holes in the adjoining flange of the next distance piece. The only difficulty would be in the smallness of the parts involved. Such arrangement was not found necessary, but since the instruments were constructed, jigs have been developed for doing this, and such a scheme will be used in further instruments. To assist in keeping the needles parallel while tightening up the end nut, the concave edges have been laid on a cylinder of wood of the same curvature (15 mm. radius) and 90 mm. long. The needles, when placed on edge in this manner, are just strong enough to support the spindle and distance pieces. The fit of the cylinder causes them to take the proper position, after which the nut is tightened up. It is convenient to attach a small loop of silk or cotton to the hole in the upper nut to hold it while being put in position in the instrument. Before hooking it to the suspension the thread is cut away.

APPENDIX 4.

Method of Mounting the Suspension Wires.

If the necessity arises for renewing the suspension wires it is important to be able to mount them at a definite distance apart in order to make the deflections of the instrument fit the scale, which has to be divided empirically.

For this purpose a simple jig has been devised for ensuring that the necessary conditions can be easily fulfilled. This is shown in Fig. 4. The free end of the wire is clamped under the screw A_1. It passes the upper part of the block B which acts as a temporary holder for the upper suspension clamp K. It then passes a small cylinder C_1 which has a comparatively large clearance hole for its fixing screw, and goes over the main part of the upper clamp. At a distance of 40 cm it passes the lower clamp L and round a screw D which has a cylindrical part turned to 3·5 mm. diameter or other required dimension. It is then crossed over and passes the screw E, which need not be of any special size. The object of this is to keep the wire tight against the cylinder D. It is then looped round a hooked end of a wire spring H. It passes back in a similar manner and is fixed by the screw clamp A_2.

The two cylinders C_1 and C_2 which determine the separation of the top of the suspension wires, are fixed at any desired distance by the fit of a small brass rod turned to the requisite diameter. When this is just a fit between the cylinders the fixing screws are tightened.

Two thin brass strips S_1, S_2 are screwed to the base board in order to keep the lower bridge-piece square with the wires while screwing it up.

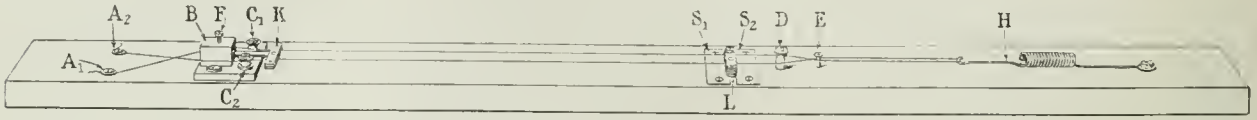


Fig. 4.—Form of jig for mounting suspension wires.

When the wires are taut the parts of the clamps with clearance holes in them are screwed down. The surplus wire is then cut off, the screw F is loosened and the suspension is ready for clamping to the central spindle of the voltmeter. It is desirable to allow for a margin of sensitivity by spacing the wires a few per cent further apart than is necessary. The sensitivity is finally adjusted by slipping small weights in the form of rings of thin wire on to the damper spindle.

THE WHITENED CUBE AS A PRECISION INTEGRATING PHOTOMETER.*

By H. Buckley, B.Sc.

[From the National Physical Laboratory.]

(*Paper received* 2 *July*, 1920.)

Summary.

The paper indicates how a whitened cube can be made to give accurate results when used in the determination of the average candle-powers of lamps having different polar distributions. As a preliminary, the relative effects of different portions of the interior of the cube in producing illumination on the window are determined, and the results used to calculate the illuminations produced by different types of light distribution. The ratios of these illuminations to that furnished by a uniform point source of unit intensity give apparent average candle-powers in the cube. It is then shown by experiment that candle-powers obtained by the usual methods of integrating photometry agree with these calculated values. Thus it is possible to determine by how much these apparent candle-powers differ from true candle-powers deduced by point-to-point methods, and hence to obtain true candle-powers from experimentally determined apparent values. In the special case when a comparison is made between lamps having identical polar distributions, it is shown that the usual method gives accurate results.

Introduction.

For several years past a 2-metre cube has been in use at the National Physical Laboratory, for determining the average candle-power of lamps whose polar distributions have not differed very much from that of the standard lamp with which they are compared. It has been recognized that such results are, to a certain extent, approximate, and it is the object of the present series of experiments to arrive at some estimate of the magnitude of the errors which may be involved in such determinations, and to derive principles which will tend to minimize these errors. The chief reasons which led to the use of the cube, especially of a large-sized one, rather than a sphere, are the greater ease of construction and the increased facility for installing and manipulating such heavy apparatus as arc-lamp projectors, as the whole of one side can be made to open for such purposes.

Dr. Sumpner † first suggested the use of a rectangular box instead of a sphere, and claimed that to a fairly close approximation it would give as good results as the sphere. In the discussion on Dr. Sumpner's paper Professor G. W. O. Howe described a box with 3-ft. edges which, he stated, acted satisfactorily. He also suggested the use of triangular pieces of cardboard to block up the corners so as to make the box approximate more closely to a sphere.

L. W. Wild,* as a result of this discussion, experimented with a rectangular box, and obtained photometric differences up to 4 per cent. His box was rather small, and his differences would probably be diminished by the use of a larger box.

Grondhal † in America has also described experiments with an oblong rectangular box, a box with the corners eliminated, and also a cube with the corners eliminated. He comes to the conclusion that for sources of similar distribution a rectangular box may be substituted for a sphere, and that a cubical box with the corners eliminated is for practical purposes a satisfactory substitute for a sphere, even for dissimilar and asymmetric sources.

Theoretical Outline.

In the case of the Ulbricht sphere the illumination at the window is proportional to the total flux from the source, provided it receives no direct light from this source. In the case of the cube, however, this does not hold, the illumination on the window being a function of the first point of incidence of any part of the flux as well as of the total flux. For the same point of incidence the illumination on the window will be proportional to the total flux, but the proportionality factor, or "contribution coefficient" as it may be conveniently called, varies from point to point in the cube. This variation of the contribution coefficient has been determined for the whole of the cube. If, now, a source of known polar distribution and average candle-power be placed in the middle of the cube, we may calculate the flux falling on each area of the walls, and this, multiplied by the contribution coefficient for that area and summed over the whole surface of the cube, will give a quantity which, when divided by the same summation for a unit point source at the centre, will give the ratio of their apparent total fluxes or, what is proportional to it, the apparent average candle-power of the source when in the cube. We have thus a method of determining in a cube by how much the apparent average candle-powers differ from the actual average candle-power determined by a point-to-point method. This again can be compared with the average candle-power obtained by the usual experimental comparison of two lamps, for one of which this value is known.

The Present Investigation.

In the present investigation an Aldis daylight signalling lamp (Fig. 1) was used as the source of a constant flux. It consisted of a gas-filled lamp with twin spiral

* The Papers Committee invite written communications (with a view to publication in the *Journal* if approved by the Committee) on papers published in the *Journal* without being read at a meeting. Communications should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

† *Illuminating Engineer* (London), 1910, vol. 3, p. 323.

* *Loc. cit.*, p. 549.

† *Transactions of Illuminating Engineering Society*, 1916, vol. 11, pt. 2, p. 152.

filaments at the focus of a 4-inch Mangin mirror. Only the light emitted towards the mirror was allowed to come from the projector, and this was effected by

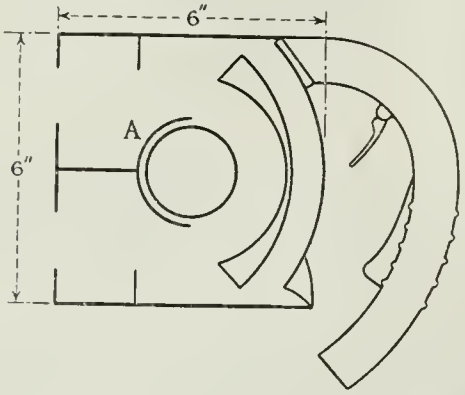


Fig. 1.—Aldis daylight signalling lamp (diagrammatic).

placing the blackened shield A over the half of the lamp away from the mirror. This resulted in a beam which had a diameter of about 17 cm. at a distance of 1 metre from the projector. The exterior was whitened, as was also the retort stand which held it in position. Throughout the experiments the projector was in the middle of the cube, and it was oriented so that the beam from it fell approximately in the centre of each of the 16 squares into which each quarter side of the cube was divided. Experiments were made on the variations of candle-power of this lamp at different angles of tilt. It was found that these were extremely small and could be neglected. A small screen of about 15 cm. diameter was placed half-way between the projector and the window. It was not necessary in this case, as direct light from the source was never incident on the window, but, as in actual practice it would be necessary, it was placed in position and remained so during all the observations. These observations were not conducted over the whole of each side, as considerations of the symmetry of the system show this to be unnecessary. In Fig. 2, which shows the squares into which each type of wall is divided, it is obvious that the contribution coefficient will be the same for corresponding squares in A, A′, A″, A‴ which are on the wall facing the window; B, B′ the halves of the sides, top, and bottom further from the window; C, C′ the nearer halves of the same walls; and D, D′, D″, D‴ on the wall containing the window. The observations are the means of two sets of five settings of the photometer by each of two observers. The illumination on the window was not sufficiently large for convenience in working with a Lummer Brodhun photometer and comparison lamp of about 20 candle-power, without the use of a mirror in the photometer head. This can be seen diagrammatically in Fig. 3, where the mirror (M) is inclined at an angle of about 60° with the axis of the photometer bench. The effect of the mirror is that instead of matching the brightness of the photometer screen as illuminated by the window, against the bench comparison lamp, the image of the window in the mirror is used, this being six or seven times brighter. On the same side of the photometer as the comparison lamp a blue filter (Wratten photometric 78B of transmission approximately 54 per cent) was placed, and this gave a fairly good colour match between the gas-filled lamp working at an efficiency of 1·25 candles per watt and the tungsten-filament vacuum lamp at 0·70 candle per watt.

Check readings were taken from time to time with the beam incident on the middle of the wall facing

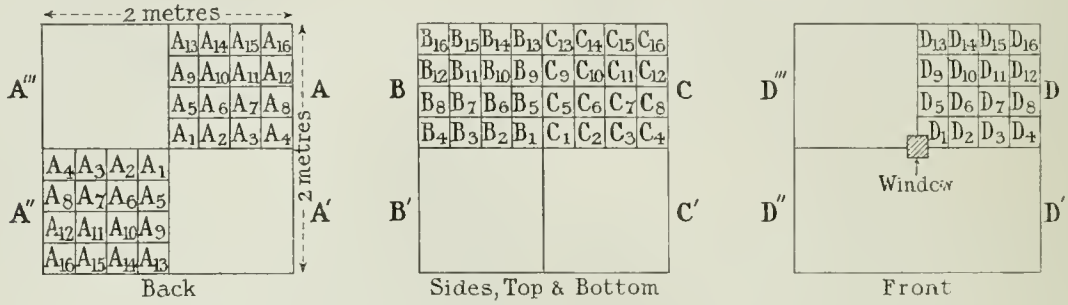


Fig. 2.—Types of wall, showing division into small squares.

the window, and results for other places were corrected to correspond to the slight variation obtained due to changes in the projector lamp.

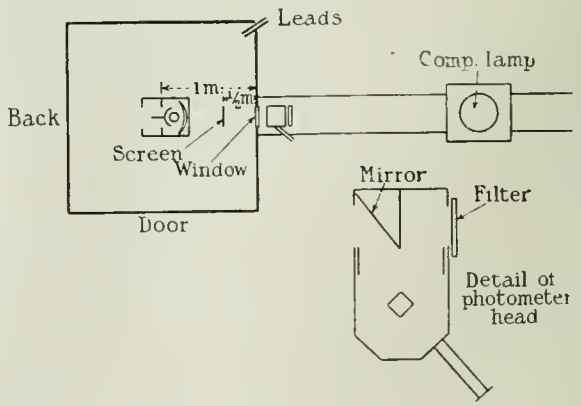


Fig. 3.—Diagram of apparatus.

Now, if L be the total flux in the beam of the projector, C the effective candle-power of the bench

comparison lamp with colour filter, and x the distance of this lamp from the photometer at balance

$$\frac{C}{x^2} = bL, \text{ where } b \text{ is a constant}$$

$$\therefore\ b = C/Lx^2$$

so that the contribution coefficient, as b has been called, is proportional to $1/x^2$.

Table 1 gives the results of the survey of the cube over the back and front. Table 2 gives the results over the sides, top and bottom.

TABLE 1.

Position	Reading ($=x$), mm.	$\frac{1}{x^2}\times 10^7$	Position	Reading ($=x$), mm.	$\frac{1}{x^2}\times 10^7$
A_1	1 199	$6\cdot96=a_1$	D_1	1 158	$7\cdot46=\delta_1$
A_2	1 022	$9\cdot57=a_2$	D_2	1 156	$7\cdot48=\delta_2$
A_3	1 068	$8\cdot77=a_3$	D_3	1 180	$7\cdot18=\delta_3$
A_4	1 092	$8\cdot38=a_4$	D_4	1 197	$6\cdot98=\delta_4$
A_5	1 056	$8\cdot97=a_5$	D_5	1 150	$7\cdot56=\delta_5$
A_6	1 031	$9\cdot41=a_6$	D_6	1 168	$7\cdot33=\delta_6$
A_7	1 081	$8\cdot55=a_7$	D_7	1 186	$7\cdot11=\delta_7$
A_8	1 110	$8\cdot12=a_8$	D_8	1 210	$6\cdot83=\delta_8$
A_9	1 049	$9\cdot09=a_9$	D_9	1 172	$7\cdot28=\delta_9$
A_{10}	1 061	$8\cdot88=a_{10}$	D_{10}	1 171	$7\cdot28=\delta_{10}$
A_{11}	1 100	$8\cdot26=a_{11}$	D_{11}	1 194	$7\cdot01=\delta_{11}$
A_{12}	1 118	$8\cdot00=a_{12}$	D_{12}	1 220	$6\cdot72=\delta_{12}$
A_{13}	1 072	$8\cdot70=a_{13}$	D_{13}	1 182	$7\cdot16=\delta_{13}$
A_{14}	1 088	$8\cdot45=a_{14}$	D_{14}	1 181	$7\cdot17=\delta_{14}$
A_{15}	1 124	$7\cdot92=a_{15}$	D_{15}	1 216	$6\cdot76=\delta_{15}$
A_{16}	1 136	$7\cdot75=a_{16}$	D_{16}	1.221	$6\cdot71=\delta_{16}$

TABLE 2.

Position	Reading ($=x$), mm.	$\frac{1}{x^2}\times 10^7$	Position	Reading ($=x$), mm.	$\frac{1}{x^2}\times 10^7$
B_1	1 105	$8\cdot19=\beta_1$	C_1	1 098	$8\cdot29=\gamma_1$
B_2	1 108	$8\cdot14=\beta_2$	C_2	1 097	$8\cdot31=\gamma_2$
B_3	1 099	$8\cdot28=\beta_3$	C_3	1 120	$7\cdot98=\gamma_3$
B_4	1 105	$8\cdot19=\beta_4$	C_4	1 165	$7\cdot37=\gamma_4$
B_5	1 101	$8\cdot25=\beta_5$	C_5	1 101	$8\cdot25=\gamma_5$
B_6	1 108	$8\cdot14=\beta_6$	C_6	1 114	$8\cdot06=\gamma_6$
B_7	1 108	$8\cdot14=\beta_7$	C_7	1 129	$7\cdot84=\gamma_7$
B_8	1 098	$8\cdot29=\beta_8$	C_8	1 176	$7\cdot23=\gamma_8$
B_9	1 116	$8\cdot03=\beta_9$	C_9	1 126	$7\cdot88=\gamma_9$
B_{10}	1 115	$8\cdot05=\beta_{10}$	C_{10}	1 138	$7\cdot72=\gamma_{10}$
B_{11}	1 130	$7\cdot83=\beta_{11}$	C_{11}	1 152	$7\cdot53=\gamma_{11}$
B_{12}	1 110	$8\cdot12=\beta_{12}$	C_{12}	1 188	$7\cdot08=\gamma_{12}$
B_{13}	1 119	$7\cdot99=\beta_{13}$	C_{13}	1 138	$7\cdot72=\gamma_{13}$
B_{14}	1 121	$7\cdot96=\beta_{14}$	C_{14}	1 156	$7\cdot48=\gamma_{14}$
B_{15}	1 119	$7\cdot99=\beta_{15}$	C_{15}	1 191	$7\cdot05=\gamma_{15}$
B_{16}	1 125	$7\cdot90=\beta_{16}$	C_{16}	1 205	$6\cdot89=\gamma_{16}$

The variation of $1/x^2$, the contribution coefficient along several lines on the walls, is shown graphically in Fig. 4.

For any given polar distribution we can now find a quantity proportional to its apparent total flux or average candle-power in the cube. If F be the flux incident on any small square, and a the contribution coefficient for that square, then $\Sigma(Fa)$ taken over the whole cube is such a quantity. Let l = semi-side of cube = 8 units; let the source be at the middle of the cube, i.e. distant l from the centre of each side, and let x_1y_1, x_2y_2, etc., be the co-ordinates of the mid-points of the small squares $A_1 \ldots A_{16}$, $B_1 \ldots B_{16}$, $C_1 \ldots C_{16}$, $D_1 \ldots D_{16}$, referred to an origin at the mid-point of the wall over which the summation is

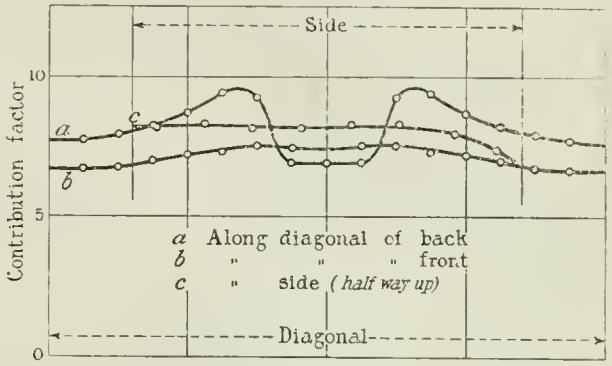


FIG. 4.—Variation of "contribution factor" across the cube.

being taken. Then the flux incident on any small square of area $(l/4)^2$

$$= \frac{\text{area} \times \cos(\text{angle of incidence})}{(\text{distance from source to centre of area})^2} \times (\text{c.p. in direction of centre of area})$$

$$= \frac{l^2}{4^2} \cdot \frac{l}{(l^2+x^2+y^2)^{1/2}} \cdot \frac{1}{l^2+x^2+y^2} \cdot \Phi_\theta$$

(See Fig. 5)

where $\Phi\theta$ is the candle-power of the source in a direction making an angle θ with the reference plane of the lamp.

$\Sigma(Fa)$ then becomes

$$\frac{l^3}{4^2}\sum\left\{\frac{a\Phi_\theta}{(l^2+x^2+y^2)^{3/2}}\right\}$$

The values of $(l^2+x^2+y^2)^{3/2}$ for the mid-points of each of the areas $A_1 \ldots A_{16}$, which are the same as those for corresponding areas in $B_1 \ldots B_{16}$, $C_1 \ldots C_{16}$, $D_1 \ldots D_{16}$, are given in Table 3.

TABLE 3.

Area	$(l^2+x^2+y^2)^{3/2}$	Area	$(l^2+x^2+y^2)^{3/2}$
A_1	536	A_9	854
A_2	636	A_{10}	969
A_3	853	A_{11}	1 218
A_4	1 218	A_{12}	1 621
A_5	637	A_{13}	1 218
A_6	742	A_{14}	1 346
A_7	969	A_{15}	1 621
A_8	1 346	A_{16}	2 062

In evaluating the values of θ, which is the angle between the reference plane and the direction of emission of the flux, two cases have to be considered :—

Case (1).—When the direction of emission is incident on a wall of the cube perpendicular to the reference plane.

Case (2).—When the direction of emission is incident on a wall parallel to the reference plane. The former is shown in Fig. 5, and it can be seen that

$$\theta' = \text{arc sin } \frac{y}{(l^2 + x^2 + y^2)^{1/2}}$$

In the latter case (see Fig. 5)

$$\theta'' = \text{arc sin } \frac{l}{(l^2 + x^2 + y^2)^{1/2}}$$

The values of θ' and θ'' are given in Table 4 for each of the areas $A_1 \ldots A_{16}$, which of course have corresponding squares on the other walls.

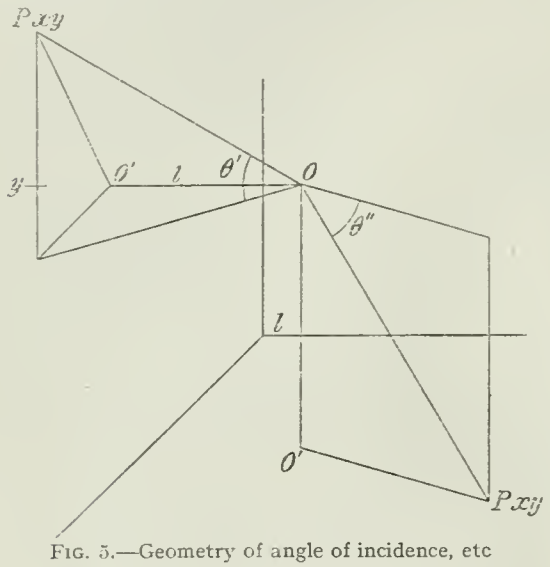


Fig. 5.—Geometry of angle of incidence, etc

$\Sigma(Fa)$ for Various Polar Distributions.

(1) *A point source.*—In this case $\Phi_\theta = \text{const.} = \Phi$, the candle-power of the source. The summation $\Sigma(Fa)$ is made up of three parts :—

(1) due to the back of the cube; this equals four times the summation over the small squares $A_1 \ldots A_{16}$

$$= 4\Phi \frac{l^3}{4^2} \sum \left\{ \frac{a}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

(2) due to the front as above, but over the squares $D_1 \ldots D_{16}$

$$= 4\Phi \frac{l^3}{4^2} \sum \left\{ \frac{\delta}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

(3) due to sides, top and bottom; this equals 8 times the summations over the small squares $B_1 \ldots B_{16}$, $C_1 \ldots C_{16}$,

$$= 8\Phi \frac{l^3}{4^2} \sum \left\{ \frac{\beta + \gamma}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

Now $l = 8$ units

$$\therefore \Sigma(Fa) = 128\Phi \sum \left\{ \frac{a + 2(\beta + \gamma) + \delta}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$= 100{\cdot}3\ \Phi$ in the particular case of the measurements quoted in Tables 1 and 2.

Table 4.

Area	θ'	θ''	Area	θ'	θ''
A_1	7°	80°	A_9	32°	57°
A_2	6°	68°	A_{10}	30°	54°
A_3	6°	58°	A_{11}	28°	49°
A_4	5°	49°	A_{12}	25°	43°
A_5	20°	69°	A_{13}	41°	49°
A_6	19°	62°	A_{14}	40°	46°
A_7	18°	54°	A_{15}	37°	43°
A_8	16°	46°	A_{16}	33°	39°

(2) *A squirrel-cage metal-filament vacuum lamp.*—This lamp had a polar distribution as shown in Fig. 6. Measurements were made at the Russell angles (ten in each quadrant) whilst the lamp was rotating, and the average candle-power was deduced as 114·0.

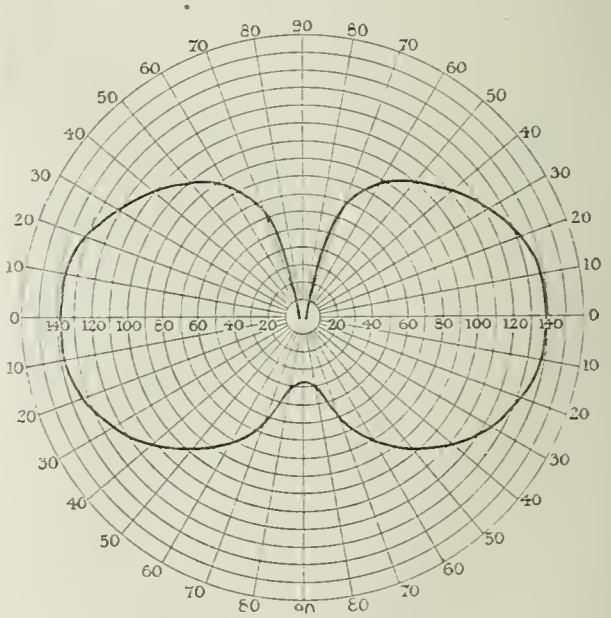


Fig. 6.—Polar curve of Osram reference lamp No. 2.

The summations in this case consist of four parts due respectively to

$$\text{(1) back} = 4 \cdot \frac{l^3}{4^2} \sum \left\{ \frac{a\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(2) front} = 4 \cdot \frac{l^3}{4^2} \sum \left\{ \frac{\delta\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(3) sides} = 4 \cdot \frac{l^3}{4^2} \sum \left\{ \frac{(\beta + \gamma)\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(4) top and bottom} = 4 \cdot \frac{l^3}{4^2} \sum \left\{ \frac{(\beta + \gamma)\Phi_{\theta''}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\therefore \Sigma(F\alpha) = 128 \sum \left\{ \frac{(\alpha + \delta)\Phi_\theta}{(l^2 + x^2 + y^2)^{3/2}} \right\} + 128 \sum \left\{ \frac{(\beta + \gamma)(\Phi_{\theta'} + \Phi_{\theta''})}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$= 11\ 336$$

In calculating these summations the values of $\Phi_{\theta'}$ and $\Phi_{\theta''}$ are those read off directly from the polar diagram at the angles given in Table 4. The ratio of this to the summation for a unit point source gives a result of 113·0. Thus a lamp with the polar distribution shown in Fig. 6 behaves in the cube in the same way as does a point source (to a fairly close approximation) and may therefore be used instead of an ideal point source for the purpose of calibrating the cube for measurements of other lamps.

(3) *A squirrel-cage metal-filament vacuum lamp with close-fitting shade. (Axis of lamp vertical.)*—This lamp is illustrated in Fig. 7, and its polar distribution given in Fig. 8. It was set up with the centre of the edge of the shade at the mid-point of the cube. It was anticipated that a distribution of light of this kind, in which only the lower half of the cube received any direct light, would be one in which the value of $\Sigma(F\alpha)$ would show deviations from proportionality with the average candle-power. In this case the average candle-power as given by a point-to-point method was 422.

As before, the summation consists of four portions

$$\text{(1) back} = 2 \frac{l^3}{4^2} \sum \left\{ \frac{a\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(2) front} = 2 \frac{l^3}{4^2} \sum \left\{ \frac{\delta\Phi_\theta}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(3) sides} = 2 \frac{l^3}{4^2} \sum \left\{ \frac{(\beta + \gamma)\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\text{(4) bottom} = 2 \frac{l^3}{4^2} \sum \left\{ \frac{(\beta + \gamma)\Phi_{\theta''}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

$$\therefore \Sigma(F\alpha) = 40\ 525$$

On comparing this with the summation for a unit point source it can be seen that with this distribution the cube has the effect of making the apparent candle-power 404 instead of 422. Thus from theoretical considerations we may say that lamps having polar distributions similar to (1) that of a point source, (2) that of Fig. 6, and (3) that of Fig. 8, may be compared among themselves for average candle-power to an accuracy of about 4 per cent, if they are situated in the cube in the same way as the above calculations have assumed.

This statement is capable of experimental verification. The two lamps whose polar curves have been given were introduced into the middle of the cube, and photometric balance was obtained in the usual way. The mirror in the photometer was dispensed with, as the illumination of the window was satisfactory in each case. The means of five observations by each of two observers gave

(1) for the 114·0-c.p. lamp, 1 172 and 1 166 mm. .. .. .. Mean = 1 169
(2) for the 422-c.p. lamp, 622 and 616 mm. .. .. Mean = 619

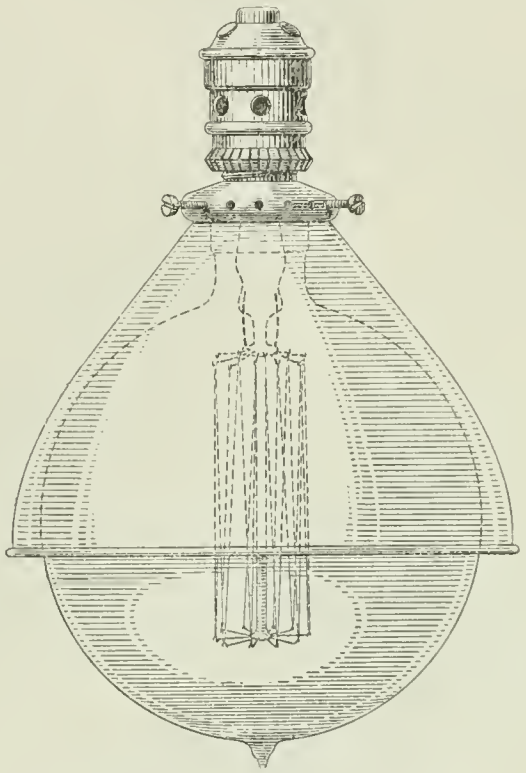


Fig. 7.—Standardized 1 000-watt, 240-volt lamp, and shade (shade whitened).

The comparison should of course be made with one lamp behaving as point source. The results of the last section, however, indicate that the 114·0-c.p. lamp is not very far from fulfilling such a condition.

$$\therefore \frac{\text{Average candle-power of 422-c.p. lamp}}{\text{Average candle-power of 114·0-c.p. lamp}} = \left(\frac{1\ 169}{619}\right)^2$$

$$\therefore \text{Average candle-power of 422-c.p. lamp} = (1\ 169 \div 619)^2 \times 114{\cdot}0 = 407$$

This result is very close to that which the summation leads one to expect.

Now the summation on the 114·0-c.p. lamp leads

to the result that in the cube it behaves as if its candle-power were 113·0 and, if this value is used instead of 114·0, we get a still closer agreement.

∴ Average candle-power of 422-c.p. lamp
$= (1\,169 \div 619)^2 \times 113{\cdot}0$
$= 403$

(4) *A squirrel-cage metal-filament vacuum lamp with close-fitting shade. (Axis of lamp horizontal and lamp facing the back of the cube.)*—The summations are made for the same lamp as that used in the last section. In this position the back of the cube and the halves of 13; 2, 6, 10, 14; 3, etc., and the products $\beta\Phi_{\theta'}$ are taken $\beta_1\Phi_{\theta_1}$, $\beta_5\Phi_{\theta_2}$, $\beta_9\Phi_{\theta_3}$, etc.

$$\therefore \Sigma(F\alpha) = 128\sum\left\{\frac{\alpha\Phi_{\theta'}}{(l^2+x^2+y^2)^{3/2}}\right\} + 256\sum\left\{\frac{\beta\Phi_{\theta'}}{(l^2+x^2+y^2)^{3/2}}\right\}$$
$$= 39\,800.$$

On comparing this with the summation for the point source, it is seen that the cube has the effect of making the apparent candle-power 397 instead of 422. This is about 6 per cent less than its known value. This

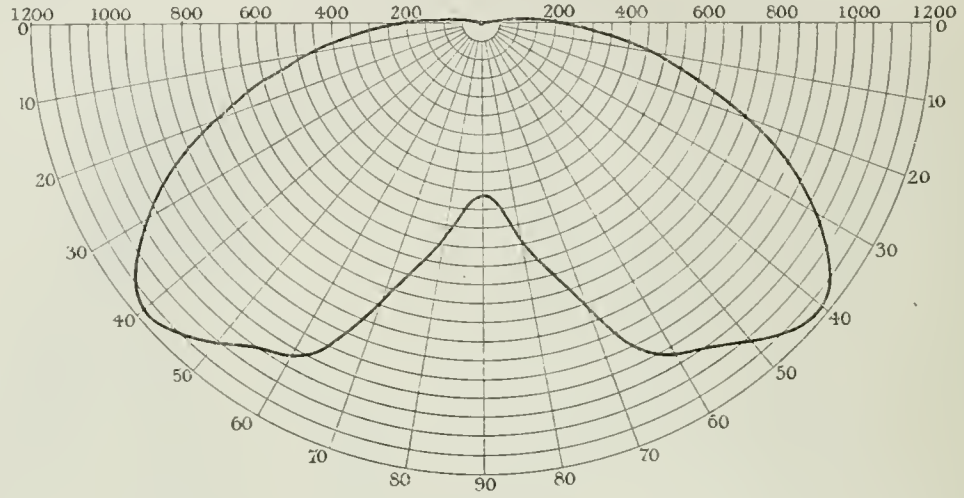


Fig. 8.—Polar curve of 1 000-watt lamp (Fig. 7).

the adjoining walls alone receive direct light. The polar curve of this lamp (Fig. 8) as thus situated in the cube is very similar to that of an arc, so that the results obtained in this case apply approximately to the case of the arc.

$\Sigma(F\alpha)$ consists of two portions (the C and D walls receive no direct light at all).

$$(1)\ \text{back} = 4\frac{l^3}{4^2}\sum\left\{\frac{\alpha\Phi_{\theta''}}{(l^2+x^2+y^2)^{3/2}}\right\}$$

θ'' taking the place of θ' because the back of the cube is now similarly situated with respect to the lamp as the bottom was previously.

(2) sides, top and bottom (the halves adjoining the back)
$$= 8\cdot\frac{l^3}{4^2}\sum\left\{\frac{\beta\Phi_{\theta'}}{(l^2+x^2+y^2)^{3/2}}\right\}$$

It should be noted in performing these summations that, since the axis of symmetry of the lamp is now horizontal, the numbering of the small squares from 1 to 16 does not correspond in the cases of the values of β and those of $\Phi_{\theta'}$. If the order 1 . . . 16 is maintained for the latter, then the order for β is 1, 5, 9, result was tested experimentally. The means of five observations by each of two observers gave

(1) for the 114·0-c.p. lamp, 1 173 and 1 175 mm. .. .. .. Mean = 1 174
(2) for the 422-c.p. lamp, 623 and 625 mm... .. .. .. Mean = 624

∴ Average candle-power of 422-c.p. lamp
$= (1\,174 \div 624)^2 \times 114{\cdot}0$
$= 403$

If, however, use is made of the result of section (2) which shows that the 114·0-c.p. lamp behaves in the cube as if it had an average candle-power of 113·0, this result is reduced to

$$(1\,174 \div 624)^2 \times 113{\cdot}0 = 400$$

This again is very close to the result indicated by theory, so that it can be assumed that a comparison of theory and practice for other positions at the middle of the cube will be accompanied by similar correspondence.

The preceding results and summations have applied only to lamps situated at the middle of the cube. It is important to find whether the same agreement will occur for an eccentric position. The position chosen

was half-way between the top of the cube and the middle. It should be remembered, however, that the conditions in this eccentric position are not quite the same as for the central position, as the lamps used in the cube exert some disturbing influence on the "contribution coefficient." This can be seen in Fig. 4, where the big dip in curve *a* is due to the action of the projector in the middle of the cube, and also to the screen. With positions away from the centre it is to be anticipated that this drop in the contribution coefficient will take place roughly at the place where the line through the window and the lamp meets the cube again, and it is probable that the effect on the summations is appreciable in certain cases.

(5) *A point source. (Eccentric position.)*

$$\Sigma(Fa) = 100{\cdot}7$$

(6) *A squirrel-cage metal-filament vacuum lamp. (Eccentric position.)*

$$\Sigma(Fa) = 11\,360$$

On comparing this with the result for a unit point source it is seen that the apparent average candle-power has become $11\,360 \div 100{\cdot}7 = 112{\cdot}8$.

(7) *A squirrel-cage metal-filament vacuum lamp with close-fitting shade. (Eccentric position.)*

$$\Sigma(Fa) = 41\,280$$

On comparison with the result for a unit point source at the same position the apparent average candle-power becomes 410. On testing this experimentally, photometric balance was obtained at distances of

(1) for the 114·0-c.p. lamp, 1 165 and 1 158 mm. .. .. Mean = 1 161·5
(2) for the 422 c.p.-lamp, 610 and 606 mm. .. .. .. Mean = 608·5

∴ Average candle-power of 422-c.p. lamp
$= (1\,161{\cdot}5 \div 608)^2 \times 114{\cdot}0$
$= 417$

On comparing with the result 112·8 as the apparent candle-power of the 114·0-c.p. lamp given by the summation in section (6), we get

$$(1\,161{\cdot}5 \div 608)^2 \times 112{\cdot}8 = 413$$

and this again is in very close agreement with theory.

Conclusions.

These results make it possible for corrections to be applied to the determinations of average candle-power made in the cube in the usual way, i.e. by comparison of the illuminations produced on the window, by the lamp under test, and by a comparison lamp of known intensity. For, instead of the real candle-power of the comparison lamp, its apparent value in the cube is used, and this is given by the ratio of $\Sigma(Fa)$ for this lamp to the corresponding summation for a unit point source at the same place. The result after the application of the above is then multiplied by the ratio

$$\frac{\text{real candle-power}}{\text{apparent candle-power in the cube}}$$

where this ratio has been calculated for the type of polar distribution exhibited by the test lamp. Thus, if x and y are the distances on the photometer bench for photometric balance with the cube comparison lamp and test lamps respectively, P is the average candle-power of the cube comparison lamp, and Q that of *any* lamp having a similar polar distribution to the test lamp, then the average candle-power of the test lamp

$$= \frac{y^2}{x^2} \times \frac{\Sigma(Fa)_P}{\Sigma(Fa)_1} \times \frac{\Sigma(Fa)_1}{\Sigma(Fa)_Q} \times Q$$
$$= \frac{y^2}{x^2} \times \frac{\Sigma(Fa)_P}{\Sigma(Fa)_Q} \times Q$$

where the suffixes indicate for which lamp the summation is to be made.

If the polar distributions of the test lamp and comparison lamps are the same, then

$$\frac{\Sigma(Fa)_P}{\Sigma(Fa)_Q} = \frac{P}{Q}$$

and the average candle-power of the test lamp

$$= \frac{y^2}{x^2} P$$

This shows that the cube is quite accurate in comparing light sources of similar distribution.

An application of this theory can be made to the measurement of the total flux in a beam. It is obvious that if a narrow beam of light having a constant flux is incident on the walls of the cube, there will be different illuminations on the window which will vary directly as the contribution coefficient of the point of incidence. Now the greatest and least values of the contribution coefficient are proportional to 9·57 and 6·71 respectively, while the average for the whole cube, when used with a point source, is 7·96. If now the beam is incident at a point where the actual value is equal to the average, we may compare the fluxes in the beam and cube comparison lamp by taking 4π times the ratio of the inverse squares of the distances from the bench comparison lamp, multiplied by the ratio

$$\frac{\Sigma(Fa)_P}{\Sigma(Fa)_1}$$

Fig. 4 shows that the contribution coefficient varies considerably on the back of the cube but is fairly constant on the sides. Fig. 9 shows the same thing in terms of contours. Large portions have a value not far from 8·20, so that a convenient spot on one of these walls can be taken and the result multiplied by $796 \div 820 = 0{\cdot}97$.

Throughout this investigation the effects of foreign bodies in the cube have been assumed to be small, as they always had an area very small compared with the total area of the inside of the cube. It is proposed

in a later paper to deal with this matter and also the effects of ventilating openings (such as have to be used in arc tests) and the non-uniformity of the cube as regards the whiteness of its interior.

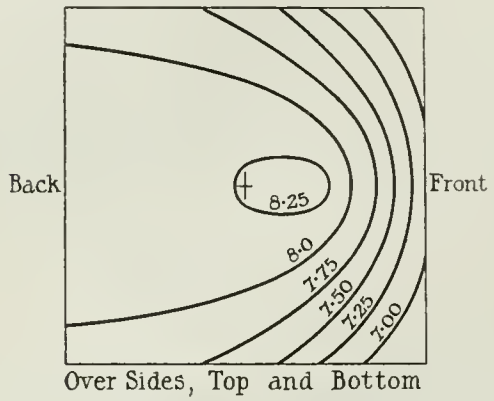


FIG. 9.—Variation of "contribution coefficient."

SYNOPSIS OF RESULTS.

(1) The variation in the contribution coefficient over the walls of a 2-metre cube has been determined.
(2) The apparent candle-powers of various light-distributions in the cube have been calculated and found to agree very closely with experiment.
(3) The cube has been shown to be quite accurate with sources having similar distributions.
(4) It has been shown how true average candle-powers may be deduced from the comparison of sources of different polar distribution, thus making the cube an instrument of precision.
(5) An application of the cube to the measurement of the total flux in beams has been indicated.

In conclusion I wish to express my indebtedness to Messrs. J. W. T. Walsh and H. C. Sturgeon of the National Physical Laboratory, the former for suggesting this work and for his interest in it, and the latter for invaluable assistance in taking the photometric readings and in the somewhat tedious task of performing the summations.

APPENDIX.

The complete calculations for $\Sigma(Fa)$ in section (6) are given here to show the method employed. In this case the lamp (Osram, Ref. No. 2, 114·0 candle-power) is midway between the middle of the cube and the top. The projection of the centre of the lamp

TABLE 5.

Area	$[l^2+x^2+y^2]^{3/2}$	Area	$[(3l/2)^2+x^2+y^2]^{3/2}$	$[(l/2)^2+x^2+y^2]^{3/2}$
A_{17}	146	A_1	1 767	76
A_{18}	154	A_2	1 911	133
A_{19}	170	A_3	2 217	272
A_{20}	194	A_4	2 703	535
A_{21}	186	A_5	1 911	133
A_{22}	194	A_6	2 063	200
A_{23}	210	A_7	2 374	353
A_{24}	234	A_8	2 869	636
		A_9	1 217	272
Values for areas		A_{10}	2 374	353
$A_1 \ldots A_{16}$ are		A_{11}	2 703	535
given in Table 3		A_{12}	3 216	855
		A_{13}	2 703	535
		A_{14}	2 869	636
		A_{15}	3 216	855
		A_{16}	3 767	1 218

TABLE 6.

Values of θ.

Area	$\theta' = \text{arc sin}\ \frac{y}{\sqrt{(l^2+x^2+y^2)}}$	Area	$\theta'' = \text{arc sin}\ \frac{3l/2}{\sqrt{[(3l/2)^2+x^2+y^2}}$	$\theta''' = \text{arc sin}\ \frac{\frac{1}{2}l}{\sqrt{[(\frac{1}{2}l)^2+x^2+y^2]}}$
A_{17}	48°	A_1	83°	71°
A_{18}	47°	A_2	79°	51°
A_{19}	44°	A_3	67°	38°
A_{20}	40°	A_4	60°	30°
A_{21}	54°	A_5	75°	51°
A_{22}	52°	A_6	70°	43°
A_{23}	49°	A_7	64°	34°
A_{24}	46°	A_8	58°	28°
		A_9	67°	38°
Values for areas $A_1 \ldots A_{16}$		A_{10}	64°	34°
are given in Table 4		A_{11}	60°	30°
		A_{12}	54°	25°
		A_{13}	60°	30°
		A_{14}	58°	28°
		A_{15}	54°	25°
		A_{16}	50°	22°

now comes at the common point of the four squares A_1 in Fig. 10, which also shows the new system of numbering squares that becomes necessary to avoid possible confusion on this account. A horizontal plane through the centre of the lamp cuts the wall at the double line, and it is about this line that symmetry occurs in the values of θ'. As regards the contribution factor, that is still symmetrical about a horizontal line through the centre of the wall. Thus the values of the contribution factor over the squares $A_1 \ldots A_8$ below the double line are the same as those over the former squares A_5, A_6, A_7, A_8; A_1, A_2, A_3, A_4, those over the squares $A_9 \ldots A_{24}$ are the same as over the former $A_1 \ldots A_{16}$, while those from $A_1 \ldots A_8$ above the double line are the same as over the former $A_9 \ldots A_{16}$. Similar considerations apply to all the vertical walls.

A_8	A_7	A_6	A_5	A_5	A_6	A_7	A_8
A_4	A_3	A_2	A_1	A_1	A_2	A_3	A_4
A_4	A_3	A_2	A_1	A_1	A_2	A_3	A_4
A_8	A_7	A_6	A_5	A_5	A_6	A_7	A_8
A_{12}	A_{11}	A_{10}	A_9	A_9	A_{10}	A_{11}	A_{12}
A_{16}	A_{15}	A_{14}	A_{13}	A_{13}	A_{14}	A_{15}	A_{16}
A_{20}	A_{19}	A_{18}	A_{17}	A_{17}	A_{18}	A_{19}	A_{20}
A_{24}	A_{23}	A_{22}	A_{21}	A_{21}	A_{22}	A_{23}	A_{24}

FIG. 10.—Division of sides of cube for summation described in appendix.

Now in the expression $\Sigma(Fa)$, F, the flux on an area, is given by

$$\frac{\text{area} \times \cos(\text{angle of incidence})}{(\text{distance from source to centre of area})^2} \times (\text{c.p. in direction of centre of area})$$

$$= \frac{l^2}{4} \times \frac{l_1}{\sqrt{(l_1^2 + x^2 + y^2)}} \times \frac{1}{(l_1^2 + x^2 + y^2)} \times \Phi_\theta$$

where l_1 = perpendicular distance of source from a wall of the cube. In sections (1) to (4), $l_1 = l$, but now, if we consider the vertical walls, $l_1 = l$, the bottom $l_1 = 3l/2$ and the top $l_1 = \frac{1}{2}l$. The expressions for θ' and θ'' also involve l_1 thus :—

$$\theta' = \text{arc sin} \frac{y}{\sqrt{(l_1^2 + x^2 + y^2)}}$$

$$\theta'' = \text{arc sin} \frac{l_1}{\sqrt{(l_1^2 + x^2 + y^2)}}$$

where $l_1 = l$, $3l/2$ and $\frac{1}{2}l$ for different walls. This will result in modifications to Tables 3 and 4. These are shown in Table 5 and 6 respectively.

The summation $\Sigma(Fa)$ consists of four parts

(1) sides, back and front, below double line,

$$= 2\frac{l^2}{4^2} \cdot l \sum_1^{24} \left\{ \frac{(\alpha + \beta + \gamma + \delta)\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

(2) sides, back and front, above double line,

$$= 2\frac{l^2}{4^2} \cdot l \sum_1^{8} \left\{ \frac{(\alpha + \beta + \gamma + \delta)\Phi_{\theta'}}{(l^2 + x^2 + y^2)^{3/2}} \right\}$$

(3) bottom

$$= 2\frac{l^2}{4^2} \cdot \frac{3l}{2} \sum_1^{16} \left\{ \frac{(\beta + \gamma)\Phi_{\theta''}}{\{(3l/2)^2 + x^2 + y^2\}^{3/2}} \right\}$$

(4) top

$$= 2\frac{l^2}{4^2} \cdot \frac{l}{2} \sum_1^{16} \left\{ \frac{(\beta + \gamma)\Phi_{\theta'''}}{\{(\frac{1}{2}l)^2 + x^2 + y^2\}^{3/2}} \right\}$$

$$\therefore \Sigma(Fa) = 64 \sum_1^{24} \left\{ \frac{(\alpha + \beta + \gamma + \delta)\Phi_{\theta'}}{(8^2 + x^2 + y^2)^{3/2}} \right\} + 64 \sum_1^{8} \left\{ \frac{(\alpha + \beta + \gamma + \delta)\Phi_{\theta'}}{(8^2 + x^2 + y^2)^{3/2}} \right\} + 96 \sum_1^{16} \left\{ \frac{(\beta + \gamma)\Phi_{\theta''}}{(12^2 + x^2 + y^2)^{3/2}} \right\} + 32 \sum_1^{16} \left\{ \frac{(\beta + \gamma)\Phi_{\theta'''}}{(4^2 + x^2 + y^2)^{3/2}} \right\}$$

$$(1) = 6\,400 \left(\frac{33 \cdot 02}{536} \times 1 \cdot 37 + \frac{32 \cdot 92}{636} \times 1 \cdot 37 + \frac{31 \cdot 64}{853} \times 1 \cdot 37 + \frac{30 \cdot 46}{1\,218} \times 1 \cdot 37 + \frac{31 \cdot 90}{637} \times 1 \cdot 31 + \frac{33 \cdot 39}{742} \times 1 \cdot 31 + \frac{32 \cdot 20}{969} \times 1 \cdot 32 + \frac{30 \cdot 72}{1\,346} \times 1 \cdot 34 + \frac{31 \cdot 90}{854} \times 1 \cdot 20 + \frac{33 \cdot 39}{969} \times 1 \cdot 22 + \frac{32 \cdot 20}{1\,218} \times 1 \cdot 24 + \frac{30 \cdot 72}{1\,621} \times 1 \cdot 27 + \frac{33 \cdot 02}{1\,218} \times 1 \cdot 10 + \frac{32 \cdot 92}{1\,346} \times 1 \cdot 11 + \frac{31 \cdot 64}{1\,621} \times 1 \cdot 15 + \frac{30 \cdot 46}{2\,062} \times 1 \cdot 19 + \frac{32 \cdot 27}{1\,767} \times 1 \cdot 01 + \frac{31 \cdot 93}{1\,911} \times 1 \cdot 03 + \frac{30 \cdot 62}{2\,217} \times 1 \cdot 06 + \frac{29 \cdot 92}{2\,703} \times 1 \cdot 11 + \frac{31 \cdot 64}{2\,538} \times 0 \cdot 93 + \frac{31 \cdot 06}{2\,703} \times 0 \cdot 96 + \frac{29 \cdot 72}{3\,042} \times 1 \cdot 00 + \frac{29 \cdot 25}{3\,582} \times 1 \cdot 04 \right)$$

$$= 6\,400(0 \cdot 7861) = 5\,030$$

$$(2) = 6\,400 \left(\frac{32 \cdot 27}{536} \times 1 \cdot 37 + \frac{31 \cdot 93}{636} \times 1 \cdot 37 + \frac{30 \cdot 62}{853} \times 1 \cdot 37 + \frac{29 \cdot 92}{1\,218} \times 1 \cdot 37 + \frac{31 \cdot 64}{637} \times 1 \cdot 31 + \frac{31 \cdot 06}{742} \times 1 \cdot 31 + \frac{29 \cdot 72}{969} \times 1 \cdot 32 + \frac{29 \cdot 25}{1\,346} \times 1 \cdot 34 \right)$$

$$= 6\,400 \times (0 \cdot 4239) = 2\,710$$

$$(3) = 9\ 600\left(\frac{16\cdot48}{1\ 767}\times 0\cdot22 + \frac{16\cdot45}{1\ 911}\times 0\cdot33 + \frac{16\cdot26}{2\ 217}\times 0\cdot68 + \frac{15\cdot56}{2\ 703}\times 0\cdot84 + \frac{16\cdot50}{1\ 911}\times 0\cdot45 + \frac{16\cdot20}{2\ 063}\times 0\cdot60 + \frac{15\cdot98}{2\ 374}\times 0\cdot75 + \frac{15\cdot52}{2\ 869}\times 0\cdot87 + \frac{15\cdot91}{2\ 217}\times 0\cdot68 + \frac{15\cdot77}{2\ 374}\times 0\cdot75 + \frac{15\cdot36}{2\ 703}\times 0\cdot84 + \frac{15\cdot20}{3\ 216}\times 0\cdot93 + \frac{15\cdot71}{2\ 703}\times 0\cdot85 + \frac{15\cdot44}{2\ 869}\times 0\cdot88 + \frac{15\cdot04}{3\ 216}\times 0\cdot93 + \frac{14\cdot78}{3\ 767}\times 0\cdot99\right)$$

$$= 9\ 600 \times 0\cdot0701 = 673$$

$$(4) = 3\ 200\left(\frac{16\cdot48}{76}\times 0\cdot58 + \frac{16\cdot45}{133}\times 0\cdot97 + \frac{16\cdot26}{272}\times 1\cdot13 + \frac{15\cdot56}{535}\times 1\cdot22 + \frac{16\cdot50}{133}\times 0\cdot97 + \frac{16\cdot20}{200}\times 1\cdot08 + \frac{15\cdot98}{353}\times 1\cdot18 + \frac{15\cdot52}{636}\times 1\cdot24 + \frac{15\cdot91}{272}\times 1\cdot14 + \frac{15\cdot77}{353}\times 1\cdot18 + \frac{15\cdot36}{535}\times 1\cdot22 + \frac{15\cdot20}{855}\times 1\cdot27 + \frac{15\cdot71}{535}\times 1\cdot22 + \frac{15\cdot44}{636}\times 1\cdot24 + \frac{15\cdot04}{855}\times 1\cdot27 + \frac{14\cdot78}{1\ 218}\times 1\cdot29\right)$$

$$= 3\ 200 \times 0\cdot9213 = 2\ 945$$

$$\therefore \Sigma(F\alpha) = (1) + (2) + (3) + (4)$$
$$= 11\ 358$$

DISCUSSION ON

"MULTIPLE-UNIT SHUNTS FOR THE MEASUREMENT OF VERY HEAVY CURRENTS." *

Mr. A. E. Moore (*communicated*): I think there is much to be said for the author's discrimination between the "constant" and the "resistance" of a shunt. The constant is the important quantity, being the potential difference on the instrument or potential terminals per ampere through the shunt via the main connections. The author rightly draws attention to the fact that in the cases of all large shunts the resistance measured between the potential terminals is not the same as the constant or K value. Also, the resistance measured between the potential terminals is practically a constant, whereas the K value may vary very considerably in some shunts, according to the manner of making the main connections. Anyone who has had considerable experience in the accurate testing of ammeters which are provided with heavy-current shunts will appreciate the advantages of the author's multiple-unit shunt combination. The errors which are introduced by variations in the main-current connections to heavy-current shunts are, however, often lost sight of by the users. A quite common form of shunt for currents of from 1 000 to 10 000 amperes is shown in Fig. A. In this design two objects are aimed at: (1) To provide a large contact surface for the main connections in order to prevent undue heating, and (2) to effect a more or less uniform entry of the current into the shunt blocks. If every precaution were taken in the using and the testing of such shunts, better results than usual might be obtained. My experience, however, is that such shunts are very much misused. I recently had to test an ammeter provided with such a shunt for 1 500 amperes. The slots in the end blocks of the shunt had been partly filled in with strips of iron, so that connections to the main current leads had to be made on the faces AA and BB. For the purpose of obtaining some data for this contribution, I made tests with the main connections: (*a*), on the faces AA, and (*b*), on the faces BB. The results are given in the following table:

Reading on Shunted Ammeter	Main Amperes, AA	Percentage Error	Main Amperes, BB	Percentage Error
0	0	—	0	—
300	267	12·3 % high	300	nil
600	538·5	11·4 % high	606	1·0 % low
900	804	11·9 % high	902	0·2 % low
1 200	1 075	11·6 % high	1 202	0·2 % low
1 500	1 340	11·9 % high	1 498·5	0·1 % low

It may be urged that the shunt is not designed to have connections made in this way. I agree, but nevertheless connections are continually made in this manner, and it is hardly to be expected that every person who has to use a heavy-current shunt is fully aware of the large errors which may be introduced by improper connections. It is probable that, even when erected on the switchboard, all the slots are not filled with busbars, and whether or not the instrument attached to the shunt indicates correctly depends

* Paper by Mr. M. B. Field (see vol. 58, page 661).

very largely on how near the main connections made in the test room agree with those subsequently made on the switchboard. In designing shunts generally, the idea seems to be that they must be short and compact, and it is this shortness which is largely responsible for the inaccuracies introduced. Some time ago some 8 000-ampere shunts of the type shown in Fig. A, together with the millivoltmeters, were sent to me for verification, and very large variations in the results could be obtained by changing the positions of the incoming leads. To get over this difficulty, copper straps about 2 ft. long were fitted to both ends of the shunts as shown in Fig. B, and the ends of the straps at A were soldered together. The straps formed part of the shunt, in that they were not afterwards removed. It was then found that, whatever the manner of making the main connections to A, the constant of the shunt remained unaffected. It is more than probable, however, that if the three copper straps were changed into three different slots, the constant of shunt would be different from that obtained

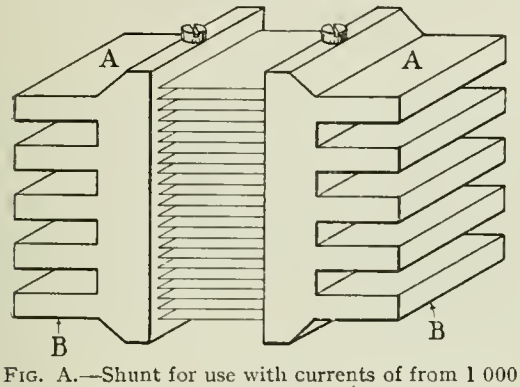


FIG. A.—Shunt for use with currents of from 1 000 to 10 000 amperes.

by the first arrangement. Such shunts are quite frequently used in important tests, and are afterwards, in some cases, sent for verification, no information being given as to how the leading-in connections had been arranged. The users seem to be of the opinion that, so long as the connections are good enough to prevent undue heating, that is all that matters, and, incidentally, that is all that should matter. Shunts which are designed for testing purposes should have long leading-in connections, and preferably these should form part of the shunt blocks so that they cannot be detached. The author's design, shown in Fig. 4, page 663 (vol. 58), seems well adapted to withstand portable work, and should prove very satisfactory for testing purposes, especially if the leading-in straps are reasonably long. The use of high-conductivity copper also should prove very much superior to the comparatively low-conductivity cast metal so frequently employed. The author gives it as his opinion that 6 000 amperes is the limit for a satisfactory shunt. If we take the form just referred to, viz. that with long leading-in connections, I think that the current might be rather higher. But for the form shown in Fig. A, with short cast-metal end blocks, I should say that the maximum current-range is much lower than 6 000 amperes, if errors not exceeding about 1 per cent are to be avoided.

A question arises as to whether the system may be used in the construction of a heavy-current shunt combination for precision measurements. If the shunts and the equalizer resistances are adjusted to the accuracy required, the theory shows that the system is applicable. But obviously, if an accuracy of one part in 10 000 is aimed at, it would be inconvenient to be dependent on all the equalizers having been adjusted to, and retaining a somewhat higher accuracy than this. But if the shunts are adjusted to the highest possible accuracy, and if the current distribution among them is quite uniform, then, provided a potentiometer is used as the measuring instrument, it is immaterial whether or not the resistances of the equalizers are the same. From this consideration it would appear that, if the equality of the current distribution is good, then the accuracy with which the P.D. on the star points represents the average P.D. on the shunts is greater than the accuracy of adjustment of the equalizers. I have not attempted any general deter-

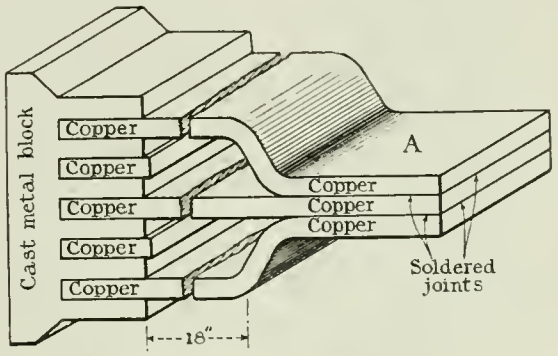


FIG. B.—Method of overcoming large variations in reading.

mination of the errors introduced by inaccuracies in the adjustment of the equalizer resistances, but, from a few calculations in specific cases, it appears that if the shunts are adjusted to the highest possible accuracy, and the equalizers are of high resistance and adjusted to, say, one part in 1 000, then, if equality of current distribution is maintained among the shunts, the P.D. on the star points is the average P.D. on the shunts without error. If there be, say, three shunts, and the total current be I amperes, then with equality of current distribution the current in each shunt will be $\frac{1}{3}I$ amperes. If the actual currents in the shunts differ from $\frac{1}{3}I$ amperes by a maximum of x per cent (the total current being I amperes) the difference between the average P.D. on the shunts and the P.D. on the star points due to errors not exceeding 0·1 per cent in the resistances of the equalizers is approximately x per cent of 0·1 per cent, provided that a potentiometer is used as the measuring instrument. Thus, if $x = 10$, the error introduced is approximately 0·01 per cent, and if $x = 1$, the error should be of the order of 0·001 per cent. For standard work it is a simple matter to measure the equality of current distribution among the shunts by measuring the drop on each of the shunts, and to improve the distribution

if necessary. It might be possible to solder each of the shunts to a common pair of busbars so as to effect an even distribution of current among them, but this would have the disadvantage that the equality of resistance adjustment of the individual shunts could not be verified from time to time without unsoldering from the busbars.

Another important consideration is the best position from which to take the connections to the millivoltmeter. In the ideal shunt the end blocks would be of material having a resistance quite negligible compared with the resistance of the strips, and in that case there would be no measurable P.D. over either of those faces of the blocks into which the strips enter. Hence connections might be taken from any points on those faces always with the same results. But in large shunts the end blocks have not a negligibly small resistance, and, as has been indicated, considerable P.D. may exist between any two points on the face of the same block, and is dependent on the main connections. For a single pair of points I suggest that the best results would be obtained by taking the tappings from the centres of the faces of the end blocks as shown in Fig. C. These tappings could be formed of stout rods screwed and brazed into the blocks before the assembly of the strips. If the rods are of the same material as the strips they would tend to eliminate errors due to the thermoelectric forces. If each of the resistance strips had identically the same resistance between the blocks, it would be possible to make use of the author's principle in a single large shunt by taking a number of points on the faces of the blocks and joining them by equal-resistance equalizers to two star-points. But, unfortunately, it is not possible to measure the resistance of individual strips after they have been soldered into the blocks. In Fig. 5, page 663 (vol. 58) the author shows how it would be possible to make up a single large shunt by making up a number of small, equal shunts, and soldering them into two common end-blocks, but it is probably better to have the more flexible arrangement of individual shunts bolted to common busbars. In a well-designed shunt it is possible that the resistances of individual strips are fairly equal, and it might well

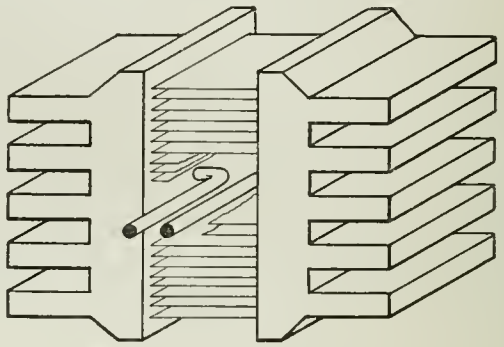


FIG. C.—Method of arranging tappings for a single pair of points.

be that to take a number of tappings from the faces of the end blocks through equal-resistance equalizers to star points would give better results than are obtained by using a single pair of potential tappings.

I am at present engaged in testing a 10 000-ampere multiple-unit shunt combination of the author's design manufactured by Messrs. Kelvin, Bottomley & Baird, Ltd., of Glasgow, and hope to be in a position to publish a description of this gear and the results of the tests at an early date.

PROCEEDINGS OF THE INSTITUTION.

654TH ORDINARY MEETING, 25 NOVEMBER, 1920.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6p.m.

The minutes of the Ordinary Meeting of the 18th November, 1920, were taken as read, and were confirmed and signed.

Messrs. H. Green and A. G. Collis were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows :—

ELECTIONS.

Members.

Campbell, Donald Fraser, B.A.
Conforti, Gennaro.
Press, Abraham.
Schuchardt, Rudolph Frederick.
Taylor, Horace Morton.
Tweedy, Robert Naudin.

Associate Members.

Billington, John Robert.
Bourke, William George.
Bowles, Richard Henry.
Brumwell, William.
Davies, Daniel Obadiah S., B.Sc.
Evans, Arthur, Lt.-Col., O.B.E., M.C., R.E.
Finlayson, Hugh Campbell.
Foster, Frederick Milburn.
Fourniss, Charles Walter.
Gifford, Walter Stanley.
Glover, Francis Harry.
Goodman, Cyril William.
Grigor, Robert.
Gumersall, Gerald Joseph, B.Sc. (Eng.).
Hale, Clarence Percival.
Hardie, Herbert John.
Harris, Thomas Howard.
Higham, Joseph, B.Sc.
Hiscock, Alfred George.
Hubbard, Charles Robert.
Hulatt, Henry.
Jowett, Charles Rhodes.
Macdonald, William Malcolm B., B.Sc.
Machen, Henry Arthur I.
Mandeno, Lloyd.
Martin, Harold.
Massie, William Charles.
Medlam, William Beresford, B.Sc. (Eng.).
Mellor, Herbert Edward, M.Sc.
Myers, Leonard Vivian.
Orr, Albert J.
Penman, John.
Perrow, Ewart Vivian.
Platt, Wilfred.
Plumbe, Charles Conway, B.Sc. (Eng.).
Porteous, Dundas Alexander S.
Robertson, George Archibald.
Rodwell, Arthur.
Rogers, Thomas.
Sanders, Harold Curtice.
Saunders, Harold Leonard, B.Sc. (Eng.).
Scarth, William McClachlin.
Smith, Frank.
Smith, Percival Harry F.
Snape, Sidney.
Stocken, Edgar Clarence.
Sutton, George.
Tewsley, Eric Neill.
Turner, Tom Ellery.
Walker, Frederick Henry.
Whittaker-Swinton, John Henry, Capt. R.E.
Wolland, George William L.

Graduates.

Bedford, Howard Edwin.
Billington, Edwin Gordon.
Briant, Frederick Gordon.
Carter, William.
Charlton, Charles Cedric.
Cook, Sydney Parker.
Corder, George Falconer.
Crowther, Arthur.
Dalton, Alfred John
Darling, John William.
Darlington, Walter Edward.
Dickson, Alexander Gillespie, Lieut. R.E.
Drewry, John Percy A.
Eclair-Heath, Stanley.
Ensor, John Leonard.
Foskett, Norman Frederick.
Hollick, Herbert Charles.
Kingston-McCloughry, Edgar James, D.S.O., D.F.C.
Lacey, Thomas Alfred.
Linay, Edward Curzon.
Lo, Sek Tean, B.Sc.
Padley, Alexander Charles.
Purday, Allen Charlton.
Ross, Daniel.
Terry, Thomas.
Thomas, William George.
Underdown, Albert Edward.
Warwick, Henry William.
Wells, Harold Charles.
Williams, William Almond.

Students.

Adcock, Fred., B.Sc.
Agate, Purushottam Narayan, B.Sc.
Allen, Robert.
Anderson, Robert Cairns A., B.Sc.
Arklay, Fred. Hill.
Ashton, Arthur Leigh.
Aspinal, Leslie Haynes.
Bagnall, Edward Alderson H.
Bailey, Arthur Roland E.
Bailey, John Wallis.
Barker, Eric.

Students—continued.

Barnes, Reginald Wilfred.
Bawcutt, Dudley James.
Beale, Lionel Attwell.
Beard, Alfred Thomas J.
Beaton, Cecil Arthur.
Bell, Andrew Clark.
Bell, William.
Berry, Arthur William.
Bisdee, Colin Edward.
Bloore, Charles George.
Braendle, Ernest William.
Bray, William James.
Broderick, Joseph Aloyious.
Brough, Leonard George.
Brown, Wilfrid Lawrence.
Bruce, Robert.
Bryning, Henry Eardley W.
Burns, James Leo.
Busby, Arthur Henry W.
Bushell, John David.
Butler, William Richard.
Byles, Robert Bedford.
Cannon, Donald Victor.
Cargill, Donald Stuart.
Cave, Percival William.
Clifford-Jones, Edward Thomas.
Clifford-Jones, John Kenneth.
Colborn, Cecil Herbert.
Collinson, Percival Leigh, B.Sc.
Conly, William Peter.
Cooper, Cyril Hoare.
Corbin, Cecil.
Cork, Edward Cecil.
Cousins, William George.
Couzens, Dennis Frederick.
Cox, Stanley Edward C.
Cripps, Edmund James.
Davies, Evan David J.
Davies, Pendry George.
Davis, Donald Henry.
Davis, Horace Gilbert.
De Lattre, Maurice.
De Nordwall, Charles Herrd.
Duke, George Edward.
Dunn, Charles Trevor.
Dyer, Walter.
Eden, Arnold Christie.
Elford, Arthur Horatio S.
Evans, Evan William.
Evans, William John.
Feiron, Alec Patrick.
Fells, Aubrey.
Fleet, Rufus Daniel.
Foden, Arthur Moore.
Foster, Cedric Bradshaw.
Fowler, Charles Frederick.
Fowler, John.
Francis, Leslie Beeson.
Frome, Norman Frederick.
Furneaux, John Alan.
Gallizia, Enrico.
Gamble, Bertram Thomas S.
Gamlen, Robert Eric.
Gee, Donald William.
Gillman, Gustave Alexander R.
Glisby, James Edward.
Gloinson, William Henry.
Grainger, Ernest James, B.Sc. (Eng.).
Gurney, John Leslie.
Haag, Claud Leopold.
Handley, William Percy.
Hannen, Gabriel John.
Hardy, Alexander Edward.
Harmer, Ernest Walter.
Hayes, Kenneth Alan.
Hayton, Tom Burneside.
Heath, Robert.
Henderson, Francis Adrian.
Heritage, Hubert James E.
Higgs, George Robert.
Hill, Hubert George W.
Holland, William Reginald, B.Sc. (Eng.).
Huggins, Percy.
Hughes, Edmund Vyvyan M.
Hughes, Edward Llewelyn.
Iddon, Tom.
Jordan, Reginald John.
Kendall, Herbert Douglas.
Kent, Batt Rowland.
Lambert, Alexander Barker.
Lamerton, Henry.
Lardge, Howard Eric.
Lawton, Sidney.
Lee, Herbert Cleave.
Lloyd, William Foulkes.
McDermott, Harold Richard.
McDonald, Herbert.
Maiden, Reginald Stuart.
Mann, Francis John.
Manners, Arthur Frederick.
Marsh, John Rogerson.
Mason, Alfred Guy L.
Mather, Gilbert.
Matson, Arthur William.
Menzies, James Aitken B., M.C., B.Sc.
Merchant, Tribhovandas Bhugvandas.
Merry, Arnold Cecil, B.Eng.

Students—continued.

Millner, William.
Moore, Raymond Edward.
Moulder, Harrington Algernon E.
Munro, Henry Hawkins.
Murray-Russell, Harry.
Nash, Stephen Herbert.
Newman, Claude.
Nisbet, Charles.
Nye, Edward Philip.
Orr, Robert.
Pacheco, Olavo Irineu de C.
Parrott, Frank Gordon, B.Eng.
Parsonage, Geoffrey.
Patel, Kashibhai Bhikhabhai.
Pearce, Arthur George.
Phillips, Sidney.
Philpott, Stuart Fred.
Phipps, William Alfred.
Porteus, Thomas Alexander.
Powell, Hugh Morgan.
Price, Joshua Marshall.
Ray, Frederick Ivor.
Reeves, Richard William C.
Rendell, John.
Rhind, William Alexander.
Robinson, Percy.
Rostron, Harry Maxwell.
Sanderson, Edward Robson.
Sillar, Laurence Gilbert.
Simpson, Archibald LacyF.
Sims, Ronald John.
Sims, William Louis.
Smethurst, Charles Edwin.
Smethurst, Thomas.
Spinks, John Henry C.
Stansfield, Joshua Bertram.
Starling, Philip Percy.
Stearn, George Fletcher.
Stewart, Charles.
Stone, Donald Dangar.
Swift, Bernard, B.Eng.
Taggart, James Ewart.
Taylor, Louis Norwell.
Thomas, Cyril Marsingall.
Thomas, Harold Leslie.
Titman, Reginald Clarke, B.Eng.
Tobin, Edward.
Tolley, Lionel Lawrence.
Topley, Harry.
Tyler, Reginald James.
Tyler, Stuart.
Villiers, Algernon Edward.
Walker, Ernest William.
Wallace, Milo Harrison.
Wallace, Robert, B.Sc.
Wansbrough, Robert Cecil.
Ward, Edmund.
Warren, Thomas Reginald.
White, Edward P.
Wild, Robert William.
Williamson, Arthur James R.
Wrightson, Francis Baliol.
Wyatt, Charles Burnet.

TRANSFERS.

Associate Member to Member.

Bruty, Albert George.
Gauvain, William Percival.
Golding, Frederick.
Jewell, Charles James.
Murray, John Campbell.
Packham, William Bowman.
Pask, Thos. Percival.
Richardson, George William.
Rye, Arthur Newcombe.
Weaving, Reginald.
Windle, Sydney Robert.
Young, James, M.B.E.

Associate to Member.

Bradfield, William Walter, C.B.E.
Prentice, Napier.

Graduate to Associate Member.

Khan, Lutfe Ali.
Kirloskar, Vinayak Ganesh
Melville, Oliver.
Perrett, Arthur Harold.
Roberts, Thomas Croker.
Rowse, Edwin John.
Sarkar, Chandra Sekhar.
Storey, Warren.
Stubbs, Albert.

Student to Associate Member.

Barry, William Cyril.
Elliott, John William, B.Sc.
Grant, Lewis Colin.
Nott, Madeline (Miss).
Richards, Leslie Cameron, B.Sc. (Tech.).

Student to Graduate.

Bishop, Bernard Lees.
Charles, David Sibbering.
Deacon, Edwin Arthur.
Dobie, Percy.
Hindon, Herbert John.
Jackson, Harry Yule V., M.C.
Speed, Leslie Craven.
Wales, William Arvon.
Whittick, Robert Baker.

A paper by Mr. W. B. Woodhouse, Member, entitled "The Distribution of Electricity" (see page 85), and a paper by Mr. R. O. Kapp, B.Sc., Associate Member, entitled "Some Economic Aspects of E.H.T. Distribution by Underground Cables" (see page 94), were read and discussed and the meeting adjourned at 7.45 p.m.

655TH ORDINARY MEETING, 9 DECEMBER, 1920.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 25th November, 1920, were taken as read, and were confirmed and signed.

The adjourned discussion on the papers by Mr. W. B. Woodhouse (see page 85) and Mr. R. O. Kapp, B.Sc. (see page 94), was continued and the meeting terminated at 8 p.m.

RANGE OF WIRELESS STATIONS.

By Captain R. Chenevix Trench, Royal Corps of Signals.

(*Paper received* 16 *August, and read at a Meeting of the* Wireless Section *of* The Institution, 15 *December*, 1920.)

Summary.

The following paper investigates the main constants of wireless stations in relation to range.

From the theoretical radiation formula for a dipole (eq. 2), the practical radiation from an aerial is deduced (eq. 7). This is applied to the Austin-Cohen formula (eq. 1), and after considering the questions of radiation efficiency and the power required in the receiving aerial, the range formula (eq. 13) is arrived at.

In Section III the Austin-Cohen formula, reduced to ideal conditions of a perfectly conducting plane surface, is compared with the theoretical formula, and the apparent discrepancy discussed.

Range formula (13) applies only to the assumed conditions given in Section I. The modifying factors to be applied for varying conditions are considered in Section IV. A suggestion is made which, it is considered, provides a suitable means of allowing for losses over any kind of land. The effect of aerial capacity in receiving is discussed, in connection with the preceding Section III. Nature of earth connections and types of apparatus are among the other variables considered.

In Section V the conclusions arrived at are applied to the solution of practical problems with ranges varying from 4 to 4 000 km.

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SECTION I.

A paper by the author under the above heading appeared in the *Electrician* of 20th and 27th April and 4th May, 1917. Some helpful criticism followed in a letter to that journal from Professor Howe, to which the author is much indebted, but which he has been prevented by the claims of active service abroad from acknowledging until now. There are, moreover, several aspects of the subject that it is desired to reconsider, particularly the effect on transmission of the height of the sending aerial; and it is necessary to bring up to date the examples at the end by the inclusion of instances of continuous-wave valve transmission, in which such great advances have been made since the original article was published.

The subject is therefore reconsidered in the present paper, and it will be shown that the principal factors determining range, with any given type of apparatus and set of conditions, are :—

(1) Wave-length employed,
(2) Natural wave-length of sending aerial,
(3) Power of sending plant,
(4) Height of receiving aerial,

and these alone will be considered in the curves to be given. There are, however, other factors which affect the range profoundly, such as :—

(1) Nature of intervening surface,
(2) Nature of aerial,
(3) Nature of earth connections,
(4) Type of sending apparatus,
(5) Type of receiving apparatus,
(6) Time of day,
(7) Atmospheric conditions,
(8) Degree of reliability required.

With regard to the above, the following assumptions of fair normal conditions are first made for the sake of arriving at a concrete result. The effects of variations from these conditions will be considered later.

ASSUMPTIONS.

(1) *Surface.*—Transmission is taken to be over the sea.

(2) *Aerial.*—The aerial is considered to be of the flat top, non-directive type, of capacity sufficient to take the power employed without brush discharge.

(3) *Earth.*—The earth connection is assumed to be good and made according to recognized methods in favourable soil.

(4) *Sending apparatus.*—This is taken to be the modern musical spark apparatus. It is assumed that only one wave-length is radiated, due either to rapid quenching of the spark or to loose coupling between the aerial and the closed circuit.

(5) *Receiving apparatus.*—This is assumed to be good modern apparatus for telephone reception, without amplifiers or relays.

(6) *Time of day.*—Daylight sending is assumed.

(7) *Atmospheric conditions.*—Allowance is made for communication through considerable atmospheric interference, but not for abnormal conditions.

(8) *Degree of reliability.*—Throughout this paper, the range means the distance at which a regular traffic of several thousand words a day can be relied on, not the extreme distance at which one may expect with ordinary luck to get messages through. It is assumed that the power required in the receiving aerial is 64 times that which would give a just audible signal. This figure includes an allowance for atmospheric conditions, minor bad adjustments, interference, etc.; it might be described as a "reliability factor."

SECTION II.

(1) With these assumptions take the result arrived at by L. W. Austin in his experiments in sending from the Brant Rock to U.S. cruisers in the Atlantic in 1909–10. A large shore aerial was used in one series of tests at varying ranges to find the variation of the received current with variation in range. In another series of tests smaller aerials of varying heights were used to find the variation of received current with variation in height of the aerial. The wave-lengths were varied in both cases. The results arrived at were tested within the following limits. Ranges up to 1 850 km.; wave-lengths between 300 m. and 3 750 m.; heights between 30 ft. and 130 ft. They apply to daylight working with flat-top non-directive aerials over sea, and are embodied in the following formula, known as the Austin-Cohen formula *:—

$$I_2 = 4{\cdot}25\frac{I_1 h_1 h_2}{\lambda x}\epsilon^{-0{\cdot}0015x\sqrt{\lambda}} \quad . \quad . \quad . \quad (1)$$

where I_1 = sending current (R.M.S.) at foot of aerial in amperes,
I_2 = receiving current (R.M.S.) at foot of aerial, in amperes,
h_1 = height of sending aerial in kilometres,
h_2 = height of receiving aerial in kilometres,
λ = wave-length in kilometres,
x = range in kilometres.

Let P_1 = power developed in sending aerial in watts,
P_r = power radiated by sending aerial in watts,
R_1 = total resistance of sending aerial in ohms,
R_r = radiation resistance of sending aerial in ohms,*
R_e = resistance in ohms of sending aerial due to earth and all other sources except radiation, so that $R_1 = R_r + R_e$,
P_2 = power delivered to receiving aerial in watts,
R_2 = total resistance of receiving aerial in ohms,
λ_n = natural wave-length of sending aerial.

(2) Now, in a Hertzian oscillator it has been shown that

$$P_r = 80\pi^2\frac{l^2}{\lambda^2}a^2 = 790\frac{l^2}{\lambda^2}a^2 \quad . \quad . \quad (2)\dagger$$

where l is the total length from pole to pole, l and λ are in any like units, and a is the R.M.S. value of the current in amperes. This assumes that the current is the same at all points of the oscillator.

Again $P_r = a^2 R_r$

$$\therefore \quad R_r = 790\frac{l^2}{\lambda^2} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

(3) Any aerial may be considered to be the top half of a Hertzian oscillator of length l, where $\frac{1}{2}l$ is the height above ground at which the distributed capacity of the aerial may be considered (for our purposes) to act as a concentrated capacity. We will call $\frac{1}{2}l$ the effective height of the aerial. Formulæ (2) and (3) may be then applied to any aerial of which we know the effective height. We must remember, however, in considering an aerial as half a Hertzian oscillator, that only the top half of the oscillator radiates energy, so the values of P_r and R_r in equations (2) and (3) must be multiplied by 0·5. Also equation (2) is based on the assumption that a is uniform throughout the oscillator. In an aerial, on account of the variation of current amplitude in accordance with a sine law throughout its length, the R.M.S. current at the base is $\sqrt{2}$ times the R.M.S. value of the effective current throughout the aerial. Therefore, in applying equation (2) to an aerial, we must write $\frac{1}{2}I_1^2$ for a^2.

With these two modifying factors applied to equation (2) we have, in an aerial,

$$P_r = \tfrac{1}{2}\left(790\frac{l^2}{\lambda^2}\right)\frac{I_1^2}{2} = 197\frac{l^2}{\lambda^2}I_1^2 \quad . \quad (4)$$

and

$$R_r = 197\frac{l^2}{\lambda^2} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

* W. H. ECCLES: "Wireless Telegraphy and Telephooy," p. 150.

* The general practice is here followed of taking the radiation resistance R_r as that quantity whose product into the square of the R.M.S. current at the foot of the aerial gives the power radiated. In order to conform to this resistance rating, the "wasting" resistance R_e is here taken as that quantity whose product into the square of the R.M.S. current at the foot of the aerial gives the power dissipated through all other sources. The total resistance R_1 is considered from the same standpoint. We can say, therefore, that $P_1 = I_1^2 R_1$, without concerning ourselves with any allowance for variation of current amplitude throughout the length of an aerial.

† W. H. Eccles (*loc. cit.*, p. 126).

(4) In an unloaded vertical aerial the charge at maximum potential difference is distributed throughout the aerial in accordance with a sine law, being zero at the foot and maximum at the top. The point at which it can be considered to act as a concentrated capacity is $\left(\frac{2}{\pi}\right)h$, or $0{\cdot}64h$, which is therefore the effective height of such an aerial.* In a heavily-loaded vertical aerial this effective height is reduced to almost $0{\cdot}5h$. In either case, by adding a roof we increase the effective height, and for an aerial with a large roof area l closely approaches $2h$ in value. The matter may be treated with approximate accuracy as follows:—

Consider a single wire aerial of T or inverted L shape, in which T is the ratio of length of roof to height. Then the horizontal part can be divided into T elements, each equal in length to the vertical part. Call each element one unit of capacity, so that the horizontal part gives us T units of capacity acting at a height h. The vertical part gives us one unit of capacity which may be taken as acting at a height between $0{\cdot}64h$ and $0{\cdot}5h$, say $0{\cdot}6h$. Thus we have the total capacity acting at the centre of effect of T units at a height h, and one unit at a height $0{\cdot}6h$. That is

$$\tfrac{1}{2}l = h\left(0{\cdot}6 + 0{\cdot}4\,\frac{T}{T+1}\right) \quad . \quad . \quad . \quad (6)$$

This formula may be applied with sufficient accuracy to any T or inverted L aerial. For an umbrella aerial or any other with a large roof area we can take l as equal to $2h$.† For other forms an approximate estimate of the relative capacities of the aerial's component parts can be made, and the height of the centre of effect worked out.

The ship's aerial used in the Austin tests consisted of a network of wires 116 ft. by 40 ft., 130 ft. above water. The capacity of the vertical part would be about $0{\cdot}2$ times that of the roof, so we have approximately

$$\tfrac{1}{2}l = h\left(0{\cdot}6 + 0{\cdot}4\,\frac{1}{1{\cdot}2}\right), \quad \therefore\ l = 1{\cdot}86h$$

Assuming the land aerials to have about the same value for l, and substituting it in equation (4), we have

$$P_r = 197\left(\frac{1{\cdot}86h_1}{\lambda}\right)^2 I_1^2 = 680\frac{h_1^2}{\lambda^2}I_1^2 \quad . \quad . \quad (7)‡$$

or

$$\frac{h_1 I_1}{\lambda} = \frac{\sqrt{P_r}}{26}$$

Substituting this value of $h_1 I_1/\lambda$ in equation (1) we get

$$I_2 = \frac{4{\cdot}25}{26}\sqrt{P_r}\frac{h_2}{x}\epsilon^{-0{\cdot}0015x/\sqrt{\lambda}} = 0{\cdot}163\sqrt{P_r}\frac{h_2}{x}\epsilon^{-0{\cdot}0015x/\sqrt{\lambda}} \quad (8)$$

Now $P_2 = I_2^2 R_2$, and we are told that in the Austin experiments $R_2 = 25$ ohms

$$\therefore\ I_2 = \frac{\sqrt{P_2}}{5}$$

(5) Substituting this in equation (8), we get

$$\sqrt{P_2} = 0{\cdot}8\sqrt{P_r}\frac{h_2}{x}\epsilon^{-0{\cdot}0015x/\sqrt{\lambda}} \quad . \quad . \quad . \quad (9)$$

This equation gives the power received in terms of the power radiated for any range and wave-length. It shows that, for any value of P_r, the greater the value of λ the better. We must now investigate the relation between P_r and P_1.

(6) Of the power developed in the aerial, the proportion radiated is $R_r/(R_r + R_e)$; that is

$$P_r = P_1\frac{R_r}{R_r + R_e} = P_1\frac{R_r}{R_1} \quad . \quad . \quad . \quad (10)$$

It is stated that the radiation efficiency of an aerial with a good earth may reach 50 per cent,* and, as an aerial radiates most efficiently at about $1{\cdot}5$ times its natural wave-length, we may take the 50 per cent efficiency to apply to this wave-length. So that for an aerial with a good earth, at $1{\cdot}5\lambda_n$, we may say $R_r = R_e$ and $P_r = 0{\cdot}5P_1$. Also, since $R_r \propto \frac{l^2}{\lambda^2}$, any increase of λ above $1{\cdot}5\lambda_n$ results in a reduction of R_r and if the total resistance $R_r + R_e$ remained constant the proportion of power radiated would vary inversely as λ^2. In practice, however, the total resistance increases at the longer waves, so the radiation efficiency on these waves is rather less than it would be if it varied inversely as λ^2. An example is quoted in which an aerial at a wave-length rather greater that the natural radiated 46 per cent of the energy imparted to it; at about twice the natural wave-length, 14 per cent; and at about three times, 4 per cent. This great loss is partly compensated for by the smaller decrement and greater persistence of the wave trains on the longer waves, and it will be sufficiently accurate for a practical estimate to say that above $1{\cdot}5$ times the natural wave-length the effect of the power radiated varies inversely as λ^2.

At waves shorter than $1{\cdot}5$ times the natural, R_r increases, but R_1 also increases rapidly. The radiation efficiency R_r/R_1 is rather less at the natural wave-length than at $1{\cdot}5$ times that wave-length, but between these limits the difference is not great. So we have for wave-lengths above $1{\cdot}5\lambda_n$

$$P_r = 0{\cdot}5P_1\left(\frac{1{\cdot}5\lambda_n}{\lambda}\right)^2 = 1{\cdot}125P_1\left(\frac{\lambda_n}{\lambda}\right)^2 \quad . \quad (11)$$

Equation (11) applies to wave-lengths of $1{\cdot}5\lambda_n$ and upwards. As already explained, there is generally some decrease in efficiency below $1{\cdot}5\lambda_n$, but not much, whereas if equation (11) were applied to these wave-

* L. W. Austin.

* Thus in an unloaded vertical aerial we have $\frac{1}{2}l = 0{\cdot}64h$, $\therefore l = 1{\cdot}28h$; and applying this to equation (4) we get—

$$P_r = 197(1{\cdot}28h/\lambda)^2 I_1^2 = 320(h^2/\lambda^2)I_1^2$$

This agrees with the equation deduced theoretically from first principles for the radiation from a plain aerial, given in J. A. Fleming's "Principles of Electric Wave Telegraphy and Telephony," 2nd ed., p. 697, in the form $P_r = 160(h^2/\lambda^2)A^2$, where A is the maximum (not R.M.S.) value of the current at the foot of the aerial.

† In an umbrella aerial h is the height to the centre of the ribs.

‡ From equation (7) it follows that for any form of aerial on its natural wave-length, where h_1/λ_n is a constant depending on the shape of the aerial, we have $P_r \propto I_1^2$. That is, for any one form of aerial the power radiated on its natural wave depends only on the aerial current and is independent of the height.

lengths it would show an increase of efficiency. To allow for this on wave-lengths between λ_n and $1{\cdot}5\lambda_n$ we should multiply the value of P_r obtained from equation (11) by $(\lambda/1{\cdot}5\lambda_n)^2$.

The question of sending on wave-lengths below λ_n is not here considered, as the efficiency depends much on the type of shortening condenser used.

It will be noticed from equation (11) that the proportion of power radiated by a sending aerial with a good earth, and consequently its effect on a distant receiving station, depend on the ratio of the natural wave-length to the wave-length employed. It is not affected by the height, except in so far as an increase in height increases the natural wave-length and therefore the ratio λ_n/λ. This increase may be equally well effected by prolonging the horizontal part of an aerial, provided always that a corresponding amount of work is done in extending the earthing system so that R_e is kept down to its minimum (which should be, at $1{\cdot}5\lambda_n$, about 50 per cent of the total resistance).

It also follows that the total resistance $R_r + R_e$ of a short-topped aerial will always be greater than that of a long one, assuming the best earth is used in each case. For at $1{\cdot}5\lambda_n$, with 50 per cent radiation efficiency, the total resistance is twice the radiation resistance, i.e. $2 \times 197(l/1{\cdot}5\lambda_n)^2$, and $l/1{\cdot}5\lambda_n$ decreases as the length of the roof increases.

This can be best illustrated by a typical numerical example. Assume two inverted L aerials each with effective height 100 m.; make the roof of one aerial "*a*," about 30 m. long, so that its natural wave-length is about 660 m., and make the roof of the other, "*b*," about 200 m. long to give it a natural wave-length of about 1 320 m. Then with "*a*" on its most efficient wave-length, 1 000 m., we have

$$R_r = 197\left(\frac{200}{1\,000}\right)^2 = 8 \text{ ohms}$$

so that R_1 with the best earth will be about 16 ohms, and R_e about 8 ohms. With "*b*" on its most efficient wave-length, 2 000 m., we have

$$R_r = 197\left(\frac{200}{2\,000}\right)^2 = 2 \text{ ohms}$$

so that in this case a properly constructed earth should bring the total resistance down to about 4 ohms, and R_e to about 2 ohms.

Again, with "*a*" sending on 2 000 m., R_r becomes 2 ohms; the total resistance will, however, not be reduced below the 16 ohms but rather will begin to increase, so that R_e (which includes resistance due to the introduction of a large tuning coil) is increased from about 8 to about 14 ohms. By increasing the length of roof, with a corresponding extension of the earthing system, we should reduce the total resistance at 2 000 m. to a minimum of about 4 ohms, and R_e to about 2 ohms, as in "*b*." The principal causes of this reduction of R_e are the increased surface over which the current entering the earth is distributed, and the elimination of the large tuning coil.

The practical limit to which length can be taken from the vertical and added to the horizontal part of a sending aerial, without detriment, is reached when R_r becomes so small that no amount of attention to the earth connections can reduce the ratio $R_r/(R_r + R_e)$ to an economical figure. For instance, let us consider a third aerial, "*c*," with effective height only 12·5 m., roof 275 m. long, natural wave-length about 1 320 m., and sending on the most efficient wave-length of 2 000 m. Then R_r is only 1/32 ohm, and it would be impracticable to reduce R_e to anything like an economical value; in fact, 2 or 3 ohms is probably the minimum it could be reduced to in any case, so that instead of about 50 per cent efficiency at $1{\cdot}5\lambda_n$ we should have only about 1 or 2 per cent.

It is interesting to take some published examples of representative cases, for instance:—

(*a*) Sayville, Long Island (*Telefunken*): Umbrella aerial; insulated counterpoise earth; h to centre of spokes is about 80 m. and l therefore 160 m.; λ_n is 1 800 m. At 3 000 m. λ, R_1 is given as 1·5 ohms,* and at this wave-length R_r works out at 0·6 ohm, so the radiation efficiency is 40 per cent. At $1{\cdot}5\lambda_n$, or 2 700 m., R_r is 0·75 ohm and, as R_1 would be about the same as before, the radiation efficiency would be 50 per cent.

(*b*) Hanover (*Goldschmidt*): Double-cone aerial; h is 210 m. and the effective height may be taken as $2h/\pi$ or $0{\cdot}64h$, so that l is $1{\cdot}28h$ or 270 m.; λ_n is 2 800 m. At 7 400 m., R is given as 3 ohms,* and at this wave-length R_r works out at 0·26 ohm, so the radiation efficiency is 9 per cent. At $1{\cdot}5\lambda_n$ or 4 200 m., R_r is 0·83 ohm, and, as R_1 at 4 200 would probably be rather less than at 7 400, the radiation efficiency at 4 200 m. would be in the neighbourhood of 30 per cent.

(*c*) Melbourne (*Balsillie*): Squirrel-cage aerial; h is 54 m., so l is about $1{\cdot}28h$ or 69 m.; λ_n is 550 m. At 600 m., R_1 is given as 6 ohms,* and at this wave-length R_r works out at 2·8 ohms, so the radiation efficiency is 47 per cent. At $1{\cdot}5\lambda_n$, or 825 m., R_r is 1·5 ohms, and at this wave-length the value of R_1 worked out from the theoretical curve † comes to 2·6 ohms and in practice would probably be rather more, putting the radiation efficiency in the neighbourhood of 50 per cent.

The generally accepted idea that the transmitting range of an aerial increases with its height, as such, can be traced to the fact that so many aerials have bad earth connections (especially portable field aerials), giving an excessive value to R_e and R_1; and any increase in l leads to a large increase in R_r without a proportionate rise in the already excessive R_1. Moreover, an increase in height without an accompanying reduction in horizontal length generally improves the ratio λ_n/λ [see equation (11)].

(7) Returning from this digression and combining equations (11) and (9), we have

$$\sqrt{P_2} = 0{\cdot}8\sqrt{(1{\cdot}125P_1)}\frac{\lambda_n}{\lambda} \cdot \frac{h_2}{x}\epsilon^{-0{\cdot}0015x\sqrt{\lambda}}$$

$$\therefore \lambda_n h_2 \sqrt{P_1} = \frac{\sqrt{P_2}}{0{\cdot}85}\lambda x \epsilon^{+0{\cdot}0015x\sqrt{\lambda}} \quad . \quad . \quad (12)$$

* These values are half those given in Dr. Eccles's "Handbook," for the stations in question, because in that book aerial resistance is taken as the quotient of the power developed in the aerial and the mean square current throughout the aerial. To reduce it to our rating, for the current at the foot of the aerial we must divide by 2.

† W. H. Eccles (*loc. cit.*, Fig. 57, p. 129).

(8) To complete the formula we must allot a value to P_2. During Austin's tests, just audible signals were obtained with a current at the foot of the receiving aerial of 10 micro-amperes. As the receiving aerial resistance was 25 ohms, this gave a power of $I_2^2R_2 = 2{\cdot}5 \times 10^{-9}$ watt. In subsequent tests in 1913, with improved receiving apparatus, just audible signals were obtained with a current of 5 micro-amperes at the foot of an aerial of 25 ohms' resistance. This entailed one-quarter of the previous power, or $0{\cdot}625 \times 10^{-9}$ watt, and we will say that $0{\cdot}6 \times 10^{-9}$ watt is the power required with modern receiving sets without amplification to give just audible signals.

Then, by assumption (7) on page 158,

$$P_2 = 64 \times 0{\cdot}6 \times 10^{-9} \text{ watt} = 38 \times 10^{-9} \text{ watt}$$

(9) Substituting this in (12) we get

$$\lambda_n h_2 \sqrt{P_1} = \frac{\sqrt{(38 \times 10^{-9})}}{0{\cdot}85} \lambda x \epsilon^{+0{\cdot}0015x/\sqrt{\lambda}}$$

or, expressing h_2 in metres for greater convenience

$$\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23\lambda x \epsilon^{+0{\cdot}0015x/\sqrt{\lambda}} \quad . \quad . \quad . \quad (13)$$

Equation (9) shows that, for any power radiated, the greater the wave-length the better the result, and the greater the range the more marked is the advantage of long waves. We have seen, however, that for any aerial the longer the wave above $1{\cdot}5\lambda_n$ the less power is radiated. This indicates that for every set of conditions there is a best wave-length, and this conclusion is confirmed by equation (13). This equation shows that for every value of x there is a best value for λ, at which $\lambda_n h_2 \sqrt{P_1}$ is a minimum; this value is given by the condition

$$\lambda = \tfrac{1}{4}(0{\cdot}0015x)^2 \quad . \quad . \quad . \quad . \quad (14)$$

and assuming this best wave-length to be employed equation (13) becomes

$$\lambda_n h_2 \sqrt{P_1} = 96 \times 10^{-8} x^3 \quad . \quad . \quad . \quad (15)$$

At short ranges the wave-length given by equation (14) is too small for practical purposes. For instance, for a 50 km. range it is only 140 cm., and it is not until the range exceeds 800 km. that the ideal wave-length over sea exceeds 300 m. Moreover, 300 m. is the shortest wave for which the authors of the Austin-Cohen formula verified their conclusions in practice. Accordingly, this is the shortest wave considered in the following curves, which show the value of $\lambda_n h_2 \sqrt{P_1}$ for any value of x up to 4 000 km., at various wave-lengths.

SECTION III.

The curve tangential to the backs of the curves in Fig. 1 is that given by equation (15) in the form

$$\frac{\lambda_n h_2 \sqrt{P_1}}{x} = 96 \times 10^{-8} x^2$$

It will be seen that, so long as the range is short for the wave-length employed, the curves approximate to straight lines, and for these short ranges the condition

$$\frac{\lambda_n h_2 \sqrt{P_1}}{x} = Kx + 0{\cdot}23\lambda \quad . \quad . \quad . \quad (16)$$

is approximately correct, where the value of K depends on the wave-length.

For instance, the curve for 600 m. wave-length, up to 200 km. range, can be approximately rendered by

$$\frac{\lambda_n h_2 \sqrt{P_1}}{x} = 0{\cdot}0003x + 0{\cdot}138 \quad . \quad . \quad (17)$$

At short ranges Kx is small compared with $0{\cdot}23\lambda$, and the shorter the range the more negligible does Kx

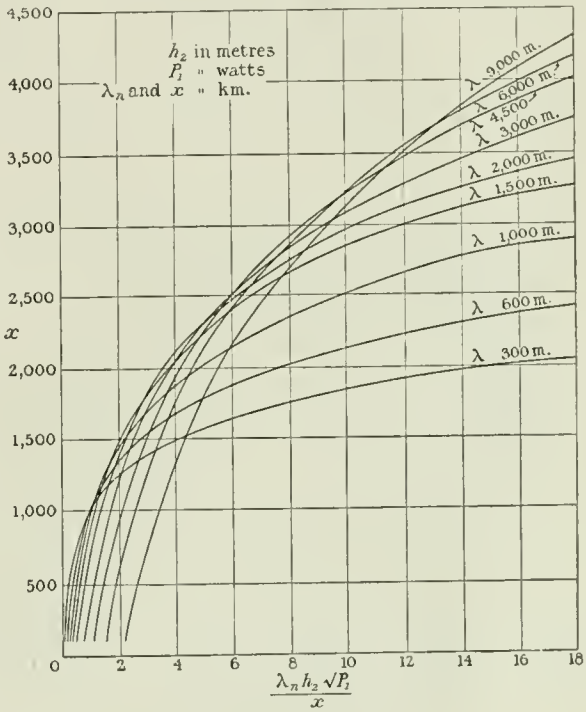

FIG. 1.—Over sea.

become. At such ranges, then, we approach the condition $\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23\lambda x$, i.e. $P_1 \propto x^2$ or $I_2 \propto 1/x$, which is the theoretical condition for radiation over a perfectly conducting plane surface. At these ranges the earth's curvature is little felt, and for transmission over sea we have the conditions of a well-conducting nearly-plane surface. This conclusion may be reached direct from equation (1) or (13), since the smaller the value of x compared with λ the nearer does $\epsilon^{-0{\cdot}0015x/\sqrt{\lambda}}$ approach unity. This expression is a measure of the absorption due to the earth's surface; in a perfectly-conducting plane surface it would become zero, leaving equation (13) in the form

$$\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23\lambda x$$

and equation (1) in the form

$$I_2 = 4{\cdot}25 \frac{I_1 h_1 h_2}{\lambda x} \quad . \quad . \quad . \quad . \quad (18)$$

It is interesting to compare this latter with the theoretical formula for radiation from a Hertzian oscillator. This is given in the form

$$F = 2\pi \frac{l_1 a}{\lambda x} \times 3 \times 10^{10} \quad . \quad . \quad . \quad (19)$$

where F = electric force per cm. at distance x in C.G.S. units,
l = length of dipole in C.G.S. units,
a = R.M.S. current, taken as the same at all points of the oscillator, in C.G.S. units.

If we take F in volts per cm. ; x, l, and λ in km. as in equation (1) ; a, in amperes, as in equation (1) ; the equation becomes

$$F = 2\pi \frac{l_1 a}{\lambda x} \times 3 \times 10^{-4} \quad . \quad . \quad . \quad (20)$$

We may consider the earthed aerial to be the upper half of a Hertzian oscillator as before, with effective

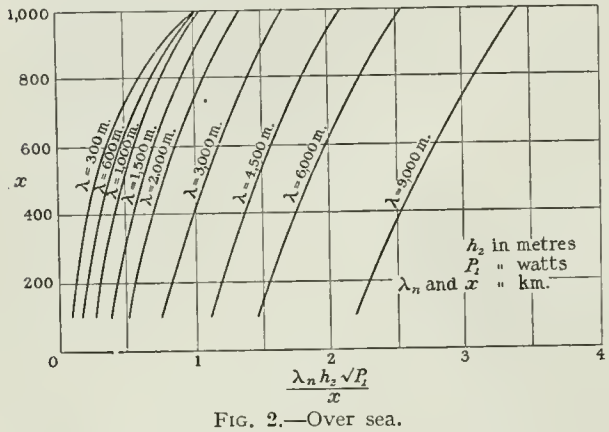

Fig. 2.—Over sea.

height $\frac{1}{2}l_1$. Call the effective height of the receiving aerial $\frac{1}{2}l_2$; then if the voltage in the aerial were maintained at the level of F volts per cm. we should have

$$V_2 = F \times \tfrac{1}{2}l_2 \times 10^{-5}$$

where V_2 is the total impressed electromotive force (R.M.S. value)

$$\therefore F = \frac{2V_2}{l_2} \times 10^{-5} = \frac{2I_2R_2}{l_2} \times 10^{-5}$$

And we have taken R_2 in our formula as 25 ohms, so we have

$$F = \frac{5I_2}{l_2} \times 10^{-4}$$

Substituting this in equation (20) we get

$$\frac{5I_2}{l_2}10^{-4} = 2\pi \frac{l_1 a}{\lambda x} \times 3 \times 10^{-4}$$

$$\therefore I_2 = 15 \frac{a \cdot \frac{1}{2}l_1 \cdot \frac{1}{2}l_2}{\lambda x} \quad . \quad . \quad . \quad . \quad (21)$$

In an aerial we may write $I/\sqrt{2}$ for a, and we have taken the effective heights of the aerials used to be 0·93 times the actual height, so that $\frac{1}{2}l_1 = 0{\cdot}93h_1$, and $\frac{1}{2}l_2 = 0{\cdot}93h_2$. Substituting in (21) we get

$$I_2 = 9 \frac{I_1 h_1 h_2}{\lambda x} \quad . \quad . \quad . \quad . \quad . \quad (22)$$

Equation (19) applies correctly only to continuous waves.

For spark transmission the value of I_2 obtained above should be multiplied by the factor $\sqrt{\left[\frac{1}{1 + (\delta_1/\delta_2)}\right]}$ where δ_1 and δ_2 are the logarithmic decrements of the transmitted wave-trains and the receiving antenna assemblage respectively. In practice this factor may be about 0·9.

It will be seen from a comparison of equations (18) and (22) that the current received under the practical Austin-Cohen formula at short ranges is only 50 per cent of that calculated theoretically. The full discussion of this apparent discrepancy involves considerations beyond the scope of this article and will not be embarked on here. A suggestion put forward in the former paper * under this heading was that it was incorrect to assume

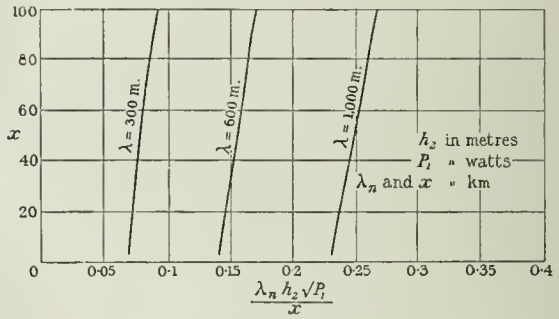

Fig. 3.—Over sea.

the voltage in the receiving aerial to be maintained at the level of the electric force in the wave field, and that in fact a local drainage of the field occurred.

This has been criticized by Professor Howe, and is mentioned again with some diffidence. If, however, it is incorrect and V_2 is constant, we are faced with the condition $P_2 \propto 1/R_2$, since $P_2 = V_2^2/R_2$. That is, we could double the power collected by an aerial from any wave-field by the act of halving its resistance (due to earth, coupled closed circuit, re-radiation, etc.), and any increase of coupling the closed receiving circuit, by increasing R_2, would result in less power being received, and vice versa. The power collected would not in any way be affected by the capacity, so that if we extended the spread and increased the capacity, keeping R_2 constant, we should collect no additional power from the passing waves. The conception that the increased aerial spread affected a larger wave-field and therefore collected more power would be incorrect.

This appears so contrary to general experience that the author ventures on the following as a working hypothesis sufficiently near the true state of affairs (which is a series of complex interactions) for the

* *Electrician*, 1917, vol. 79, p. 102.

practical purposes of this paper, namely, that the aerial collects a certain amount of power from the passing waves depending mainly on its height, capacity and properties of re-radiation, and the voltage and current developed from this power then depend on the total resistance of the aerial. This will be referred to later in considering the effect on signals of the capacity and earth resistance of a receiving aerial.

SECTION IV.

Having discussed above the relations between height, wave-length, power and range for an assumed normal set of conditions, we shall now consider the effect of variations from those conditions.

(1) *Nature of intervening surface.*—It is probable that, with long-range stations using very long waves, the intervening surface does not much affect matters unless much of it is mountainous, or either station is screened by a near mountain range. For short-range work over land, however, with small wave-lengths, the value of $\lambda_n h_2 \sqrt{P_1}$ obtained from the curves is too low. In the first place the wave-length obtained from equation (14) is too small for transmission over land, and a longer wave must be used giving a higher value to $\lambda_n h_2 \sqrt{P_1}$. In the second place this higher value applies only to transmission over sea for that wave-length, and must be further increased for transmission over land.

Dr. Eccles * gives the following formula for the screening effect of mountains, based on the diffraction theory :—

$$P_2 \propto \frac{P_1}{\lambda^2 x^2} \epsilon^{-2\sqrt{(2x\chi_1\chi_2\lambda)}} \quad . \quad . \quad . \quad (23)$$

where (if S and R are a sending and receiving station and T the intervening crest)

χ_1 = angle TSR in radians (1 radian = $180°/\pi$)
χ_2 = angle TRS in radians,

x and λ being in any like units.

This may be written

$$\sqrt{P_1} \propto \sqrt{P_2}\lambda x \epsilon^{\sqrt{(2x\chi_1\chi_2/\lambda)}}$$

Combining this with equation (13) we get

$$\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23\lambda x \epsilon^{[(0{\cdot}0015x + \sqrt{2x\chi_1\chi_2})/\sqrt{\lambda}]} \quad . \quad (24)$$

or the value of $\lambda_n h_2 \sqrt{P_1}$ obtained from the curves must be modified by the factor $\epsilon^{\sqrt{(2x\chi_1\chi_2/\lambda)}}$. This factor will allow for screening by the height of opaque land intervening, and it may assume a considerable value.

We have also to allow for loss due to the actual accidents of surface, whether hilly, mountainous, heavily wooded, bearing magnetic ores on the surface, etc. This is exceedingly high for short waves, but much less for long. One way of making a rough allowance is to consider the overland range equivalent to an increased range over sea, the increase depending on the kind of land and the wave-length. There are, however, no sufficiently inclusive data on which to base such a direct estimate of equivalent ranges. Another method here put forward is to multiply the oversea value of $\lambda_n h_2 \sqrt{P_1}$ by ϵ^A, where A is some function of x and λ; there is not enough information available to deal with the matter fully, but the form $\epsilon^{Kx^{\frac{1}{2}}/\lambda}$ is suggested as satisfying practical conditions, where K is a coefficient depending on the nature of intervening land.

The following values are proposed for K :—

For flat land	0·02
For hilly land	0·1
For mountainous land ..	0·24

No authority is claimed for the above suggestion and it is based on no special series of tests. It is put forward, however, as giving consistent results in accordance with practice.

Equation (13) then becomes

$$\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23\lambda x \epsilon^{\frac{0{\cdot}0015x}{\sqrt{\lambda}} + \frac{Kx^{\frac{1}{2}}}{\lambda}} \quad . \quad . \quad (25)$$

This in turn suggests a most economical value of λ for every range, at which $\lambda_n h_2 \sqrt{P_1}$ is a minimum. The solution of the differential equation for the minimum gives the following result :—

$$\sqrt{\lambda} = \sqrt{\left[Kx^{\frac{1}{2}} + \left(\frac{0{\cdot}0015x}{4}\right)^2\right]} + \frac{0{\cdot}0015x}{4} \quad . \quad (26)$$

It will be seen that, when K is zero, this gives the same result as equation (14). The curves given for flat, hilly and mountainous land are worked out from equation (25).

(2) *Types of aerial.*—The aerial to which the curves apply has an effective height of about 0·93 times its actual height, while the capacity of the receiving ship's aerial is given as 0·0018 μF.

In using various types of aerial we should make the following allowances :—

(*a*) *Sending.*—The only allowance necessary in sending is that involved in the use of the directive aerial, which is treated below under a separate heading.

The case of the aerial whose height is so small compared with its length that it must be an inefficient radiator, because its radiation resistance is so much less than the minimum practicable earth resistance (see page 160), is included below in considering the effect on transmission of a bad earth connection, under the heading " Earth Connections."

(*b*) *Receiving.*—The conditions here are much more complex than in sending, owing largely to the partial re-radiation of the received energy. Authorities differ on the various effects, and it is not proposed to go into all the considerations here. One important point, however, pointed out by Dr. Eccles,* must be explained. This is broadly as follows :—

The coupled detector circuit introduces an effective resistance into the aerial, which may be increased or decreased by closing or loosening the coupling. On the old principle that most work is

* W. H. Eccles (*loc. cit.*, p. 171).

* W. H. Eccles (*loc. cit.*, p. 173).

done when the external (working) resistance is equal to the internal (wasting) resistance, the coupling should be such that the effective resistance introduced is equal to the total aerial resistance from re-radiation, earth and all other sources. When this is achieved 50 per cent of the aerial's energy will be delivered to the coupled circuit, and more than this cannot be delivered.

Thus, whatever the resistance of the receiving aerial, about 50 per cent of its received energy may under best conditions be delivered to the coupled circuit.

Considering then the effects of the qualities of an aerial on receiving we may make the following allowances.

Effective height.—The value of $\lambda_n h_2 \sqrt{P_1}$ obtained from the curves must be multiplied by $1 \cdot 86 h_2 / l_2$, where $\frac{1}{2} l_2$ is the effective height of the receiving aerial. In most flat-top aerials this factor is near unity and need not be applied.

Capacity.—In receiving, on the hypothesis given above (see Section III) we must make allowance for the effect of a greater or less aerial capacity. If no disturbing factor were introduced the ratio $P_2 \propto C_2$ would apply, where C_2 is the capacity of the receiving aerial. This ratio may be applied with sufficient accuracy for our purposes to small or medium-sized aerials with good earths. For long-distance directive aerials, however, using spark telegraphy, where the great sending power requires a very large acrial capacity, it has been found advisable to use a smaller capacity for receiving to avoid excessive earth losses. The approximate accuracy of this is supported by an experiment carried out by D. Darrin, in which he increased the number of spokes of an umbrella aerial and noted the result on the current received. The resulting curve is given in Fig. 56 of Dr. Eccles's "Handbook." Taking the square of the received current against the number of wires, we get:—

Number of Spokes	I_2^2
1	4·8
2	7·8
3	10·9
4	13·7
5	15·2
6	16·8
7	17·6
—	—
—	—
12	21·2

It will be seen that the increase of I_2^2 keeps pace with the increase of capacity that would be expected from the number of spokes. If the total resistance of the aerial and galvanometer did not vary greatly during the experiment, we have approximately the condition

$$I_2^2 R_2 \propto C_2, \text{ or } P_2 \propto C_2$$

We will say then, as an approximate estimate for general application, that the value of $\lambda_n h_2 \sqrt{P_1}$ obtained from the curves should be multiplied by $\sqrt{(0 \cdot 0018 / C_2)}$. while the allowance to be made for earth losses will be discussed later.

It is therefore necessary for our purposes to be able to form an approximate estimate of the capacity of a receiving aerial. The capacity of a single-wire aerial per unit length depends mainly on the ratio of the diameter to the height of its various elements, but for practical purposes of an approximate estimate we may say that the capacity per metre length is about $6 \cdot 5 \times 10^{-6}\ \mu$F. For any other form of aerial we are

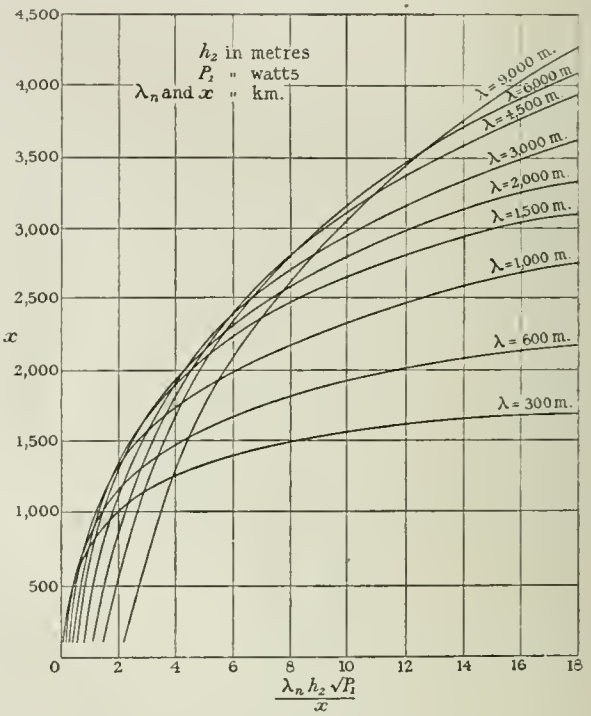

FIG. 4.—Over flat land.

indebted to L. W. Austin for the following empirical formula * :—

$$C = \left(4\sqrt{a} + 0 \cdot 885 \frac{a}{h}\right) 10^{-5} \quad . \quad . \quad . \quad (27)$$

where C is capacity in microfarads,
a is area in square metres,
h is mean height in metres.

This applies to aerials not too elongated in shape and having their wires not too widely spaced. For aerials having a length l more than eight times the breadth b, the above formula must be multiplied by an elongation factor, giving us

$$C = \left(4\sqrt{a} + 0 \cdot 885 \frac{a}{h}\right)\left(1 + 0 \cdot 015 \frac{l}{b}\right) 10^{-5} \quad . \quad (28)$$

It is claimed that these equations can be depended upon to give results correct in general to within 10 per cent for the antenna top, to which must be added

* *Proceedings of the Institute of Radio Engineers*, April 1920, vol. 8, No. 2.

the capacity of the down leads and that due to metal towers, etc.

(*c*) *Directive aerials.*—A special case is that of the directive inverted L aerial. If both sending and receiving aerials are of this kind, the sending power required to deliver a certain power to the receiving aerial is a fraction of that required with non-directive aerials of the same total radiation. When the length is about 10 times the height a study of the polar diagrams suggests a value of about 0·4 for this fraction, and accordingly we may modify the value of $\lambda_n h_2 \sqrt{P_1}$ by $\sqrt{0{\cdot}4}$, or 0·63, if both aerials are directive, or by 0·8 if one aerial only is directive.

(3) *Earth connections.*—The results given by the curves will not be reached even approximately if a bad earth connection is used. As already explained, the radiation efficiency is $R_r/(R_r + R_e)$ and the greater part of R_e is due to earth connections. With the best possible earth in good soil on the most efficient wave-length (about $1{\cdot}5\lambda_n$), R_e will hardly be less than R_r. With a bad earth it may be many times as great,

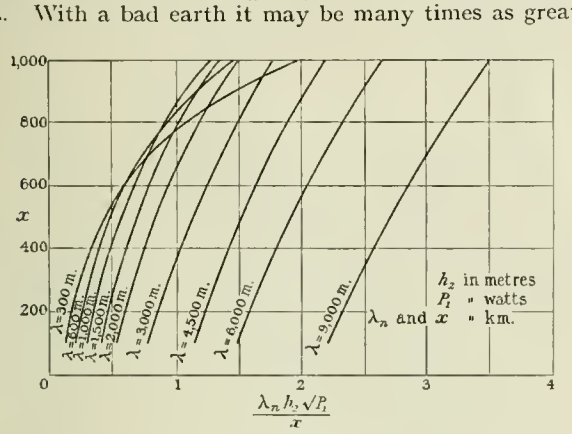

FIG. 5.—Over flat land.

and the importance of keeping it as low as possible is evident. This is especially the case with an aerial of low radiation resistance. A bad earth also increases the damping and impairs resonance; this is especially harmful in sending with the quenched spark, where the oscillations in the aerial decay with its own decrement, and receive no reinforcement from a loosely-coupled closed circuit.

For the purposes of this discussion we will call a good earth one in which R_e is nearly equal to R_r at about 1·5 times the natural wave-length; a moderate earth one in which R_e is about three times R_r; and a bad earth one in which R_e is in the neighbourhood of ten times R_r. Then in cases where bad soil, a low value of R_r, or other causes impose a bad earth the best radiation efficiency becomes about $R_r/(R_r + 10R_r)$, or 0·09 instead of 0·5; that is, the necessary power must be multiplied by about 6, or $\lambda_n h_2 \sqrt{P_1}$ by about $\sqrt{6}$ or 2·4. For a moderate earth P_1 should be multiplied by 2, or $\lambda_n h_2 \sqrt{P_1}$ by 1·4.

Where the height of a sending aerial is less than about 1/25th of its natural wave-length, the value of R_r will be so small that a special allowance must be made in each case for low radiation efficiency.

The effects of a bad earth connection are much less felt in receiving, because there the coupled circuit can take about half the power collected by the aerial, whatever the resistance of the latter. But, as the absorbing qualities of an aerial depend partly on its powers of re-radiation (since a good radiator is a good absorber), a bad earth connection impairs reception by reducing the radiation properties of an aerial.

If an insulated counterpoise is used in sending, the value of R_e will generally, but by no means invariably, be less than with the conductive connection, and the radiation efficiency greater.* The use of a counterpoise also imparts to its aerial a smaller capacity and greater inductance than an earth, and so reduces the decrement at any wave-length, since $\delta \propto R/(fL)$. In spite of this, due perhaps to the absence of some "ground wave" phenomenon, its effect at a distance is not so great as that of a good earth. For the sake of an approximate estimate we may class a counterpoise at the sending station with a moderate earth, entailing a modifying factor of about 1·4 to be applied to $\lambda_n h_2 \sqrt{P_1}$.

On account of its small decrement, however, a properly

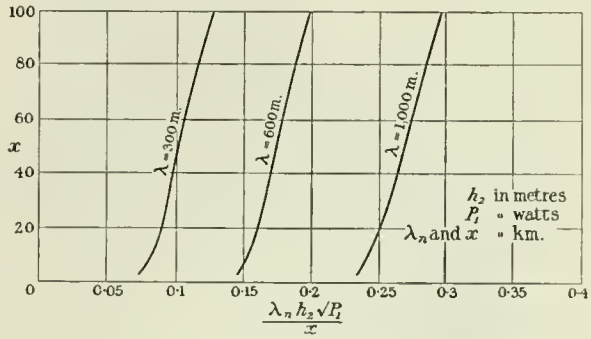

FIG. 6.—Over flat land.

built counterpoise of large area may be used with advantage in quenched spark systems, instead of a direct earth connection. In these systems it is important to keep the aerial decrement from all sources as low as possible, in order to get good resonance effect.

The capacity of an earthed aerial is greater than that of the same-sized aerial with a counterpoise. In receiving, due partly to this greater capacity and partly, perhaps, to some "ground wave" effect, there is no doubt that an earthed aerial picks up a considerable amount of energy that is missed by a counterpoise aerial. Also, as already stated, a receiving aerial is much less affected by a bad earth than a sending aerial, and it appears in practice that even a bad earth may receive more strongly than a good counterpoise. At certain stations fitted with earth and counterpoise for alternative use, but unfortunately without means of measuring the received power, it appeared that the aerial with a good earth received about 100 per cent stronger signals than with a good counterpoise, and this excess was rather less with a bad earth. We will

* The use of the counterpoise eliminates the resistance of the direct earth contact, but it introduces a large resistance due to induction of earth currents below the counterpoise. The latter will generally be less than the resistance of direct contact unless the conductive earth is a very good one.

say then, for the sake of an approximate estimate, that the value of $\lambda_n h_2 \sqrt{P_1}$ must be multiplied by about $\sqrt{2}$ or 1·4 for receiving with a counterpoise, and by a less amount, say 1·2, for receiving with a bad earth connection. The effect of a moderate instead of a good earth in receiving is not great enough to necessitate any modification of our figures.

(4) *Sending apparatus.* — Where continuous-wave transmission is used instead of the spark system to which the curves apply, there is a gain in resonance effect so that, if the same power is radiated in continuous waves as in damped trains, the former will deliver more power to the receiving aerial. This increase in efficiency has been discussed by Dr. Eccles and others, and depends on the decrements of the various circuits; it is shown that modern spark systems are capable of such a high degree of resonance that

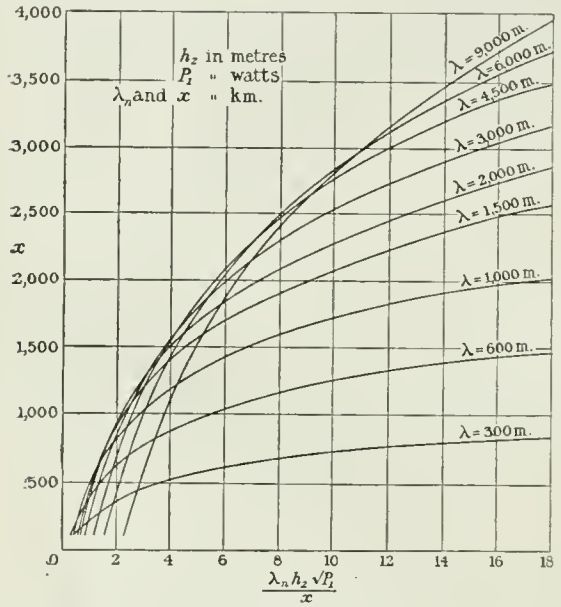

FIG. 7.—Over hilly land.

the efficiency gain in using continuous waves is small; about 20 per cent gain is a fair allowance, so that $\lambda_n h_2 \sqrt{P_1}$ must be multiplied by $\sqrt{0{\cdot}8}$ or 0·9.

The great advantage of continuous waves lies, not in the greater power delivered to the receiving aerial, but in the fact that the power delivered can be subjected to the "beat" or heterodyne method of treatment. This is considered in the following section.

(5) *Receiving apparatus.*—The efficiency of a receiving set, whether with a crystal or valve detector, is by no means constant. There may be temporary dullness in the detector; there is room for minor errors in tuning, coupling, voltage adjustments, etc., and the operator's personal factor is involved. The large factor of 64 times audibility power which is taken should cover such variations (since 16 times audibility is considered to give good communication), and so long as there is nothing seriously wrong with the receiving set the curves may be applied without modification for all ordinary purposes.

With the more modern receiving methods introduced by the development of the triode valve for rectifying, amplifying, and with continuous waves heterodyning the incoming signals, the possible combinations are so many that the question must be treated on the broadest lines to meet the purpose of this paper.

With a perfectly reliable amplifying device and no atmospheric or other interference, the sending power could be reduced in the exact amplification ratio. In practice, with interference to be considered, the effect of this might be to reduce the strength of signals below the strength of normal unavoidable interference, so that there would be no gain in magnifying both. We may now be on the way to universal adoption of continuous-wave methods and the perfection of some atmospheric eliminator. When this is achieved we may look for a great modification of the sending power and height now considered necessary for any range, and the value of $\lambda_n h_2 \sqrt{P_1}$ obtained from the curves may be modified by a factor of $\sqrt{(U/600)}$, where U is the minimum audibility power in the aerial, in micro-microwatts, for the particular receiving device employed.

At the present stage of development, with powerful

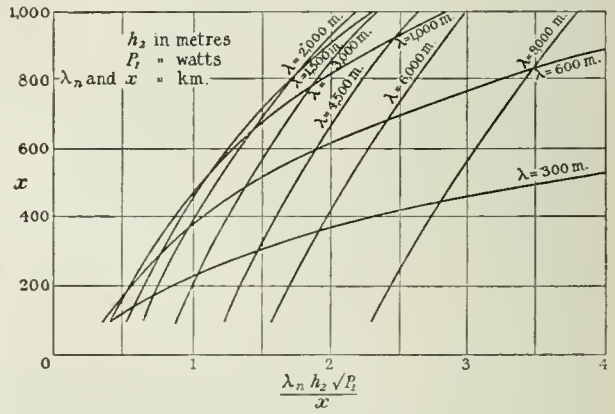

FIG. 8.—Over hilly land.

spark stations still in existence and the perfect atmospheric preventer still to come, we have to arrive at modifying factors to suit practical conditions. The subject is considered on the following lines:—

(a) *Beat reception with continuous-wave transmission.* —Where beat reception is used, the strength of signals is greatly increased with no further amplification than that involved in the heterodyning process. Moreover, outside interference is not magnified to the same extent as the continuous oscillations received.

Actual figures available for minimum audibility vary between $0{\cdot}15 \times 10^{-9}$ watt when using the earlier method of a separate generating circuit with a high-frequency generator,* and $1{\cdot}2 \times 10^{-15}$ watt when using the more modern valve heterodyne with its accompanying relay action.†

The minimum audibility on which the curves are based is $0{\cdot}6 \times 10^{-9}$ watt in the aerial—say $0{\cdot}3 \times 10^{-9}$ watt delivered to the detector circuit—so that the above

* J. L. HOGAN. † L. W. AUSTIN.

figures imply amplification varying from 2 with the earlier method to 250 000 with the modern valve arrangement. The latter figure could only be relied on under laboratory conditions with the optimum combination of several highly critical interacting adjustments, any one of which may involve a multiplication of several hundreds. For practical signalling conditions, with ordinary operators and a normal amount of interference, a safe factor of 25 is suggested, so that $\lambda_n h_2 \sqrt{P_1}$ may be multiplied by $\sqrt{(1/25)}$ or 1/5 to allow for beat reception.

(*b*) *Amplification.*—Received signals can be multiplied to almost any extent by the use of valves in cascade, in addition to any amplification entailed in heterodyning in the case of continuous waves. The practical limit is only reached when accidental retro-action between successive valve circuits becomes unmanageable. A 7-valve instrument, for instance, allowing the very moderate-power amplification of 5 per valve, gives a multiplication of 78 000. But, as interference of all kinds is amplified at the same time, there is a limit to which we can profit by amplification to reduce our sending power. A practical figure of

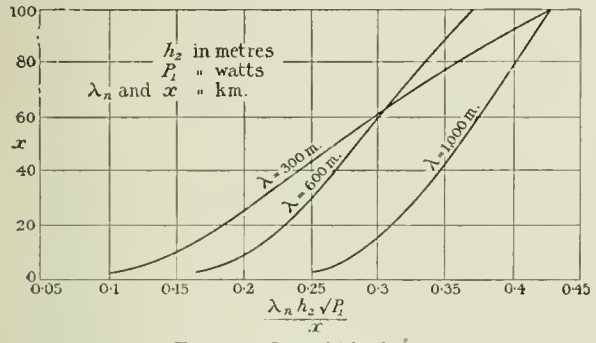

Fig. 9.—Over hilly land.

10 will be taken for this paper, so that $\lambda_n h_2 \sqrt{P_1}$ may be multiplied by $\sqrt{0{\cdot}1}$, or 0·3.

(6) *Time of day.*—The question of "freak" signals by night, sunset and sunrise effects, etc., will not be considered here. Although there is no doubt that short waves travel better by night, there is some difference of opinion as to the night effect on long waves. It is probable, however, that all wave-lengths travel better by night, and for general purposes this may be allowed for by replacing the figure 0·0015 in the exponential index by a smaller figure 0·001, or by multiplying the daylight value of $\lambda_n h_2 \sqrt{P_1}$ by the factor $\epsilon^{-0{\cdot}0005x/\sqrt{\lambda}}$. This method is only approximately correct, since the night absorption appears to depend on the range, and is less at the longer ranges.

(7) *Atmospheric conditions.*—In conditions under which little or no atmospheric interference may be expected, such as is the case with small-capacity low aerials, or in districts notedly immune from atmospherics, a smaller power than 64 times audibility may safely be taken, but the minimum power received should not be less than 16 times audibility. That is, in such favourable cases, the value of $\lambda_n h_2 \sqrt{P_1}$ given by the curves may be modified by $\sqrt{0{\cdot}25}$ or 0·5.

The question of seasonal fluctuations of signal strength is not treated here. It is considered that such fluctuations are covered by the "reliability factor" of 64.

(8) *Degree of reliability.*—It may often be the case that a smaller margin of signal strength than that assumed is sufficient; such may occur when there are only a few short messages to be expected daily, or when one may count always on the best operators working under the best conditions. In such cases the received power may be reduced to about 16 times audibility, or $\lambda_n h_2 \sqrt{P_1}$ modified by a factor of not less than 0·5.

A table of all the modifying factors discussed above is appended:—

(1)	For screening by mountains	$\epsilon^{\sqrt{(2x_1x_2/\lambda)}}$
(2)	For intervening surface	$\epsilon^{Kx^{\frac{4}{3}}\lambda}$
(3)	For effective height of receiving aerial	$1{\cdot}86h_2/l$
(4)	For capacity of receiving aerial	$\sqrt{\frac{0{\cdot}0018}{C_2}}$
(5)	For sending on wave-length less than 1·5 times natural wave	$(1{\cdot}5\lambda_n/\lambda)^2$
(6)	For directive aerial one end	0·8
	For directive aerial both ends	0·6
(7)	For bad earth at sending end	2·4
	For moderate earth at sending end	1·4
	For counterpoise at sending end	1·4
(8)	For bad earth at receiving end	1·2
	For counterpoise at receiving end	1·4
(9)	For use of continuous waves in sending (not allowing for beat reception in receiving)	0·9
(10)	For beat reception of continuous waves	0·2
(11)	For use of more sensitive detector than crystal without amplification, where interference need not be considered	$\sqrt{\frac{U}{600}}$
(12)	For use of valve amplifiers under normal conditions of interference	0·3
(13)	For sending by night	$\epsilon^{-0{\cdot}0005x\sqrt{\lambda}}$
(14)	For absence of atmospheric interference	0·5
(15)	For reduction of "reliability factor" to lowest limit consistent with successful communication	0·5

In addition, where the height is very low compared with the aerial length, we must examine the value of R_2 to see if any special allowance should be made for a radiation efficiency lower than contemplated above [see Section IV (3)].

SECTION V.

The foregoing is a partial attempt to deal with a subject which cannot be treated fully without a great deal more investigation and research than is yet available for reference. It is treated from a severely practical point of view, and to this end many approximations

have been made which are open to criticism, and some considerations treated separately which are really inter-related. In conclusion we will take a few examples of results.

Example (1).—What power is required to communicate 400 km. by day between ships, using 600 m. wave-length, with T aerials 60 m. long and 30 m. high, with musical spark transmission and unamplified crystal reception?

Here λ_n is about $4{\cdot}5 \times 60$ m. or $0{\cdot}27$ km., and C_2, with an ordinary ship's 2-wire aerial, would be about $0{\cdot}00085\,\mu$F.

From the curves, $\lambda_n h_2 \sqrt{P_1} = 114$.

To this we must apply the modifying factor for capacity of receiving aerial, namely $\sqrt{\frac{0{\cdot}0018}{0{\cdot}00085}}$ or $1{\cdot}4$.

$$\therefore\ 0{\cdot}27 \times 30\sqrt{P_1} = 114 \times 1{\cdot}4$$
$$\therefore\ P_1 = 400 \text{ watts}$$

This should be developed by a 1·5-kW set.

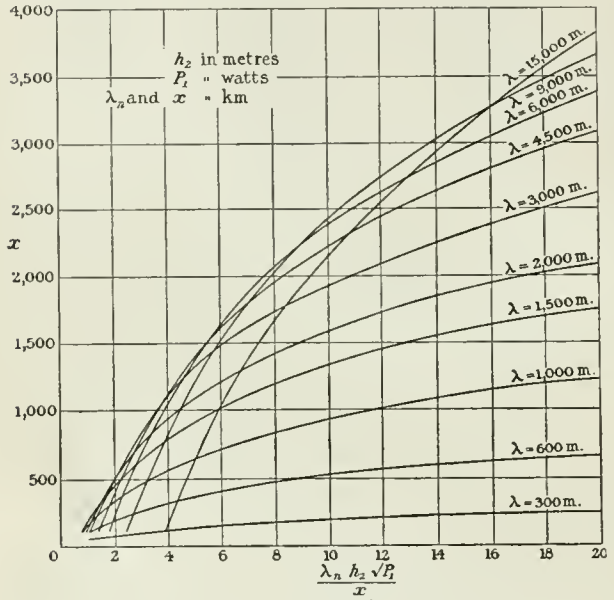

FIG. 10.—Over mountainous land.

Example (2).—What are suitable constants to employ for two stations required for 400 km. by day over hilly land, with moderate earths, using musical spark transmission and unamplified crystal reception?

Here the best wave-length is seen from equation (26) or the curves to be 1 000 m., so that an aerial of natural wave-length 660 m. would be suitable.

A 2-wire inverted L aerial of this natural wave-length would have a capacity about $0{\cdot}0014\ \mu$F.

From the curves $\lambda_n h_2 \sqrt{P_1} = 400$.

Modifying factors are:—

(1) For receiving capacity $\sqrt{\frac{0{\cdot}0018}{0{\cdot}0014}} = 1{\cdot}15$

(2) For moderate earth sending .. 1·4

$$\therefore\ 0{\cdot}66 h_2 \sqrt{P_1} = 400 \times 1{\cdot}15 \times 1{\cdot}4$$

and
$$h_2 \sqrt{P_1} = 1\,000$$

So that if we take h_2 as 50 m. we get $P_1 = 400$ watts. That is, 1·5-kW sets (which should give 400 watts in the aerial), with inverted L aerials 50 m. high and 100 m. long (natural wave-length about 660 m.) would be suitable.

Example (3).—How would the above constants be affected by the use of continuous-wave valve transmission, beat reception and valve amplification?

Here the value of $\lambda_n h_2 \sqrt{P_1}$ may be modified by the factors:—

For use of continuous waves in sending ..	0·9
For use of beat reception	0·2
For use of valve amplifiers	0·3

The product of these is 0·054, so that we have $h_2 \sqrt{P_1} = 54$.

So that if h_2 is 20 m. we have $P_1 = 7$ watts.

This needs consideration, however, because with the height only 20 m. the value of R_r is only

$$200(40 \div 1\,000)^2, \text{ or } 0{\cdot}32 \text{ ohm}$$

It would be difficult in this case to reach the radiation efficiency of 25 per cent assumed above [see Section IV (3)] for a moderate earth at 1·5 times the natural wave-length, as it would mean a total aerial resistance of only 1·28 ohms. In practice it might be preferable to spend less trouble on the earth connections and use more power; for instance, if we allow a total aerial resistance of 5 ohms we must multiply P_1 by $5 \div 1{\cdot}28$, giving us 27 watts.

That is to say, sets developing from 10 to 30 watts in the aerial, according to the state of the earth connections, with inverted L aerials 20 m. high and 130 m. long (natural wave-length 660 m.) would be suitable.

Example (4).—What are suitable constants for two stations 4 000 km. apart, half sea and half hilly land, using arc transmission, beat reception and amplification, and directive aerials, on the best wave-length?

Here the best wave-length both for sea and for hilly land, at 4 000 km., is seen from the curves to be 9 000 m.

On this wave-length we have for 4 000 km. over sea

$$\lambda_n h_2 \sqrt{P_1} = 60\,000$$

and over hilly land $\lambda_n h_2 \sqrt{P_1} = 71\,000$.

Take 65 000 for our case. Then if we use a sending aerial of the most favourable natural wave-length, 6 000 m., it will be about 1 300 m. total length and height. As this is an unwieldy size we will take one of half that length whose natural wave will be 3 000 m. The receiving aerial to correspond might be a single wire of the same length, with capacity about $0{\cdot}004\,\mu$F.

Thus we have the following modifying factors:—

For effective height of receiving aerial ..	0·93
For capacity of receiving aerial $\sqrt{\frac{0{\cdot}0018}{0{\cdot}004}} = 0{\cdot}7$	
For directive aerials	0·6
For continuous-wave sending, beat reception and amplification	0·054
The total product of these is ..	0·02

That is, $\lambda_n h_2 \sqrt{P_1} = 65\,000 \times 0{\cdot}02$

$\therefore\ 3h_2\sqrt{P_1} = 1\,300$

and $h_2\sqrt{P_1} = 430$

If we take h_2 as 50 m. we have $P_1 = 74$ watts. But here again we have R_r at $1{\cdot}5\lambda_n$ (4 500 m.) only $200\left(\frac{100}{4\,500}\right)^2$ ohms $= 0{\cdot}1$ ohm, and it will be obviously impossible to reach a total aerial resistance of only 0·2 ohm, giving us the 50 per cent radiation efficiency on which the curves are based. We should do well in practice if we could get such an aerial down to 2·5 ohms on 4 500 m., so that our figure for P_1 should be multiplied by $\frac{2{\cdot}5}{0{\cdot}2}$ or 12·5. That is, we require 900 watts in aerials 50 m. high and 600 m. long.

Example (5).—How would the above result be affected if we used a 3 000 m. wave-length instead of the most efficient wave of 9 000 m.?

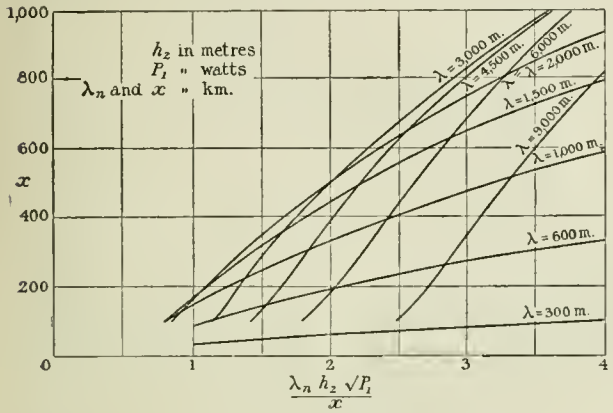

Fig. 11.—Over mountainous land.

In this case we have, for 4 000 km. over sea $\lambda_n h_2\sqrt{P_1} = 100\,000$. Over hilly land, $\lambda_n h_2\sqrt{P_1} = 120\,000$.

Take 110 000 for our case.

The best aerial will have a natural wave-length of about 2 000 m.; that is, a total length and height of about 450 m. Assuming again a single-wire aerial for reception, we have C_2 about $0{\cdot}003\mu$F. Thus we must apply the following modifying factors:—

For capacity of receiving aerial $\sqrt{\frac{0{\cdot}0018}{0{\cdot}003}}$	$= 0{\cdot}8$
For directive aerials	0·6
For continuous-wave reception and amplification	0·054
For effective height of receiving aerial ..	0·93
The total product of these is ..	0·024

So we have $\lambda_n h_2\sqrt{P_1} = 110\,000 \times 0{\cdot}024$

If h_2 is 50 m. $P_1 = 700$ watts

Here again, as the height is low compared with the wave-length, we must examine R_r, which is 200 $(100 \div 3\,000)^2$ ohms or 0·22 ohm. This is still too low to attain a 50 per cent radiation efficiency, as our aerial is sure to have a greater total resistance than 0·44 ohm. If we allow it 2·5 ohms as a practicable value, we must multiply P_1 by $\frac{2{\cdot}5}{0{\cdot}44}$ or 5·7, giving us 4 000 watts required in aerials 50 m. high and 400 m. long.

Example (6).—What would be the screening effect of a range of mountains 4 000 m. high, with the crest 20 km. from one station, if the stations are 2 000 km. apart, on a 2 000 m. wave?

Here $\chi_1 = 0{\cdot}2$ radian

$\chi_2 = 0{\cdot}002$ radian

$\therefore\ \epsilon^{\sqrt{(2r\chi_1\chi_2)}\lambda} = \epsilon^{\frac{1}{2}\sqrt{(2\times 2\,000\times 0{\cdot}0004)}} = 2{\cdot}5$

So that the value of $\lambda_n h_2\sqrt{P_1}$ must be multiplied by 2·5, or P_1 alone by 6, to allow for the height of intervening land, apart from any other modifying factor.

Example (7).—What ranges by day could be relied on between two stations with portable field T aerials 15 m. high and 100 m. long, with bad earth connections, using valve continuous-wave transmission, beat reception, and amplification, if 25 watts are developed in the sending aerial?

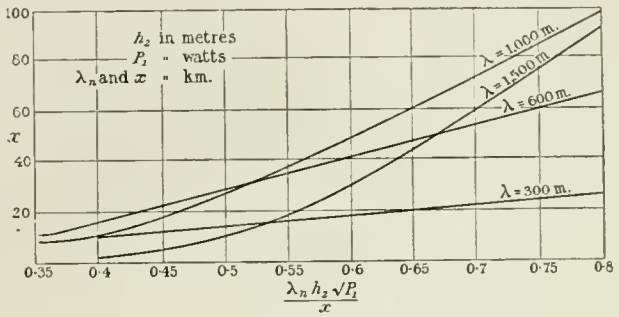

Fig. 12.—Over mountainous land.

(*a*) Over flat land on 2 000 m. wave.
(*b*) Over hilly land on 2 000 m. wave.
(*c*) Over flat land on 1 000 m. wave.
(*d*) Over hilly land on 1 000 m wave.
(*e*) Over flat land on 600 m. wave.
(*f*) Over hilly land on 600 m. wave.

Here λ_n is about $4{\cdot}5 \times 65$ m., or 0·3 km.

And C_2 for such an aerial is about $0{\cdot}001\mu$F.

Thus the various modifying factors are:—

For effective height of receiving aerial	0·93
For capacity of receiving aerial .. $\sqrt{\frac{0{\cdot}0018}{0{\cdot}001}}$	$= 1{\cdot}3$
For bad earth sending	2·4
For bad earth receiving	1·2
For continuous-wave sending, beat reception and amplification	0·054
Total product	0·19

We must apply the reciprocal of this to our figure for $\lambda_n h_2\sqrt{P_1}$ to get its equivalent value for application to the curves, giving

$$\lambda_n h_2\sqrt{P_1} = \frac{1}{0{\cdot}19} \times 0{\cdot}3 \times 15\sqrt{25} = 120$$

We are considering a bad earth connection for which we assume [see Section IV (3)] a radiation efficiency of about 8 per cent at 1·5 times the natural wave-length (in this case 450 m.). At this wave-length

$$R_2 = 200\ (30/450)^2 \text{ ohms} = 0{\cdot}9 \text{ ohm}$$

so that the total aerial resistance is 11 ohms. This is a practicable figure entailing no further modification of our value for $\lambda_n h_2 \sqrt{P_1}$, namely 120.

To determine the range given by this for any wave-length we must find by trial a point on the curve for which the abscissa is $120/x$ and the ordinate is x.

Following this method we get :—

(*a*)	Range over flat land on 2 000 m. wave ..	170 km.
(*b*)	Range over hilly land on 2 000 m. wave ..	150 ,,
(*c*)	Range over flat land on 1 000 m. wave ..	270 ,,
(*d*)	Range over hilly land on 1 000 m. wave ..	200 ,,
(*e*)	Range over flat land on 600 m. wave ..	310 ,,
(*f*)	Range over hilly land on 600 m. wave ..	200 ,,

Example (8).—What range by day could be relied on from a ship sending to a shore station, all over sea, where the stations have the following constants ?

Ship.—Inverted L aerial 30 m. high and 60 m. long, sending continuous waves by valve transmission developing 50 watts in the aerial.

Shore.—T aerial 100 m. high and 200 m. long, capacity $0{\cdot}003\,\mu$F. Wave-length 3 000 m.

Here λ_n is about $4{\cdot}5 \times 90$ m. or 0·4 km.

$$\therefore \lambda_n h_2 \sqrt{P_1} = 0{\cdot}4 \times 100\sqrt{50} = 280$$

Modifying factors are :—

For capacity of receiving aerial $\sqrt{\frac{0{\cdot}0018}{0{\cdot}003}}$	$= 0{\cdot}8$
For continuous-wave sending, beat reception and amplification	0·054
Total product	0·043

Applying the reciprocal of this to 280, to get our value of $\lambda_n h_2 \sqrt{P_1}$ for application to the curves, we get $280/0{\cdot}043 = 6\,500$. So we must find a point on the 3 000 m. curve where the abscissa is $6\,500/x$ and the ordinate is x.

This is obtained where $x = 1\,800$ km.

Example (9).—Suggest suitable constants for forward battle sets of 4 km. range, using continuous waves.

Here, from equation (26), the best wave-length for flat land is 32 m. and for hilly land 160 m.

Take 100 m. as generally suitable and apply equation (25), giving K a value between 0·02 for flat and 0·1 for hilly land—say 0·06.

Then we have

$$\lambda_n h_2 \sqrt{P_1} = 0{\cdot}23 \times 0{\cdot}1 \times 4\epsilon^{\frac{0{\cdot}0015\times 4}{\sqrt{0{\cdot}1}} + \frac{0{\cdot}06\times 4^{\frac{1}{2}}}{0{\cdot}1}} = 0{\cdot}246$$

(*a*) Take a single-wire inverted L aerial 1 m. high by 10 m., natural wave-length about 50 m. Then C_2 will be about $0{\cdot}00007\,\mu$F, and modifying factors are :—

For receiving capacity $\sqrt{\frac{0{\cdot}0018}{0{\cdot}00007}}$	$= 5$
For continuous-wave beat reception and amplification	0·054
For bad earths each end	2·88
Total product	0·8

$$\therefore 0{\cdot}05 \times 1 \times \sqrt{P_1} = 0{\cdot}246 \times 0{\cdot}8 = 0{\cdot}2$$

and $$P_1 = 16$$

With our aerial, on $1{\cdot}5\lambda_n$ or 75 m. wave, $R_r = 0{\cdot}14$ ohm, and with our assumption of bad earths we have [see Section IV (3)] $R_1 = 11R_r = 1{\cdot}5$ ohms, which is a practicable figure entailing no modification of our result.

That is, 1 m. by 10 m. aerials, with 16 watts in the aerial, should be suitable, or

(*b*) By using a 2 m. by 20 m. aerial in rear we should have :—

(1) In sending from front to rear h_2 and C_2 are both doubled, so that $\sqrt{P_1}$ may be divided by $2\sqrt{2}$, giving us only 2 watts required in the forward aerial.

(2) In sending from rear to front λ_n is doubled, so that $\sqrt{P_1}$ may be divided by 2, giving us only 4 watts required in the rear aerial.

Discussion before the Wireless Section of The Institution, 15 December, 1920.

Professor G. W. O. Howe : In discussing the paper I shall confine my remarks to one or two points in the early part of it, where, in my opinion, the author makes a serious error in his fundamental assumptions. In developing the simple formulæ on page 158 the author seems to introduce an unnecessary and inaccurate correction. With regard to equation (3), for instance, the author says : " Any aerial may be considered to be the top half of a Hertzian oscillator of length l where $\frac{1}{2}l$ is the height above ground at which the distributed capacity of the aerial may be considered (for our purposes) to act as a concentrated capacity." Then, further on, he says : " Also equation (2) is based on the assumption that a is uniform throughout the oscillator." That is to say, we are using here the ordinary Hertzian oscillator formula, in which it is assumed that the capacity of the end plates is so great that the current throughout the vertical part of the oscillator may be considered uniform, and there is no variation of amplitude along the oscillator, because, if the vertical wire is assumed to have no capacity compared with the capacity of the upper part of the aerial, the current must have the same amplitude all along it. The current cannot change in amplitude if there is no capacity to take the charge. But having made this assumption the author then says : " In an aerial, on account of the variation of current amplitude in accordance with the sine law throughout its length, the R.M.S. current at the base is $\sqrt{2}$ times the R.M.S. value of the effective current throughout the aerial. Therefore, in applying equation (2) to an aerial, we must write $\frac{1}{2}l^2$, for a^2." It seems then that the author has given

up the assumption he made just previously, and that he is now going to assume that the aerial has no upper capacity whatever but is a plain vertical wire in which the current varies from the maximum at the bottom to zero at the top, and he reduces the current to allow for that. One must not make this correction for the current, if one has already reduced the height to its effective value corresponding to a uniform current equal to that at the foot of the antenna, the only place where it can be conveniently measured. I think it is better not to try to correct the current along the aerial if it does vary, but, using the current at the bottom of the aerial, to find an effective height to allow for this decrease in the amplitude of the current as one goes up the aerial. He goes on to say, "With these two modifying factors applied in equation (2) we have, in an aerial . . .," and then he gives equation (4). These two modifying factors are really one and the same factor taken twice over, i.e. two different ways of allowing for the effective height of the antenna. Therefore I cannot accept equations (4) and (5), or any of the equations developed from them. If we have an antenna with a large upper capacity, as compared with the capacity of the vertical wire, then we can assume that the effective height is the full height of the antenna, and if a current is set up in the antenna it will have practically the same amplitude all the way up. In such a case one can apply the ordinary Hertzian oscillator formula. If one regards the vertical wire as a chain of little oscillating dipoles, one can find the strength of the electric field at any point and calculate the total energy radiated. Then the formula for the field strength at the earth's surface is

$$\mathcal{E} = H = \frac{4\pi}{10} \cdot \frac{h}{\lambda} \cdot \frac{I}{x}$$

The power radiated from the antenna is simply half the power that would be radiated from the complete Hertzian oscillator, that is

$$1\,584 \frac{h^2}{\lambda^2} I^2 \text{ watts.}$$

If this is equated to I^2R_r, the equivalent radiation resistance R_r may be found, and the radiation resistance of such a flat-top aerial is obviously $1\,584h^2/\lambda^2$ ohms. In the paper, instead of the effective height h the author uses $\frac{1}{2}l$, taking l as the length of the oscillator. The radiation resistance has really twice the value given by equation (5). In the above equation I is in amperes, and the dimensions in centimetres. If the formula for the electric force at the earth's surface be multiplied by 300 we get it in volts per cm. If then this electric field acts on a receiving antenna of a similar type, i.e. with a large upper capacity, and of effective height h_2, the electromotive force induced in the antenna will be

$$\mathcal{E}h_2 = 120\pi \cdot \frac{h_1 h_2}{\lambda x} \cdot I_1 \text{ volts.}$$

If the antenna has a total resistance of R_2 the received current set up in the antenna will be

$$\frac{120\pi}{R_2} \cdot \frac{h_1 h_2}{\lambda x} \cdot I_1 \text{ amperes.}$$

In Austin's experiment he says that his antenna had a total resistance of 25 ohms. If we put 25 ohms for R_2, our formula becomes

$$15 \cdot 1 \frac{h_1 h_2}{\lambda x} I_1$$

Austin says that from his experiments the numerical coefficient should be 4·25, but in his experiment h_1 and h_2 are the total heights of the antennæ, whereas I have taken them to be the effective heights. Personally I have not the least doubt that, if one made accurate experiments with a perfectly conducting earth and an ideal antenna, one would get results at short ranges in strict accordance with the theoretical formula. In the August number of the *Radio Review* I gave results for the effective heights of various antennæ obtained in a new way, viz. from the curve that is obtained if the total effective resistance of the antenna is plotted against the wave-length. Making assumptions, which I admit are open to some doubt, I calculated from the data that Austin had given for various aerials the ratio of the effective height to the total height. He gives curves for two United States' cruisers, similar to those used in the experiment. In the case of the *Maine* the height is 38·4 metres, and the calculated effective height 17·4 metres. In the *Massachusetts*, the height is given as 39 metres, and the calculated effective height as 24·1 metres. Taking the mean of these two results we find that the effective height is about 0·53 of the total height, which is almost exactly the value required to bring Austin's results into agreement with theory. In dealing with these effective heights, other factors than the aerial and the hull of the ship must be taken into account. When the current rushes up the antenna to charge it, currents will pass in the opposite direction through every vertical piece of metal on the ship. The aerial, together with the masts, funnels, stays, etc., form, to a certain extent, partly-closed loops, and the currents in the masts and every vertical piece of metal on the ship detract from the total radiated power and reduce the apparent effective height of the antenna.

Turning again to the paper, the author, after referring to what he regards as a discrepancy between Austin's results and theory, puts forward a theory which I have already criticized on a previous occasion. It is this, that the energy drained from the surrounding space prevents the proper electromotive force being set up in the receiving antenna. It is, of course, true that the antenna does drain some energy out of the surrounding space, and that this effect must be allowed for. If an antenna has a total resistance (excluding radiation), say, of 10 ohms, and the calculated electromotive force induced in the antenna is taken as 1 micro-volt, one would expect to get a current of one-tenth of a micro-ampere; but that is not the case. Something less than one-tenth will be obtained. This is really due to a back electromotive force in the antenna, but the common assumption made to simplify matters is that, on account of radiation, the resistance of the antenna is increased so that the total resistance is not 10, but, say, 12 ohms. We reduce the received current by adding to the resistance of the antenna a

fictitious amount called the radiation resistance, but that is only a convenient way of taking into account the fact that the current in the antenna itself modifies, as it must do, the electromagnetic field in its neighbourhood. The electromagnetic field is not what it would be, due to the distant station alone; it is modified by the field due to the current in the receiving antenna. One sometimes hears the question: To what distance from the receiving aerial does the drainage of energy extend? The only answer to this is, to infinity, since the antenna may be looked upon as an oscillating circuit coupled up to infinity of space. It should be borne in mind that, in addition to the energy radiated from the vertical wire, which is usually looked upon as the useful output of the antenna, energy is radiated from the horizontal portion. In some aerials of the inverted L type this waste radiation may be quite considerable. The author says: "If, however, it [his theory] is incorrect and V_2 is constant, we are faced with the condition $P_2 \propto 1/R_2$, since $P_2 = V_2^2/R_2$." It does seem, at first sight, to be a paradox that the amount of energy which can be got out of space is independent of the height of the aerial; that one can get just as much energy out of space by the use of an aerial 10 feet high as by the use of one 1 000 feet high. This is so, however, if one condition be fulfilled, viz. that all the resistances are reduced in the same ratio. If that were possible then the same amount of energy would be collected by a small aerial as by a large one. If the electromotive force induced in the aerial is $\mathcal{E}$, multiplied by the height of the receiving aerial, then the current will be $\mathcal{E}h_2/R_2$. Let us assume that the copper and dielectric loss resistance of the antenna can be neglected, so that R_2 is made up of the radiation resistance R_r and the detector resistance R_d; then we get the maximum amount of power in the detector by making $R_d = R_r$. In this case $I_2 = \mathcal{E}h_2/2R_r$, and the power supplied to the detector is

$$I_2^2 R_d = \mathcal{E}^2 h_2^2/4R_r = \frac{\mathcal{E}^2\lambda^2}{4 \times 1\,584}$$

which is independent of the height of the antenna. R_r is proportional to h^2, and it is impossible in practice to reduce the resistance corresponding to the copper and dielectric losses in this ratio, hence the assumed conditions are unobtainable. The author states on page 164 that Austin has published an empirical formula for the capacity of aerials. In the November number of the *Radio Review* I published an article in which I calculated the capacity of a rectangular aerial consisting of a large number of parallel wires at a given height above the ground and obtained a formula very like that given by Austin. It is surprising that Austin, from a number of measurements with aerials, obtained an empirical formula that agrees so well both in form and in numerical results with the theoretically established formula.

Major H. P. T. Lefroy: I am not in a position to criticize the theoretical part of the paper, but there are one or two practical points to which I should like to refer. As regards the transmission side of the paper, I observe that the author has omitted all mention of "multiple" aerials. I have had no practical experience with them, but, quoting from the "Proceedings" of the American Institute of Radio Engineers for August, 1920, I find that the figure there given for the earth resistance is 0·33 ohm with six parallel aerials; presumably if there had been eight parallel aerials that figure would have been reduced still lower. That at once changes the aspect of the examples given at the end of the paper, where the author lays down that it is impossible to get such low earth resistances. I believe that it was impossible until this method of multiple aerials was evolved, but if the article to which I have referred is strictly accurate it will make a considerable change in ideas as regards the total resistance of aerials. For example, for an 8 000-metre wave the article quotes a total resistance of 0·6 ohm, of which the earth connection is 0·33 ohm and the radiation resistance 0·2 ohm. With regard to the question of the environment of an aerial, the author has made no detailed mention of leaky dielectrics, but it seems to me that we should place considerable emphasis on that factor. Consider the case of a wireless station on the sea-shore, in a tropical country, with palm groves around it; under those circumstances one may have an exceedingly good earth and at the same time a considerable total resistance, due to the fact that it has a bad dielectric environment, owing to the palm trees that exist in the neighbourhood. It seems to me that this form of energy dissipation might be dealt with by not including it in the term r_e, as the author does, but by dealing with it as a separate modifying factor, so as to make the analysis more complete. As regards the counterpoise, I have noticed that the marked reduction of aerial capacity due to its use in many cases was owing to the fact that the counterpoise was not large enough in capacity relative to the capacity of the aerial. In tests I have made I have found that the effective capacity of an aerial with a counterpoise is equal to the capacity of the aerial relative to earth in series with the capacity of the counterpoise relative to earth, and, by making the counterpoise of at least 10 times greater capacity, it only slightly reduces the effective capacity of the aerial. As regards the reception side of the paper, it seems to me very necessary that a standard detector should now be evolved in connection with range tests, and for calibration of amplifiers. We require some absolutely definite standard, requiring no adjustment, and which would not vary with position, humidity, temperature or time, to which we could refer our tests. I do not yet know whether it would be practicable to use a Wilson thermopile as a non-rectifying detector, in shunt to the closed circuit receiving condenser, but I believe Mr. Wilson is working on very sensitive high-resistance thermopiles of that nature, which may prove suitable; if so, we could get a more or less exact measurement of the received energy, even of very small quantities, as the heating wire could be calibrated by direct current, and the reading indicated on a standard d.c. microvoltmeter. As regards amplifiers, the time is coming when we shall have to abandon the idea that all amplifiers amplify the interferences as much as the signals. That is the case with high-frequency

"resistance" amplifiers, and with amplifiers for low frequency, but M. Mathieu has produced an amplifier which seems to be exceedingly selective as well as being very intense. The author made a casual reference to the question of night effect. I have worked at many stations in the Near and Middle East where we should have been surprised to receive certain signals during the day, but where we should have been equally surprised not to receive them during the night. These are not freak effects; they are very definite "night" effects; ranges are normally greatly increased at night as compared with the day. As regards atmospherics, they are so very serious at times, within 40 degrees of the Equator, that I do not know what factor could be used to allow for them; they make working absolutely impossible, as the noises in the operators' ears when using amplifiers are so intense that they cannot keep the receivers on; they are practically like a rapid succession of rifle shots fired near by. The author made no mention of the "north and south" effect. It is easier to receive signals over long ranges north and south than east and west, on account of the effect of the sun's rays being, in the former case, uniform all along the path between sending and receiving stations. That is probably a matter chiefly of scientific interest, but in a paper like this it seems desirable to mention it in order to make the paper as complete as possible.

Professor E. W. Marchant: I am very glad to find that the author and Professor Howe, who appeared to be in disagreement on a good many points, really agree with one another. The constant that Professor Howe obtained after his very ingenious calculation exactly agreed with the one that the author gives on page 163, and it was mainly because the explanations of the reason the Austin formula did not agree with the calculated formula, that there was any difference between them. The Austin formula and the formula that the author has given do not give the right values for the received antenna current, except when the aerials are freely exposed, as Major Lefroy has just suggested in connection with stations in the tropics, where there may be an effect, produced by the adjacent palm trees, which causes a reduction in the received antenna current. If aerials are attached to a building with a steel frame structure, the value of the current that is received is very much less than that which is given by the Austin formula. I have found it to work out to something of the order of one-tenth or one-eighth of that given by the Austin formula. Two assumptions may be made as to why that occurs; either that the building alters the effective height of the antenna or that it alters the effective resistance of the antenna. I was very interested to hear what Major Lefroy suggested about tropical stations, because it entirely agrees with what we have observed. [**Major Lefroy:** It was only a hypothetical case of a very good earth and a very bad dielectric.] Austin gave 10 micro-amperes as the smallest current that could be observed on an antenna, as giving just readable signals. We find we can get to a very much lower figure than that. We have easily been able to detect down to 4 micro-amperes with a crystal detector. We have not made measurements with valves, but no doubt we could get much lower still with a valve amplifier. There is one other thing I should like to say, more from the practical side of the question, as to the audibility factor of 64 that the author has referred to; it seems a safe figure, but my experience has been that the greatest trouble one has had in receiving circuits is due to bad contacts on the various parts of the system. This, however, cannot well be taken into account by an audibility factor. With regard to the corrections which the author suggests in the formulæ, that is, the different factors that should be allowed for, I will quote some measurements that we have made on the relative values of the strength of signals that have been received by night and by day. Generally speaking, on what may be called a normal night in the winter, about double the current that is received in the daytime will be obtained in the antenna. In the summer, when there is no true darkness, the change in the strength of the received signals is considerably less. As regards seasonal variations, our experience has been that the normal antenna current received varies a little throughout the year; that is to say, the strength of the current that is obtained in January in daytime is very much the same as that obtained under corresponding conditions in summer. There is just one thing I should like to say with regard to the corrections which the author has not referred to specifically, namely, the effect of the natural wave-length of the receiving antenna on its efficiency as a receiving aerial. I think that is a very important factor. I should also like to lay stress on the great importance which should be attached to reducing the effective resistance of a receiving antenna as much as possible. The author seems to imply that the earth resistance of the receiving antenna does not affect the total amount of energy that can be obtained from the wireless aerial. I do not think that is correct. If the resistance of the receiving aerial be increased, the amount of current that is flowing in the receiving antenna must be reduced, and even under the most favourable conditions one cannot get the same amount of energy out of the antenna as if the aerial resistance were reduced as much as possible.

Professor C. L. Fortescue: The author appears to have had particularly in mind, aerials of a temporary nature as opposed to aerials that are permanent and well constructed with properly arranged earth connections. The table of modifications on page 167 covers, as the author states, a very large number of variables, and, so long as it is remembered that the temporary type of aerial is referred to, these factors are no doubt of considerable value. The mathematical portions of the paper, however, appear rather unconvincing. Professor Howe has drawn attention to the unusual formula for the radiation resistance. This may perhaps be merely a different convention, in that the author refers all resistances to currents $\sqrt{2}$ times greater than those customarily used. With regard to formula (14) for the optimum wave-length, too much reliance should not be placed on the results obtained from it, for two reasons. In the first case it is derived from the Austin-Cohen formula, which is empirical, and in the second

case no allowance is made for the resistance of the sending aerial. The Austin-Cohen formula gives the current in the transmitting aerial to produce a certain field strength at receiving stations at varying distances. The power required at the transmitting station is equal to the square of this current multiplied by the total effective resistance of the transmitting aerial. This effective resistance is made up of the radiation resistance and the wastage resistances. The former is proportional to $1/\lambda^2$, and the latter are roughly proportional to λ. That is to say, the resistance R_1 may be written $R_1 = (r_0/\lambda^2) + r'_0\lambda$, where r_0 and r'_0 are resistance coefficients which are different for different aerials. If the variation of R_1 with λ is allowed for, the absurdity of equation (14) disappears to a large extent. For an aerial of height 120 m. and a wave-length of 6 000 m., r_0 has a value $2{\cdot}28 \times 10^{11}$ (with the usual convention for defining radiation resistance, and with all lengths in centimetres) and, putting $r'_0 = 5 \times 10^{-5}$, corresponding to a wastage resistance of 3·0 ohms, the following table of optimum wave-lengths is given by the Austin-Cohen formula. The results obtained from formula (14) of this paper and from the Fuller empirical formula are also given. It is interesting to note that the Austin-Cohen and Fuller formulæ are in fair agreement, in spite of the apparently wide differences between them.

Distance in km.	500	2 000	10 000
	Optimum Wave-length in Metres		
Austin-Cohen	2 200	3 700	24 000
Fuller	3 100	5 200	12 300
Formula (14)	140	2 250	56 000

The higher the r'_0 coefficient becomes, the more nearly are the results of equation (14) approached. For transmitting aerials radiating on such long wave-lengths that the radiation resistance is negligible compared with the wastage resistance, the Austin-Cohen formula shows that the power necessary should vary as the eighth power of the distance, instead of the sixth power as given by formula (14). The Fuller formula indicates that under the same conditions the required power in the transmitting aerial should vary as the fourth power of the distance. Thus, although both the Austin-Cohen and the Fuller empirical formulæ can be made to represent the observed results with fair accuracy, the conclusions to be derived from them may be very different. Frequent mention is made of earth resistance throughout the paper, and it will have served a very useful purpose in directing attention to this important point in the design of an aerial system. Too little attention has been paid to aerial resistance in the past, presumably because with the old spark system it was of relatively little importance. With continuous-wave working, however, the conditions are different, and the aerial resistance becomes a matter of primary importance.

Mr. L. B. Turner: The subject of the paper is very opportune at this stage in the development of the wireless art, but I think that the formulæ, as they stand, are unreliable, being erected on more than questionable foundations. In the later sections of the paper the author discusses various modifying factors covering effects of hilly ground, the use of amplifiers, and so on. These are admittedly in the nature of guesses, and I do not propose to refer to them; but I must criticize the first three sections of the paper, in which is obtained the main formula for transmitting power P_1 in terms of range x, wave-length λ, natural wave-length of transmitting antenna λ_n, and height of receiving antenna h_2. On page 159 of the paper it is stated that with normally good earthing arrangements the radiation efficiency of an antenna, which may in theory assume any value between zero and unity, may reach 50 per cent, and that this would occur when $\lambda = 1{\cdot}5\lambda_n$. This relation between radiation and total antenna resistance (which may doubtless obtain in some particular cases) is then adopted as of general application, and is woven into the analysis. The author seeks support for the 50 per cent relation in figures (on page 160) taken from Dr. Eccles's Handbook for certain well-known stations, but in doing so, owing to what appears to me a complete misconception expressed in the footnote to page 160, he has halved the antenna resistances quoted by Eccles. Further, he himself throws overboard the 50 per cent relation in various numerical cases (page 160, and Examples 3, 4 and 5 at end of paper). Above $1{\cdot}5\lambda_n$, he assumes the antenna efficiency varies inversely as λ^2, whereas in fact it falls more rapidly as λ increases. (An expression given by Professor Howe, for example, in the article referred to below, contains an important λ^3 term.) These assumptions account for the, I think, erroneous deduction on page 160, that the efficiency of a sending antenna "is not affected by the height, except in so far as an increase in height increases the natural wave-length and therefore the ratio λ_n/λ." It leads to a view of the transmitting antenna which seriously conflicts with that developed in Professor Howe's recent articles in the *Radio Review* (September, 1920), where he points out the enormous importance of using a transmitting antenna with "such small dielectric loss that the minimum total effective resistance is reached at long wave-lengths." I think Captain Trench's assumptions substantially imply that one antenna is as good as another in this respect. In Example 4 (page 168), the author finds from his formulæ that the wave-length and power for 4 000 km. over half sea and half hilly land, with an antenna 1 300 m. long, are 9 000 m. and 74 watts respectively. But as the 50 per cent relation here gives an impracticably low figure (0·2 ohm) for the total antenna resistance, he multiplies by 12·5 as a sort of correction, and so arrives at 900 watts. It is interesting to compare this numerical result with the tables and curves in Professor Howe's article. Taking the same antenna, natural wave-length 3 000 m., wave-length for minimum total resistance, say, 6 000 m., we find that 4 000 km. over sea demands a wave-length of about 6 000 or 7 000 m. and a power of some 120 kW. Howe's

calculations provide 5 times the power in the receiving antenna allowed for by the author, so that we must compare the latter's 900 watts (say 1 kW) with the former's $120/5 = 24$ kW. The discrepancy is uncomfortably wide, since both calculations are based on the Austin-Cohen formula. A second case of a variable being given a particular numerical value and then lost sight of in the formulæ, appears to be that of the receiving antenna resistance R_2. The Austin-Cohen formula for current I_2 in a 25-ohm antenna is used, and, at the top of page 159, $25I_2^2$ is replaced by power P_2. The final result, therefore, refers only to a 25-ohm antenna, which explains why, in equation (13) for transmitter power, the resistance of the receiving antenna does not appear. Actually, the received power is inversely proportional to the antenna resistance, and a term for the latter should, I think, find a place in the formula. With the modern retroactive triode circuits, we are, of course, able to give any value we wish to the total antenna resistance, so that the omission is not, perhaps, of first-rate importance. This brings me to my third and last point. I am glad to see that the author remarks on the existence of an optimum wave-length $\lambda = \frac{1}{4}(0{\cdot}0015x)^2$, which makes the minimum transmitter power proportional to the sixth power of the distance (equation 15). This exposes very clearly the magnitude of the long-range problem, even for the indefinitely slow signalling speeds to which the analysis refers. In these days of long waves and attempts at high speeds of working, we meet the new difficulty that low decrements and high speeds are incompatible; for the epochs of rising and falling amplitude occupy an appreciable fraction of the Morse dot. It can be shown that, for high-speed long-wave signalling, the corresponding optimum λ becomes a quarter of that given by the author for indefinitely low speeds, and that the requisite transmitter power ultimately varies as the tenth power of the distance—an enormous rate.

Major A. C. Fuller: If it is in any way possible by the author's methods to forecast results with reasonable accuracy in the way that appears so simple in the examples given, then the paper has done us a great service. Possibly the remarks of other speakers will help us to form a true estimate of the author's conclusions; one's faith in them has been rather shaken by the discussion this evening. I feel confident that at least some of the modifying factors will themselves have to be severely modified before they are of practical use, particularly that which the author allows for amplification. I am rather doubtful whether the author realizes the capabilities of a modern tuned high-frequency amplifier. Again, he does not appear to differentiate between high-frequency and low-frequency amplification. With tuned high-frequency amplification we can go a very long way towards bringing up the required signal without increasing much the strength of others. Therefore that factor at least will require further modification. The paper is of value, inasmuch as it tackles and ventilates a big subject, and gives us a chance of hearing other views and estimating its true worth after it has been subject to criticism.

Dr. E. H. Rayner: A considerable amount of experience has been obtained in the Services in the last few years on the amount of power, or perhaps it would be more correct to say nominal power, required for various distances. I have been informed that a good working rule over quite a considerable range, say 20 miles to 500 miles, is about one watt per mile. It would be interesting to know how the author's deductions agree with this rule-of-thumb figure. Incidentally it shows how closely the physics of electric wave propagation corresponds to a two-dimensional, rather than a three-dimensional, phenomenon, at least for moderate distances.

Dr. W. H. Eccles: Looking at the paper as a whole we may say that the principle adopted by the author is to take the Austin-Cohen formula, which was developed for a certain pair of antenna, to enumerate the important variables at each end, and to show us how those variables influence the extension of the formula universally. In doing this there are many possible schemes; the author's is a very well-considered one, in my opinion, and gives a number of results which are now open to the test of experiment. I do not think the author will insist that they are final; they are subject to modification, as experiment may show. The Austin-Cohen formula is a very remarkable guiding law that has been partially corroborated by theory, and is therefore likely to be fairly trustworthy and in fairly correct mathematical form. But that formula has caused a great deal of fog. People do not always remember what it really does. In the first place, it allows for the convexity of the globe; in the second place, it allows for the Hertzian law of radiation from an oscillator of any height; in the third place, it includes the laws of reception of radiation by the receiver, whatever its height and wave-length, within the limits of the Austin experiments. Therefore, it must not be "corrected" by adding to it any consideration such as re-radiation. That is contained in it; re-radiation could not be left out of the experiments, whether it was much or little. Doubtless it is not correct to assume that the antenna resistance is 25 ohms at all wave-lengths, but, in fact, the variation of antenna resistance is small relative to other variations in the trials. Professor Fortescue's point about not differentiating an empirical formula is a well-known point in applied mathematics, and is important in cases where there are likely to be undiscovered discontinuities. The mathematical proof that followed the empirical discovery of the Austin-Cohen formula shows that there are no discontinuities, and, indeed, ordinary physical considerations show that there could not be any discontinuities within a reasonable range of application of the formula.

Mr. J. E. Taylor (*communicated*): The paper materially supplements existing knowledge on the subject and sheds some light on that elusive quantity which is known as "range" of wireless stations. In regard to differences in propagation efficiency as between land and sea, I have for long felt that differences in surface conductivity are considered too much in this connection, and I do not know that this paper is any exception in that respect. It has always seemed to me that the conductivity will not, in general, produce

any appreciable effect as compared with other causes of attenuation, such as large irregularities of the surface giving rise to reflections of wave energy, because, as the distance from the transmitter increases, the waves are, in effect, passing over a conductor of enormous diameter with consequent small total resistance, even though the specific resistance may not be small and the current may be confined to a limited depth of penetration. The radially-directed current leaving the base of the transmitting antenna will, as it expands, preserve sensibly the same current flux, except for such other causes as the reflections referred to. Hence, at a considerable distance the I^2R losses will become vanishingly small, I being the total current flux across a belt or annulus of ground, and R the resistance of that annulus. I may remain constant while R diminishes with distance, so that the percentage loss of energy due to resistance becomes progressively smaller. In regard to transmission over hills and mountain ranges, I think it is open to doubt whether diffraction allowances will give correct results. I imagine it will be found that propagation in excess of that calculated will frequently occur, seeing that certain experimental results have apparently indicated that there is a considerable difference between day and night propagation, even at quite short distances, when a mountain range intervenes. This, however, is one of the numerous points on which more experimental work is desirable. On the question of the calculation of energy received I am decidedly in agreement with the author in regard to the important function of the receiving antenna in draining energy from the surrounding space. I think we shall have to discard the idea of E.M.F. induced by magnetic lines cutting the antenna. It is not, in my view, a helpful conception. The author has brought out several points which have hitherto been but scantily recognized, among which may be mentioned the consideration that, of the energy developed in the receiver, only 50 per cent is the maximum utilizable. I must, however, join issue with the author in regard to his footnote in Section IV, where he assumes heavy ground losses to occur beneath the counterpoise of an antenna. Surely the counterpoise should have the effect of practically screening the ground beneath from any appreciable electrical action.

Captain R. C. Trench (*in reply, communicated*): The volume and vigour of the discussion are a measure of the importance and interest of the subject, while the fact that in many cases the same point is attacked by one authority and defended by another shows the necessity for thorough ventilation of the various aspects of the problem. Many of the criticisms are best answered by pointing to the severely practical nature of the problem that I set myself as an engineer, which may be stated as follows: "In the present stage of development, on what must I base my specification of the wireless equipment necessary for any range?" It is a question that must be answered in one way or another by every engineer responsible for establishing wireless communication between two points, and the paper examines the data available and attempts to show the general conclusions to which they lead. Some such reasoned basis of calculation, however incomplete, is a step towards replacing the present too common methods of guessing by rule of thumb and precedent. It necessarily contains a number of guesses and rules of thumb in itself, which are due for revision in the light of progress, but it is claimed that these are superimposed on a reasoned basis which is susceptible to proof and which provides the framework of the argument. No finality is claimed or even possible, whereas some critics have attacked aspects of the paper as if it pretended to ultimate truth.

Professor Howe first points out that, in considering the aerial as the top half of a dipole which would radiate the same energy, I have not only corrected the height of the aerial to its effective height to centre of capacity, but have also corrected the base current I to a value $I/\sqrt{2}$ as an allowance for current variation up the aerial. He points out quite rightly that this correction for current is wrong, and that having considered the aerial as a lumped capacity at its effective height joined to earth by a conductor of no capacity, I must take the uncorrected base current I to be flowing in that conductor. That is, the values of P_r and R_r in equations (4) and (5) must be multiplied by 2, and all values of $\lambda_n h_2 \sqrt{P_1}$ obtained from the paper must be multiplied by $\sqrt{2}$. My thanks are due to Professor Howe for pointing out this mistake, for which the correction entailed on the results given in the paper is fortunately simple.

The formula given by Professor Howe for current received under conditions of no absorption due to the earth's resistance and curvature is the same as equation (21) in the paper. We agree therefore on the extent of the disparity between this numerical coefficient of 15·1 and the corresponding figure of 4·25 in the Austin-Cohen formula, but we account for it differently. Professor Howe maintains that if the actual heights h_1 and h_2 in the Austin tests could be corrected to their true effective values, the disparity would disappear and the coefficient 4·25 would become 15·1; in other words, the effective heights in those tests were only $\sqrt{(4{\cdot}25 \div 15{\cdot}1)} = 0{\cdot}53$ times the actual heights, in spite of the large elevated areas. Although much of the discrepancy must be accounted for by the difference between actual and effective heights, I find it hard to accept it as the sole factor, for the following reasons. Professor Howe's equation and equation (21) depend on the assumption that the voltage V_2 in the aerial is maintained at the level of the electric force in the wave-field, and depends only on the field intensity and the aerial height. That is, for an aerial of fixed height the power received varies inversely with the resistance and is affected by nothing else, since $P_2 = V_2^2/R_2$. I find this difficult to accept in the face of experience, and I submit on the other hand that the power received by an aerial of fixed height varies only slightly with the resistance and to a much greater extent with the capacity of the aerial. In support of the contention that received power varies only slightly with aerial resistance, I will quote the following extract from a report of tests in 1920 by Captain C. T. Hughes of the Signals Experimental Establishment, Woolwich: "The moving of the aerial system from a position in marshy land on to sand and finally

on to chalk hills caused no appreciable variation in signals received from a transmitter with constant input, although my own transmitter aerial current fell from 1 ampere to the merest flicker with the chalk earth." It is clear that the resistance of the moving aerial (except that proportion due to the coupled receiver) must have been multiplied many times to produce such a fall in transmission, while reception was not appreciably affected by it. In support of the contention that received power varies with aerial capacity, I refer to page 164 of the paper where an experiment by D. Darrin is described, in which the power received by an umbrella aerial with a varying number of spokes increased nearly proportionately with the aerial capacity.

The above considerations and my own experience have led to my working assumption for the practical purposes of the paper, namely that the power drained by an aerial from surrounding space depends mainly on its height and capacity, and comparatively little on its resistance. Up to 50 per cent of this power collected can then be delivered to a coupled receiver circuit in whatever proportion of current to voltage is most suitable, and subjected to any form of amplification. Mr. J. E. Taylor appears to support me in general over this. In regard to the statement which he attributes to the paper that the received energy is independent of the height, Professor Howe misunderstood my meaning, which was that if my theory is wrong and if V_2 is constant, we are faced with the condition, in an aerial of fixed height, that $P_2 \propto 1/R_2$, since $P_2 = (V_2)^2/R_2$. I am unable to accept this condition, for the reasons given above.

In reply to Major Lefroy there seems no doubt that the use of multiple aerials to which he refers would impart a much higher radiation efficiency than is contemplated in the paper, and where these are used a very simple modifying factor can be introduced. The excessive size of a multiple aerial required for any given natural wave-length militates against their general use, since the single aerial must be repeated in the same proportion that it is required to reduce the resistance of the parallel earths. Major Lefroy suggests a separate factor to allow for imperfect dielectric, but owing to the need for simplicity in such a paper, I think it better to include such a resistance in the term R_e. I am entirely in agreement with all his remarks about counterpoise aerials. The production of the standard detector which he suggests would be invaluable as a means of classifying and comparing receiving devices, and it would be of the greatest service if he could produce one. By its means we could agree upon a formula for the "figure of merit" of any receiver depending on its unit audibility and selectivity, and this figure of merit could be applied as a modifying factor to any range formula or curves in use. As regards amplification, I agree that the time is coming when we may abandon the idea that interference is amplified as much as the signals. My modifying factor of 0·3 for valve amplification was suggested a year ago as a working figure for application in practical signalling; it is obviously subject to progressive improvement, and anyone with sufficient confidence in the selectivity of his receiver can substitute for it any value he pleases down to the minimum $[\sqrt{(U/600)}]$, where U is unit audibility in micro-microwatts. I agree with what Major Lefroy says about night effect, North and South effect and the effect of atmospherics in the tropics. My own experience with the latter is that they begin suddenly at fairly regular daily hours, continue with such intensity that work is utterly out of the question, and then stop as suddenly and as regularly as they began. No margin of power would work through them, and I have had in practice to recognize the fact that communication must be abandoned for certain times.

Professor Marchant draws attention to the very serious adverse effect of screening by neighbouring conductors such as steel-frame buildings, rigging, funnels, etc., due to either reduction of effective height or increase in effective resistance or both. The omission of this point from the paper is one of its many shortcomings; the matter must be faced, and the best approximate method for practical engineering is perhaps the following. Take the earth's surface immediately under the aerial and extending as far as half the aerial height all round, with an area of, say, N square metres. If this contains a raised earthed conductor of height H, estimate the area of its top surface, say N_1 square metres, and of its sides say N_2 square metres, and work out the height of the centre of gravity of $(N - N_1)$ units at ground level, N_1 units at height H, and $\frac{1}{2}N_2$ units at height $\frac{1}{2}H$; if there is more than one raised conductor they can be included similarly. This height may be taken as the mean earth level for the aerial, and the effective height taken from this level to the centre of capacity. Measure the decrement δ of the aerial by any recognized method, at $1{\cdot}5\lambda_n$. Then the total resistance R_1 is given by $R_1 = 2\delta f L$. The radiation resistance R_r is $1\,580\ (\frac{1}{2}l)^2/\lambda^2$, where $\frac{1}{2}l$ is the effective height obtained as above, and from this the radiation efficiency can be obtained. Having thus found the radiation efficiency at $1{\cdot}5\lambda_n$ and the effective height, we can modify accordingly the results given by the curves.

In regard to the effect of the natural wave-length of a receiving aerial on receiving efficiency, an aerial can be loaded above its natural wave-length for reception to an extent that would be fatal for transmission, without apparent detriment. A small aerial will, of course, receive long waves less strongly than a large one, but it is considered that all necessary practical allowance for this is made in the modification for aerial capacity in reception. Professor Marchant says that I seem to imply that the earth resistance of a receiving antenna does not affect the total amount of energy that can be obtained from it. That is so, my point being that what matters is the amount of power in the receiving aerial, and not the proportion of voltage to current in which that power is manifested. Once the power is there, we can modify it by coupling to any form we need. It will be seen from the paper that allowance is made for the adverse effect of a bad receiving earth on the power received, but this effect is much less than that of a bad sending earth.

In reply to Professor Fortescue I did not mean my paper to apply chiefly to temporary aerials as he suggests, but possibly my experience has given me an un-

conscious bias in that direction. I am sorry to find he does not think that the Austin-Cohen formula justifies the differentiation which gives equation (14) for the optimum wave-length, since if he were right it would affect the foundation of the paper. The case for accepting the Austin-Cohen formula as I have done, has been so ably put by Dr. Eccles in his remarks that I cannot do better than refer to them without further amplification on my part.

Mr. Turner says that I have taken a particular case of 50 per cent radiation efficiency at 1·5 times the natural wave-length, and then woven this into the analysis as if it were of general application. To reply to this I must point to the scheme of the paper. Certain conditions are first defined, as a starting point, and equations and curves worked out to apply to those conditions. One of these conditions is defined in section 1 (6) as giving a radiation efficiency of 50 per cent at 1·5 times λ_n, and it is shown later on how to modify results to suit any other degree of radiation efficiency. Examples 3, 4 and 5 to which Mr. Turner refers are merely cases in point, where this radiation efficiency does not apply and results are modified accordingly. He says, correctly, that the resistance of the well-known stations referred to should be twice what I have said. This is an error on my part, but it merely means that in applying the curves to these stations we should make allowance for lower radiation efficiency in the way shown in the paper. Mr. Turner further says that I assume radiation efficiency to vary inversely as λ^2, whereas in fact it falls more rapidly as λ increases. It is true that one of the working approximations made in discussing radiation efficiency is to ignore the increase in the total resistance R_1 on the longer waves. My grounds for this are that a study of many typical aerial resistance curves shows that, at the limit of wave-length to which an aerial is likely to be loaded for transmission, say 4 times the natural, the rise in resistance is only just beginning, and it is both simpler and more accurate to take no account of it below this limit. If we take the efficiency to vary inversely with λ^3 in sending with waves longer than this, the range formula becomes

$$\lambda_n^{\frac{3}{2}} h_2 \sqrt{P_1} = 0 \cdot 23 \lambda^{\frac{3}{2}} x \epsilon^{0 \cdot 0015 x \setminus \lambda}$$

instead of the simpler form given in equation (13) for more normal conditions. This does not affect my contention that the radiation efficiency of a sending aerial with a given earth depends ultimately on the ratio of the wave-length used to the natural wave-length, and is not affected by the height except in so far as an increase in height improves this ratio. (This is qualified in the paper for the case of long low aerials whose very low radiation resistance imposes an unavoidably low efficiency; that is, when the length exceeds about four times the height.)

Mr. Turner compares my result in Example 4 with the result given by Professor Howe for similar conditions. This example shows that for a 4 000-km. range on a $\frac{3}{4}$ 9 000-m. wave we want about 1 kW in aerials 50 m. high by 600 m. long. Mr. Turner finds from Professor Howe's article that the same aerial on a 6 000-m. wave (not the most efficient) requires 24 kW for the same reception strength. I have acknowledged a mistake pointed out by Professor Howe, the correction for which raises my power to 2 kW; now 2 kW in an aerial whose resistance I have taken in the example to be 2·5 ohms gives a current of 28 amperes, and I leave it to the verdict of general experience to say whether 28 amperes arc transmission in directed aerials 50 m. by 600 m., on a 9 000-m. wave, would not give easy communication over 4 000 km., with modern valve reception and amplification.

Mr. Turner notes quite rightly that I have made equation (13) on page 161 and the curves to fit the case of a receiving aerial of 25 ohms' resistance and have allowed no correction for different resistances (though I have made a general allowance for receiving with a bad earth). This is because, as already explained in my reply to Professor Howe, I believe the power received to depend mainly on height and capacity and to a minor degree on resistance. As Mr. Turner points out himself in his "Outline of Wireless" (ch. x, sec. 1) the aerial resistance can be reduced to any extent in theory (with certain limitations imposed by practical conditions) by the use of retro-active amplification. If the power received varied inversely as resistance and the latter can be reduced indefinitely we rapidly find ourselves outside the realm of reality. I have therefore thought it best, for the practical purposes of the paper, to ignore all allowance for R_2 because it is not important in practice compared with the other determinants, and its value with any one receiver varies so much with the adjustments of the moment that no practical method suggests itself which appears better than the one followed in the paper.

Finally Mr. Turner remarks on the new (and I think unforeseen) difficulties introduced by high-speed work on long waves, which force us to the use of much shorter waves and consequent great increase of power when working at high speeds. This is a point of increasing importance and demands a section to itself to reduce the problem to manageable form for practical application.

Major Fuller thinks I have not given enough credit to the selectivity of modern amplifiers. This is quite probable, as the paper is a year old, but, as explained in my reply to Major Lefroy, the point is one for which progressive allowance can be made.

Dr. Rayner suggests a rough rule of 1 watt per mile for ranges between 20 and 500 miles, but even for short ranges, before attenuation due to the earth's resistance and curvature begins to be felt, we have $P_1 \propto x^2$ for aerials of fixed height, so that if he wishes to make $P_1 \propto x$ he must introduce a second variable for height. Such a rule might then be of use for short ranges, but is of course no use as soon as attenuation due to the earth's resistance and curvature begins to make itself felt, and I think 200 miles is a safer practical limit than 500. If a rough rule of this sort is wanted, I do not think the form given in equation (16) can be further simplified with any advantage.

Dr. Eccles's sympathetic understanding of the true scope and nature of the paper has been of great encouragement to me, particularly his acceptance of the

general scheme. As he points out, the results are in a flexible form suitable for modification with progress and claiming no finality. His exposition of the true nature of the Austin-Cohen formula, which appears to be misunderstood by some speakers, should help investigators to avoid confusing the issue with unhelpful complications ; the chief difficulty is to attain to clear thinking, facing the essential and rejecting the superfluous, and this I consider Dr. Eccles's remarks help one to do.

I am not qualified to comment on Mr. Taylor's opinion of the small effect of surface conductivity on attenuation, compared with other causes such as surface irregularity. I am glad to find he appears to agree with what is perhaps the most controversial point raised in the paper, i.e. the E.M.F. in reception. With regard to his point that there are no ground losses under a counterpoise because the counterpoise wires should screen the ground beneath from electrical action, I think there are heavy ground losses due to induction from the currents in the counterpoise itself. If there are not, the only two important resistances in a well-insulated aerial with counterpoise are (*a*) radiation resistance and (*b*) ohmic resistance of conductors, as opposed to an earthed aerial which has (*a*) and (*b*) and in addition the resistance due to earth losses. In practice, the resistance of a counterpoise aerial is much greater than could be accounted for by (*a*) and (*b*), generally more than twice as great, and the balance must be due to some form of earth losses.

Finally, I must thank those who have pointed out aspects of the problem that will help towards its satisfactory solution. If it is conceded, first, that the expression $\lambda_n h_2 \sqrt{P_1}$ embodies the principal determinants of range at any given wave-length, and secondly, that equations (13) and (25) and the curves based on them illustrate well enough the process of attenuation, then the paper has served its purpose ; the determination of the numerical coefficient given as $0 \cdot 23$, and the discussion of variations on the main theme are of secondary importance. The touchstone for the practical value of the paper to an engineer lies in the examples at the end, and it is for the critic to say whether these results (modified by Professor Howe's correction) are in accord with sound engineering practice. I think that they are.

Discussion before the Scottish Centre, at Edinburgh, 11 January, 1921.

Mr. W. B. Hird : The method of starting on a subject of this kind by obtaining formulæ and equations from the purely theoretical and mathematical point of view, and substituting in these equations the necessary coefficients, not from a concrete example or experiment but from what the author calls personal experience, is one which I have so often found useful in other ways that it seems very suitable when dealing with this subject. I should think that those who have to deal with it practically, to design stations and improve apparatus, will find the paper of great use and the formulæ of practical assistance.

Mr. N. Wells : The author emphasizes the value of earthing for transmission aerials, and certainly in practice this is a most important matter. The laying of a multitude of wires and the burying of a large number of earth plates is a laborious task and one difficult to supervise efficiently, yet it matters little how wisely and elaborately the earth system is planned if the connections are not carefully watched and unless reliable men are put on the job. When convenient, connections should always be sweated and not bolted, and where there is risk of erosion or corrosion the joints to plates should be protected with clay before the ground is filled in. I am glad that the author has a good word to say for the Marconi form of aerial ; doubt has been expressed in some quarters as to the policy of the Company in adhering to this type, yet, as pointed out in the paper, absolute height is by no means the criterion of radiating efficiency. A comparatively low aerial suitably designed as to form, capacity, earth balance and primary machinery will radiate equally as well as a tall aerial and, as the total installation cost is lower, the matter is of considerable importance to the customer as well as being one of scientific interest. Another point is that if the aerial is kept low the electric strain on the insulators is minimized, and I might add that for high power transmission this has become a serious problem ; in other words, with increasing power the low aerial becomes more reliable under actual working conditions. The same arguments do not apply in the case of receiving aerials. At the transmitting end there is probably a certain amount of wave distortion from the emission of a low aerial, but after travelling some distance the wave-form becomes normal and, I suggest, tends to keep normal despite the effect of ground resistance, so that at the receiving end an aerial that will respond readily to a normal wave-front probably makes the most efficient collector of energy ; that is, for all-round reception a plane vertical aerial, as near a simple oscillator as possible, is appreciably more efficient, and height becomes important as long as it bears the correct relationship to wave-length. But when dealing with directive reception, which for land work is often more important, there is a different problem ; I think that the inverted L aerial owes its directive properties to some phase relationship between current in the main vertical element and the displacement about the centre of capacity of the complete aerial, giving rise to the well-known polar curve of signal strengths as a result of virtual reflection. If this is so it is obvious that the vertical member, as main oscillator, must be in front of the centre of capacity and in line, for both transmission and reception. Here horizontal

length, in relation to wave-length, becomes a valuable feature, and a comparatively low aerial erected at less outlay will give the same results as a high aerial, and in addition be considerably freer from jamming. Generally speaking, when dealing with long waves and high power, it is obvious from practical considerations and limitations that for transmission the Marconi aerial is designed mainly as an efficient radiator more than for its directive properties, whereas for reception a single wire can be carried at a reasonable height over a considerable horizontal distance, thus taking full advantage of directional effect.

RESEARCH ON THE HEATING OF BURIED CABLES.

PRELIMINARY REPORT OF WORK ALREADY COMPLETED AND OF WORK CONTEMPLATED.

(Report received from The British Electrical and Allied Industries Research Association.)

(*Read before* The Institution 16 *December, before the* North-Western Centre 7 *December, before the* North-Eastern Centre 13 *December, before the* North Midland Centre 14 *December*, 1920, *before the* Tees-side Sub-Centre 3 *February, and before the* Liverpool Sub-Centre 7 *February*, 1921.)

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INTRODUCTION.

The investigation was undertaken to provide definite values of current-carrying capacity for cables laid underground in the conditions prevailing in practice in this country. As a first step, a summary of all the hitherto published work was prepared and published in the *Journal* of the Institution of Electrical Engineers, 1914, vol. 52, p. 779. The American and German mathematical work was fairly complete so far as cables laid direct in the ground were concerned, but it appeared that certain factors which had been assumed in drawing up the German tables required redetermination and, further, there was no very definite information regarding cables laid either solid in bitumen or drawn into ducts.

The research was, therefore, directed primarily to determination of the necessary thermal constants and of the relative heating of cables laid under the three main systems. During its progress, however, and as a result of further suggestions, various other important points arising from the work in hand, such as the large variation of thermal resistivity in different cables, methods of measurement, etc., dielectric losses in high-pressure three-core cables, and the safe working temperature limit, have been wholly or partly explored.

The investigation has been carried on under two Committees of the Institution of Electrical Engineers, namely, the original Buried Cables Sub-Committee and the North-East Coast Sub-Committee on three-core cables, and in three places, at :—

(1) The National Physical Laboratory,
(2) Newcastle-upon-Tyne Electric Supply Company's test-room,
(3) Liverpool University.

The work of the two Sub-Committees has now been transferred to the British Electrical and Allied Industries Research Association.

A number of cables have been available for the work. Some of these have been purchased, but the greater part, and especially the more expensive cables, have been provided by the Cable Makers' Association and by various firms or supply authorities, such as the County of London Electric Supply Co., the Glasgow Corporation, the Newcastle-upon-Tyne Electric Supply Co., and the Liverpool Corporation.

List of Members of the Conductors Sectional Committee of the British Electrical and Allied Industries Research Association (*May* 1920).

Mr. C. Vernier (*Chairman*).
Mr. H. B. Atkinson.
Mr. A. S. Barnes.
Mr. J. R. Beard.
Mr. G. L. Black.
Sir T. O. Callender.
Mr. J. D. Dallas.
Mr. E. Fawssett.
Mr. J. S. Highfield.
Prof. E. W. Marchant, D.Sc.
Mr. S. W. Melsom.
Mr. Evan Parry.
Sir J. E. Petavel, F.R.S., K.B.E.
Mr. S. A. Pollock.
Mr. H. F. Proctor.
Mr. P. Rosling.
Mr. B. Welbourn.
Mr. C. H. Wordingham, C.B.E. (*ex-officio*).

Director of Research.

Mr. E. B. Wedmore.

List of Members who served on the Buried Cables Research Sub-Committee of the Institution of Electrical Engineers.

The late Mr. W. Duddell, F.R.S.*
Mr. J. S. Highfield (*Chairman*).
Mr. A. S. Barnes.
Mr. G. L. Black.
Sir T. O. Callender.
Mr. J. D. Dallas.
Sir R. T. Glazebrook, F.R.S.
Prof. E. W. Marchant, D.Sc.
Mr. S. W. Melsom.
Mr. R. B. Mitchell.
Mr. H. F. Proctor.
Mr. C. H. Smyth.
Mr. C. P. Sparks, C.B.E.
Mr. C. Vernier.

List of Members of the North-East Coast Buried Cables Research Sub-Committee of the Institution of Electrical Engineers.

Mr. C. Vernier (*Chairman*).
Mr. J. R. Beard.
Mr. E. Fawssett.
Mr. S. W. Melsom.
Mr. J. F. Watson.
Mr. B. Welbourn.

* Mr. Duddell was the first chairman of the Sub-Committee.

Work Still Outstanding.

A number of points on which further information is desired have been dealt with in the Report. They may, however, be summarized here.

(1) Before a table of permissible currents for cables laid under various conditions can be prepared, it is essential that the Association should settle upon (*a*) the permissible temperature-rise, (*b*) the normal maximum temperature of the soil, (*c*) the thermal resistivity of the soil, (*d*) the thermal resistivity of the cables on which such table should be based.

(2) The largest cable available should be laid in order to confirm, or otherwise, the facts already obtained for different conditions of laying.

(3) The question of the relative rating of high- and low-pressure cables requires to be investigated further. It is possible that the information already available is not sufficient for the purpose of calculation and that further tests should be made with short lengths of other sizes of cable.

(4) This Report deals only with cable laid singly in the ground. Tests are proceeding to obtain information as to the effect of running two or more cables in the same troughing, up to six cables drawn into a duct, and with neighbouring cables laid direct in the ground.

(5) The factors and curves given in the Report are based on continuous running conditions. It is desirable to show the effect of loading for some hours only each day, as may occur in practice. Several results have already been obtained on existing cables in supply undertakings, but these need to be checked by comparative tests on the experimental cables.

(6) The amount of moisture in the soil is to be determined in three towns, over long periods.

(7) The application of the general results requires investigation in connection with the results obtained by Mr. Fawssett (see Section II) with cables laid at different depths. The assumption of the shape of the isothermal surfaces surrounding the cable requires further experimental evidence. A certain amount of work on this matter has already been done by Mr. Fawssett.

(8) Probably one of the most important matters is the question of the maximum temperature to which a cable may be subjected for an indefinite period without deterioration, due either to the high temperature or to excessive losses in the dielectric. Arrangements have been made (*a*) for the dielectric losses to be determined if possible both with the whole cable raised to a given temperature and also with the cable heated by the current through the cores, that is, with the actual temperature gradient in the insulation that would occur in practice, and (*b*) for samples of high-pressure cables to be subjected to high temperature combined with high voltage, samples of the paper being removed from time to time for examination and tensile-strength tests.

SECTION I.

REPORT, BY MR. S. W. MELSOM AND MISS V. COCKBURN, ON WORK AT THE NATIONAL PHYSICAL LABORATORY ON PAPER-INSULATED CABLES LAID UNDER VARIOUS CONDITIONS.

Schedule of Cables Used in the Investigation.

A full list of the cables examined and their dimensions is given in Table 1, Schedules 1 and 2.

Methods of Test.

The method used to determine the temperature-rise was to use the cable core as a resistance thermometer, the change in total resistance as measured by a Kelvin double bridge giving the average temperature-rise of the hottest part of the cable to a high degree of accuracy. A similar method was used to determine the temperature-rise of the lead sheath.

The use of these methods involved an accurate determination of the temperature coefficient of both lead and copper, and, for the copper, samples were taken from the conductor of each cable and the resistivity and temperature coefficient were determined. In all, some 47 samples of copper were tested for temperature coefficient, the resultant values varying from 0·00377 to 0·00407 for 1 degree C., a maximum variation between any two samples of 8 per cent. For the purpose of the tests, the average figure for the whole of the samples of 0·00390 at 20° C. was used. In addition, some 230 samples of copper were tested for resistivity, the extreme values being 0·1513 ohm per metre-gramme (101·2 per cent conductivity), and 0·1588 ohm per metre-gramme (96·5 per cent conductivity). In these cases, however, the results for each cable were corrected for any variation of resistance. The value of temperature coefficient obtained by Dr. Marchant (see Section III) is 0·00409 at 10° C. (0·00393 at 20° C.).

For the determination of the temperature coefficient of the lead sheath, 21 samples taken from different cables were tested, the average temperature coefficient at 20° C. being 0·00385 for 1 degree C. The highest value obtained was 0·00404, and the lowest 0·00373. The resistivity of the samples of lead was determined, the average value being 2·443 ohms per metre-gramme, equal to a relative mass conductivity of 6·2 per cent. The extreme difference between any two samples was 6·9 per cent, while in most cases the samples taken from the same cable agreed to within ± 1 per cent. It will be seen that from the point of view of consistency, both of resistivity and of temperature coefficient, the lead sheath is nearly, if not quite, as consistent as the copper core; moreover, observations taken on the complete length as laid shows that the resistance remains constant over long periods. In view of this, the method of determining the temperature of the lead sheath will be of considerable use in practice to

ascertain the approximate temperature of a cable already laid, and especially where it is impracticable to use the cores.

Tests of Cables in Air.

In order to provide an accurate comparison of the cables under similar conditions, and to investigate the effect of burying them in the ground under various conditions of laying, a preliminary series of tests was made on all of the cables when laid out along the wood-block floor of a room designed for constancy, evenness of temperature, and freedom from draughts. Also, from these tests the thermal constants of the various cables were determined and other points, such as the effect of armouring and colour of surfaces, were wholly or partially explored. The values obtained are given in Table 2 in the form of the current required to produce a temperature-rise of 50 degrees F. in the cables when so laid, and also in Figs. 1 to 4. In this Table the values for the single-core cables refer to conditions when two cables are laid close to, and touching each other. The values of thermal resistivity are given in Tables 7 and 3.

Figs. 1 to 4 show the rate of heating and the relative temperature of the cores and the lead sheath in four sizes of cable when laid in air under the foregoing conditions. It will be noted that the 0·1 sq. in. low-pressure single cable attains to its maximum temperature in from 2 to 3 hours, while the much larger cables (0·15 sq. in., three-core, E.H.P.) require about 8 hours. In Fig. 5, the time required for cables of various sizes to attain to maximum temperature is shown. It is obvious that, apart from the specific heats of the constituents of the cable, these values will depend both on the size of the conductors and on the thickness of the dielectric. In the curve the total cross-section of the conductors in the cable in square inches, multiplied by the overall diameter (inches) of

Table 1.—Schedule 1.

Paper-insulated Lead-covered Cables.

Cable No.	Sectional Area		Remarks	Pressure	Approx. Length	Armoured	Tested		Age
	Nominal	Actual					In Air	In Soil	
(1)	(2)		(3)	(4)	(5)	(6)	(7)		(8)
	sq. in.	sq. in.	*Single Cables.*		ft.				
1	0·1	0·094	Single	L.P.	110	No	Yes	Yes	New
2	0·2	0·196	Single	L.P.	110	No	Yes	Yes	New
			Concentric Cables.						
3	0·1	0·108	Concentric	L.P.	110	Yes	Yes	Yes	New
4	0·1	0·099	Concentric	L.P.	110	No	Yes	Yes	New
5	0·1	0·100	Concentric	L.P.	230	No	Yes	Yes	Old
6	0·2	0·192	Concentric	L.P.	110	No	Yes	Yes	New
7	0·2	0·196	Concentric	L.P.	120	No	Yes	Yes	New
8	0·2	0·197	Concentric, jute insulated	L.P.	110	(Jute covered)	Yes	Yes	Old
9	0·2	0·205	Concentric	L.P.	110	Yes	Yes	Yes	New
10	0·5	0·480	Concentric	L.P.	110	No	Yes	Yes	New
			Three-core Cables.						
11	0·025	0·029	3-Core	L.P.	30	No	Yes	No	—
12	0·05	0·048	3-Core, shaped cores	10 000 V	51	Yes	Yes	No	New
13	0·1	0·105	3-Core	6 000 V	99	No	Yes	Yes	—
14	0·1	0·100	3-Core, shaped cores	L.P.	114	No	Yes	Yes	—
15	0·1	0·101	3-Core, shaped cores	6 000 V	30	Yes	Yes	No	—
16	0·15	0·150	3-Core, shaped cores	6 000 V	30	Yes	Yes	No	New
17	0·15	0·145	3-Core	E.H.P.*	50	Yes	Yes	No	New
18	0·15	0·145	3-Core	E.H.P.*	50	Yes	Yes	No	New
19	0·15	0·147	3-Core	E.H.P.*	50	Yes	Yes	No	New
20	0·15	0·145	3-Core	E.H.P.*	50	Yes	Yes	No	New
21	0·15	0·144	3-Core	E.H.P.*	50	Yes	Yes	No	New
22	0·15	0·145	3-Core	E.H.P.*	50	Yes	Yes	No	New
23	0·1	0·120	6-Core } Split conductor system,	E.H.P.*	66	Yes	Yes	No	New
24	0·1	0·116	6-Core } each core circular	E.H.P.*	84	Yes	Yes	No	New
25	0·2	0·208	3-Core, Split conductor shaped cores	3 300 V	30	Yes	Yes	No	New
26	0·2	0·200	3-Core oval concentric split conductor	E.H.P.*	72	Yes	Yes	No	New
27	0·25	0·250	3-Core	10 000 V	30	Yes	Yes	No	New

* E.H.P. cables insulated as frequently employed for 20 000-volt working.

the finished cable, is plotted against the time required to attain to maximum temperature.

In Fig. 6, the heating for each of the six cables, Nos. 17 to 22, when laid in air is shown for various loads. These were all E.H.P. three-core armoured cables of nearly the same dimensions but of different makes, insulated as frequently supplied for 20 000-volt working. It will be noted that while four of the curves show almost identical results, two of them are markedly different. The curves are given mainly to illustrate the overall differences that may be obtained with cables of nominally the same type and size. The differences between the various curves are, however, due to a number of factors, such as thermal resistivity, dimensions, emission of heat from the lead sheath (depending somewhat on the nature and amount of the compound used in the wrappings of the armouring).

Effect of alternating and direct current.—Tests were made both with alternating current at 50 cycles per second and with direct current, and there was no appreciable difference between the two sets of results.

Effect of armouring.—The extent of the difference in the heating due to the presence of armouring was determined with cable No. 12, a 10 000-volt three-core wire-armoured cable. A heating test was made in air, both with the cable armoured and after the armouring had been stripped off. The actual results are set out in Table 4.

In a cable of this small size any effect due to armouring will be greater than in the larger cables, so that the difference due to armouring is in any case very small.

It should, however, be noted that the above figures refer to armouring only, and not to the difference between a bright lead finish and the dull black of an armoured cable. In the case given, the lead covering, after the removal of the armouring, retained a certain amount of the compound and was generally black.

Effect of colour of the surface.—Some tests have been made with a plain lead-sheathed cable to ascertain the difference, if any, due to painting the surface of the cable in different colours. The results are shown in Figs. 7 and 8.

It would appear that the actual colour of the surface makes little difference, but that a bright surface pro-

TABLE 1.—SCHEDULE 2.

Dimensions of Cables.

No.	Diameter over Lead		Thickness of Lead		Thickness of Insulation							
					Inner Dielectric		Outer Dielectric		Between Cores		Between Core and Lead	
	in.	cm.	in.	cm.	in.	cm.	in.	cm.	in.	cm.	in.	cm.
Single Cables.												
1	0·75	1·90	0·08	0·20	0·08	0·2	—	—	—	—	—	—
2	0·95	2·41	0·10	0·25	0·09	0·24	—	—	—	—	—	—
Concentric Cables.												
3	1·14	2·89	0·13	0·33	0·08	0·2	0·08	0·2	—	—	—	—
4	1·15	2·92	0·09	0·23	0·07	0·18	0·08	0·2	—	—	—	—
5	1·13	2·87	0·13	0·33	0·12	0·3	0·10	0·26	—	—	—	—
6	1·37	3·48	0·11	0·27	0·14	0·35	0·08	0·20	—	—	—	—
7	1·35	3·42	0·09	0·24	0·12	0·3	0·09	0·24	—	—	—	—
8	1·38	3·50	0·08	0·20	0·10	0·25	0·09	0·22	—	—	—	—
9	1·30	3·30	0·10	0·25	0·07	0·18	0·09	0·24	—	—	—	—
10	1·35	3·42	0·09	0·24	0·16	0·4	0·16	0·4	—	—	—	—
Three-core Cables.												
11	0·43	1·10	0·06	0·15	—	—	—	—	0·06	0·16	0·06	0·16
12	0·90	2·29	0·05	0·13	—	—	—	—	0·31	0·8	0·31	0·8
13	2·23	5·66	0·11	0·28	—	—	—	—	0·25	0·65	0·40	1·0
14	1·23	3·12	0·10	0·25	—	—	—	—	0·10	0·25	0·10	0·25
15	1·69	4·30	0·14	0·35	—	—	—	—	0·24	0·6	0·24	0·6
16	1·81	4·60	0·10	0·25	—	—	—	—	0·24	0·6	0·24	0·6
17	2·56	6·50	0·11	0·28	—	—	—	—	0·37	0·95	0·31	0·8
18	2·60	6·60	0·12	0·30	—	—	—	—	0·40	1·0	0·28	0·7
19	2·52	6·40	0·16	0·40	—	—	—	—	0·35	0·9	0·31	0·8
20	2·48	6·30	0·16	0·40	—	—	—	—	0·35	0·9	0·28	0·7
21	2·50	6·36	0·16	0·40	—	—	—	—	0·37	0·95	0·35	0·9
22	2·40	6·08	0·12	0·30	—	—	—	—	0·35	0·9	0·31	0·8
23	3·19	8·10	0·14	0·35	—	—	—	—	0·43	1·1	0·40	1·0
24	3·06	7·76	0·11	0·28	—	—	—	—	0·40	1·0	0·40	1·0
25	1·83	4·64	0·13	0·32	—	—	—	—	0·16	0·4	0·16	0·4
26	3·07	7·80	0·16	0·40	0·12	0·3	—	—	0·40	1·0	0·40	1·0
27	1·95	4·96	0·11	0·28	—	—	—	—	0·20	0·5	0·20	0·5

duces a marked change. These tests were made in order to investigate a small difference in some actual observations of cable temperatures. They are perhaps outside the scope of the present Report, but, if considered important, further tests could be made and the question of colour, etc., of surface more fully investigated.

It will be noted that while there is an appreciable difference between the heating with bright lead and dull black covering respectively, there is no marked difference between white and black. In the case of the lead in Fig. 8, the surface was dull and this probably accounts for the small differences.

TABLE 2.

Cables Laid in Air along the Floor of the Room.

Cable No.	Sectional Area		Remarks	Pressure	Armoured	Current for a Rise of 50 deg. F. (27·8 deg. C.) *		
	Nominal	Actual						
(1)	(2)		(3)	(4)	(5)	(6)		
	sq. in.	sq. in.				amp.	amp.	amp.
			Single Cables.					
1	0·1	0·094	Single	L.P.	No	—	190	—
2	0·2	0·196	Single	L.P.	No	—	304	—
			Concentric Cables.					
3	0·1	0·108	Concentric	L.P.	Yes	180	—	195
4	0·1	0·099	Concentric	L.P.	No	159	—	167
5	0·1	0·100	Concentric	L.P.	No	161	—	177
6	0·2	0·192	Concentric	L.P.	No	245	—	258
7	0·2	0·196	Concentric	L.P.	No	243	—	265
8	0·2	0·197	Concentric (jute insulated)	L.P.	(Jute covered)	265	—	295
9	0·2	0·205	Concentric	L.P.	Yes	268	—	288
10	0·5	0·480	Concentric	L.P.	No	404	—	460
			Three-core Cables.					
11	0·025	0·029	3-Core	L.P.	No	—	66	—
12	0·05	0·048	3-Core, shaped cores	10 000 V	Yes	—	100	—
13	0·1	0·105	3-Core	6 000 V	No	—	143	—
14	0·1	0·100	3-Core, shaped cores	L.P.	No	—	156	—
15	0·1	0·101	Shaped cores, 3-core	6 000 V	Yes	—	158	—
16	0·15	0·150	3-Core, shaped cores	6 000 V	Yes	—	195	—
17	0·15	0·145	3-Core	E.H.P.†	Yes	—	208	—
18	0·15	0·145	3-Core	E.H.P.†	Yes	—	204	—
19	0·15	0·147	3-Core	E.H.P.†	Yes	—	219	—
20	0·15	0·145	3-Core	E.H.P.†	Yes	—	208	—
21	0·15	0·144	3-Core	E.H.P.†	Yes	—	208	—
22	0·15	0·145	3-Core	E.H.P.†	Yes	—	195	—
23	0·1	0·120	6-Core } Split conductor system,	E.H.P.†	Yes	—	284	—
24	0·1	0·116	6-Core } each core circular	E.H.P.†	Yes	—	264	—
25	0·2	0·208	—	—	—	—	—	—
25	0·2	0·208	3-Core, split conductor shaped cores	3 300 V	Yes	—	219	—
26	0·2	0·200	3-Core, oval concentric split conductor	E.H.P.†	Yes	258	—	270
27	0·25	0·250	3-Core	10 000 V	Yes	—	255	—

* In the case of concentric cables, the values are given separately for inner and outer conductors; with three-core cables the values are the mean for the three cores.

† E.H.P. cables insulated as frequently employed for 20 000-volt working.

TESTS ON BURIED CABLES.

General arrangements.—The cables laid in the ground and the methods of laying are as follows:—

(1) *Armoured laid direct in the ground.*—Two concentric cables 0·1 and 0·2 sq. in.

(2) *Drawn into a six-way glazed stoneware duct with spigot joints laid direct in the ground.*—The six paper-insulated cables drawn into the duct include one 0·2 sq. in. concentric, one 0·5 sq. in. concentric, two lengths of 0·1 sq. in. concentric, one in an upper and one in a lower hole, and two 0·1 sq. in. three-core, one high-tension and the other low-pressure. The 0·1 sq. in. concentric in the lower hole was withdrawn later and replaced by a single cable with potential ends, connected on various points along its length. Each hole in the duct was $3\frac{1}{2}$ inches diameter.

(3) *Laid solid in bitumen in stoneware troughing.*—Two 0·2 sq. in., one single and one concentric

TABLE 3.

Cables Corrected to Two Standard Values of Thermal Resistivity.

Cable No.	Sectional Area		Remarks	Pressure	Armoured	Cables laid in Air. Current to produce a Rise of 50 deg. F. (= 27·8 deg. C.)			
	Nominal	Actual				K = Actual Value		K = 500	K = 1 000
(1)	(2)		(3)	(4)	(5)	(6)		(7)	(8)
	sq. in.	sq. in.	*Single Cables.*			K	amp.	amp.	amp.
1	0·1	0·094	Single	L.P.	No	1 200	190	206	197
2	0·2	0·196	Single	L.P.	No	800	304	353	294
			Concentric Cables.						
3	0·1	0·108	Concentric	L.P.	Yes	620	180	181	165
4	0·1	0·099	Concentric	L.P.	No	1 060	159	181	164
5	0·1	0·100	Concentric	L.P.	No	1 160	161	186	168
6	0·2	0·192	Concentric	L.P.	No	1 060	245	270	247
7	0·2	0·196	Concentric	L.P.	No	1 000	243	271	243
8	0·2	0·197	Concentric (jute insulated)	L.P.	(Jute covered)	870	265	286	259
9	0·2	0·205	Concentric	L.P.	Yes	720	268	274	240
10	0·5	0·480	Concentric	L.P.	No	1 090	404	445	405
			Three-core Cables.						
11	0·025	0·029	3-Core	L.P.	No	1 050	66	71	67
12	0·05	0·048	3-Core, shaped cores	10 000 V	Yes	730	100	111	90
13	0·1	0·105	3-Core	6 000 V	No	720	143	161	132
14	0·1	0·100	3-Core, shaped cores	L.P.	No	670	156	159	145
15	0·1	0·101	3-Core, shaped cores	6 000 V	Yes	420	158	156	136
16	0·15	0·150	3-Core, shaped cores	6 000 V	Yes	500	195	195	167
17	0·15	0·145	3-Core	E.H.P.*	Yes	460	208	206	174
18	0·15	0·145	3-Core	E.H.P.*	Yes	550	204	211	176
19	0·15	0·147	3-Core	E.H.P.*	Yes	470	219	217	178
20	0·15	0·145	3-Core	E.H.P.*	Yes	550	208	213	178
21	0·15	0·144	3-Core	E.H.P.*	Yes	460	208	204	170
22	0·15	0·145	3-Core	E.H.P.*	Yes	570	195	202	170
23	0·1	0·120	6-Core } Split conductor system,	E.H.P.*	Yes	600	284	298	250
24	0·1	0·116	6-Core } each core circular	E.H.P.*	Yes	600	264	281	238
25	0·2	0·208	3-Core, split conductor shaped cores	3 300 V	Yes	650	219	227	206
26	0·2	0·200	3-Core, oval concentric split conductor	E.H P.*	Yes	710	258	278	238
27	0·25	0·250	3-Core	10 000 V	Yes	580	255	262	231

* E.H.P. cables insulated as frequently employed for 20 000-volt workings.

in one trough; and four 0·1 sq. in., two single and two concentric in another trough. All the cables are paper-insulated.

The depth of laying is 2 feet to the top of the duct or cables.

The ends of the cables emerge into two observation huts 100 feet apart. These are sunk 4 feet below the ground-level, the floor and walls being of concrete, 6 inches thick.

The wall is continued for 2 feet above the concrete with weather boarding, the roof being pitched high enough to allow of a door 5 feet 6 inches high in the centre of one end. The hut at the south end is 11 feet wide and 8 feet long, the cable ends being brought up to a terminal board to which they are attached by bolts passing through the board and through a specially designed thimble with screw thread. This hut is used for making connections to the cables as required.

The hut at the north end is larger, being 11 feet

TABLE 4.

Current	Temperature Rise	
	Armoured	Armour removed
75 amperes	15·8 deg. C.	16·4 deg. C.
100 amperes	27·8 deg. C.	28·6 deg. C.
120 amperes	41·5 deg. C.	42·7 deg. C.

square, contains all the measuring instruments, transformers, regulating resistances, etc.

The length of cable in the ground between the huts is 100 feet.

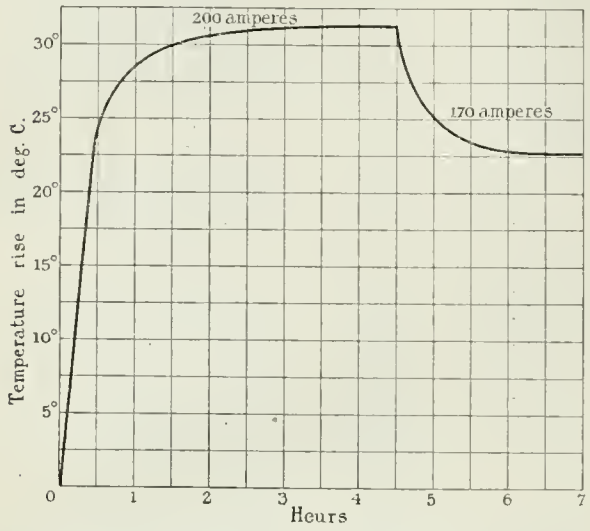

FIG. 1.—Curve showing temperature-rise of a 0·1 sq. in. single cable, tested in air. The cable was loaded with lead and return together; and consequently no resistance readings (and therefore no temperature readings) could be taken on the lead sheath.

In the case of the armoured cables and those laid solid in bitumen, potential points were taken off at a distance of 5 feet from the huts. For the cables laid in the duct, however, the construction did not allow

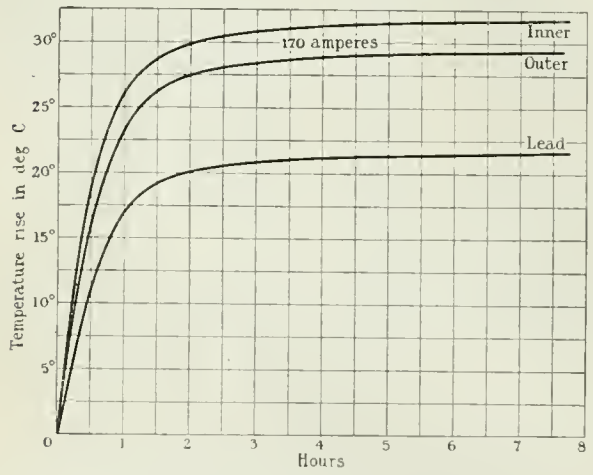

FIG. 2.—Curves showing temperature-rise of a 0·1 sq. in. concentric cable, tested in air.

of these tappings, hence the potential wires were taken off just above the sealing boxes. To meet this, however, a complete investigation was made of the thermal conditions along the duct so that a correction could be applied for the lengths of cable in the hut between the duct and the terminal board, and for any end effect due to heating or cooling from the huts.

The current supply was alternating at 250 volts,

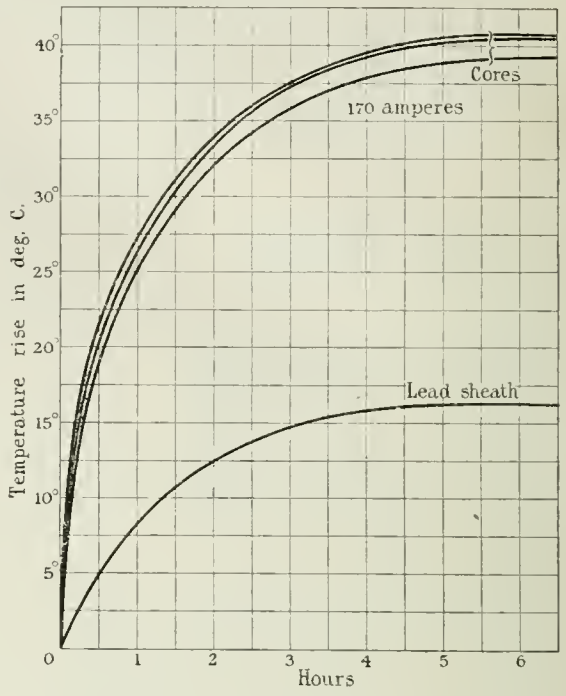

FIG. 3.—Curves showing temperature-rise of a 0·1 sq. in., 3-core, 6 000-volt cable, tested in air.

50 periods, the regulation being obtained by suitable resistances in the primary circuit of the transformers. The current was maintained constant at any given figure by means of a Tirrill regulator.

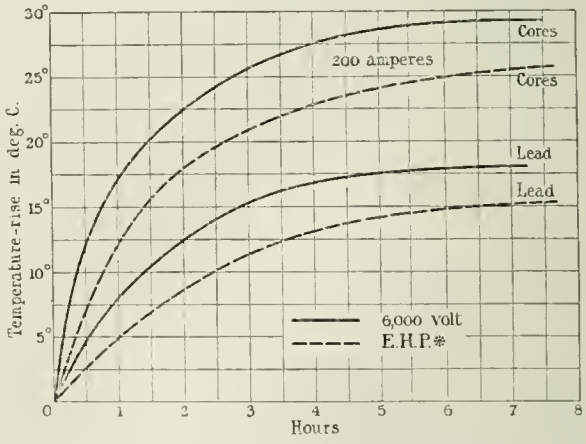

FIG. 4.—Curves showing temperature-rise of 0·15 sq. in. 3-core cable, tested in air.

* E.H.P. cables insulated as frequently employed for 20 000-volt working.

Methods of measurement.—The rise of temperature was determined in each case by measuring the increase in resistance of the conductors by a Kelvin double bridge. The method used was to superimpose a small direct current on the alternating current. The ar-

rangement is shown in Fig. 20, from which it will be seen that the accumulators can be put into series with the secondary of the transformer and with the cable by means of the double-pole change-over switch whenever a reading is to be taken. Three accumulators connected in parallel were used, each of 150 ampere-hour capacity.

The primary measurements of the current were made with a precision dynamometer-type ammeter, working in conjunction with a current transformer. Since, however, this instrument did not indicate the small direct current passing at the time at which the observations were made, a hot-wire ammeter was put into the circuit. On switching over on to the accumulators, the total current, alternating and direct, rose or fell slightly, depending on the relation of the total current to that given by the accumulators, and the internal resistance of the accumulators. The hot-wire ammeter, therefore, was used to set the current immediately after switching over and during the time that the observations were made.

The time required for the observations, usually only a few seconds, was in no case greater than one minute, so that any slight discrepancy in the current for this period would not affect the results appreciably.

For concentric cables or for single cables when the lead and return are close together, if galvanometers are used that are not affected by the frequency of the alternating current, the method gives excellent results, although the accuracy is, of course, not so high as when using direct current only. But when single cables or a number of concentric cables are to be tested in such a way that the circuit cannot be kept non-inductive, the method fails. Even a small amount of induction results in a considerable widening of the galvanometer spot, thus rendering definite observations out of the question (an increase of as much as 2 cm. has been observed, although normally the line is 0·25 cm. wide, or less).

The instruments generally were unaffected by the comparatively large alternating current passed through them in addition to the direct current to which they responded. The accumulators too, after a large amount of use under these conditions, do not appear to have suffered.

The only difficulty that may be encountered is the possibility of overheating in the cells if the alternating current is left on for a long time.

The usual proportion between the currents was about 20 amperes drawn from the accumulators to 300 amperes alternating current.

In addition to observations on the inner and outer cores of a concentric cable and the cores of a single cable, the temperature-rise of the outside of the cable was determined by measuring the rise of resistance of the lead sheath. A separate current of about 15 amperes was passed through the sheath for a sufficient time to allow the readings to be taken, and the resistance was measured by means of a double bridge made by Mr. R. W. Paul. Connection was made to the sheath by means of band clamps, and potential leads taken off at nearly the same points as in the case of the conductors.

An attempt was made to determine the temperature of the air in the duct by means of a series of thermo-junctions placed along it, the measurements of thermo-E.M.F. being made by means of a potentiometer. For reasons stated later, these observations proved to be valueless and were discontinued.

End effects.—The question as to the extent to which the results are affected by the exposure of the ends of the cable in the hut between the ground and the

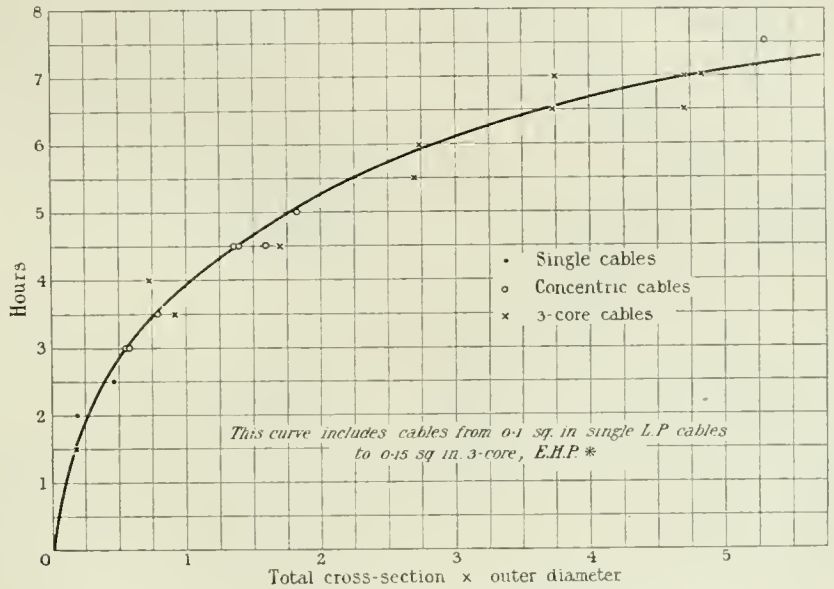
Hours
Total cross-section × outer diameter
Single cables
Concentric cables
3-core cables
This curve includes cables from 0·1 sq. in single L.P cables to 0·15 sq in. 3-core, E.H.P *

FIG. 5.—Curve showing the time taken to attain the maximum temperature-rise in air.

* E.H.P. cables insulated as frequently employed for 20 000-volt working.

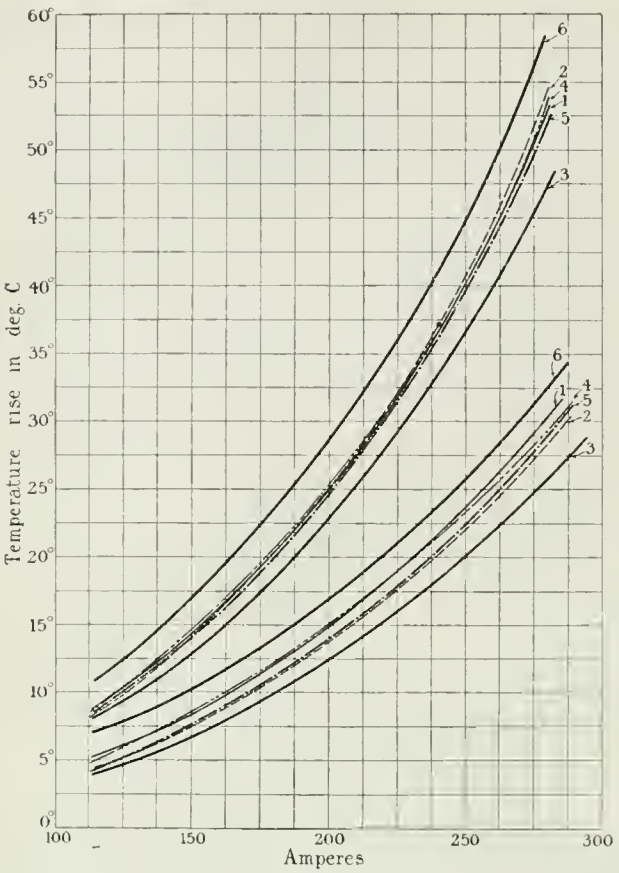

FIG. 6.—Curves showing current in relation to temperature-rise in steady state of 0·15 sq. in., 3-core, E.H.P. cables, tested in air.

Upper curves refer to cores.
Lower curves refer to lead sheaths.

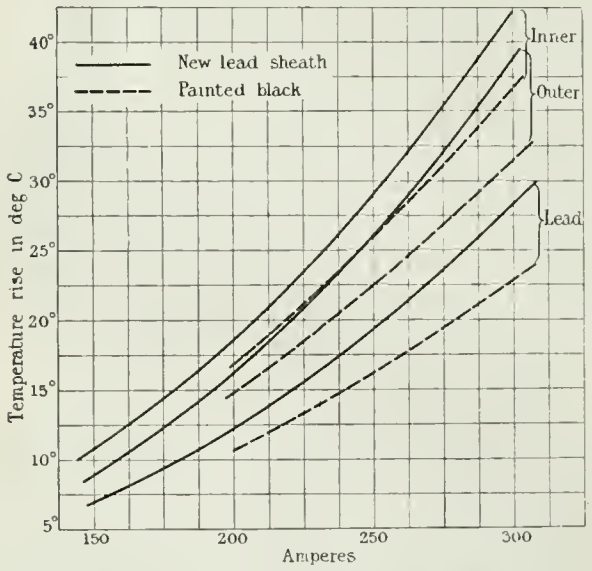

FIG. 7.—Curves showing the effect of different coloured surfaces on the temperature-rise of a 0·2 sq. in. concentric cable, tested in air.

terminal boards, or by the change of temperature due to the hut itself, has been investigated fairly fully, and other check tests that have lately suggested themselves are now being carried out. The point is met in the case of the armoured cables and those laid solid by taking off potential points at a distance of 5 feet in the ground. In the case of the cables in the duct, however, it was impossible to do this, and measurements were made, therefore, with a view to obtaining a correcting factor.

It was proposed to determine the temperature along the duct by means of a series of thermo-junctions. The junctions were attached to, and spaced out along

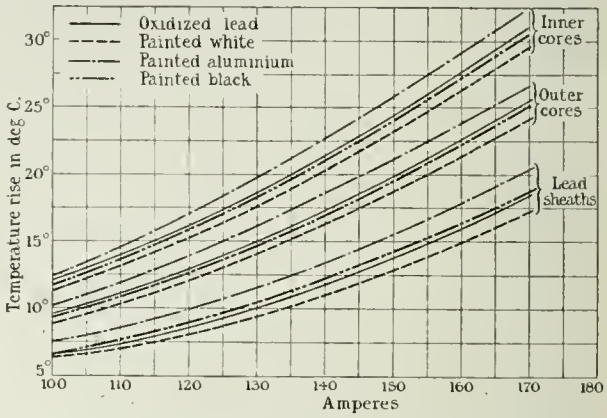

FIG. 8.—Curves showing the effect of different coloured surfaces on the temperature-rise of a 0·1 sq. in. concentric cable, tested in air.

wires which could be drawn into any position along the length of the duct, and they were protected by being passed through wooden distance-pieces fixed at definite points on the wire. The results obtained were, however, irregular, and a close investigation

TABLE 5.

Position	Temperature
	°C.
Inner core	47·0
Outer core	41·0
Lead sheath	34·0
Touching cable	31·0
0·5 cm. vertically above sheath ..	26·0
1·5 cm. vertically above sheath ..	22·0
2·5 cm. vertically above sheath ..	18·0
3·5 cm. vertically above sheath ..	16·0
4·5 cm. vertically above sheath ..	14·5
Touching top of duct	14·0

was made of the temperature gradient in the hole by means of a series of thermo-junctions spaced one above the other and fixed rigidly.

This investigation yielded some interesting results which are given in Table 5 and in Fig. 9. The cable used was a 0·2 sq. in. concentric with a current of 300 amperes. It will be seen that if the position of

a thermo-junction were displaced by 5 mm. in either direction, the difference in the observed temperature can be placed on any measurements made by means of a thermometer pushed into a duct.

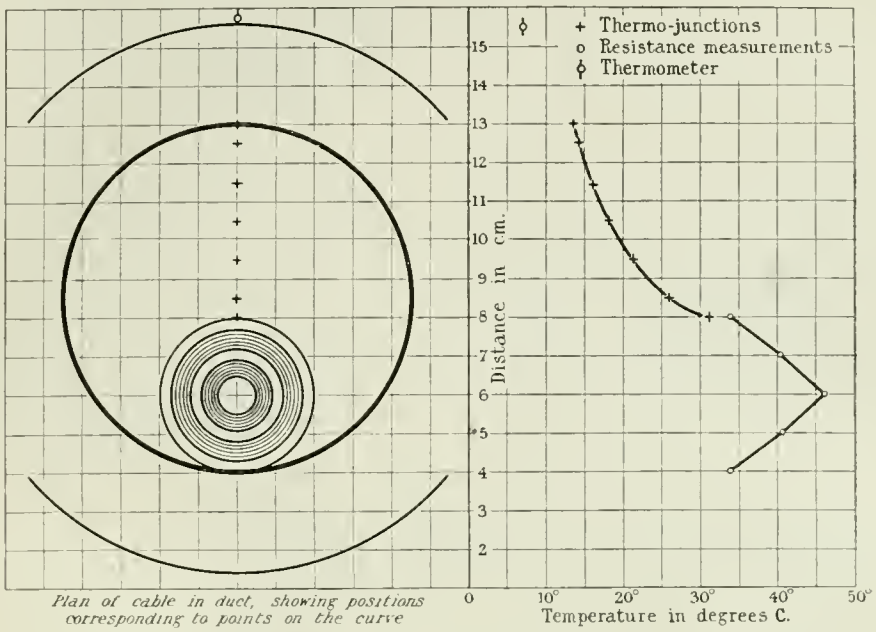
+ Thermo-junctions
o Resistance measurements
ϕ Thermometer
Distance in cm.
Temperature in degrees C.
Plan of cable in duct, showing positions corresponding to points on the curve

FIG. 9.—Curves showing temperature gradient in a 3½ in. duct.

would be of the order of 10 per cent, a far larger amount than the total end effect.

The construction of a cable duct does not allow of the thermo-junctions being drawn in along the whole

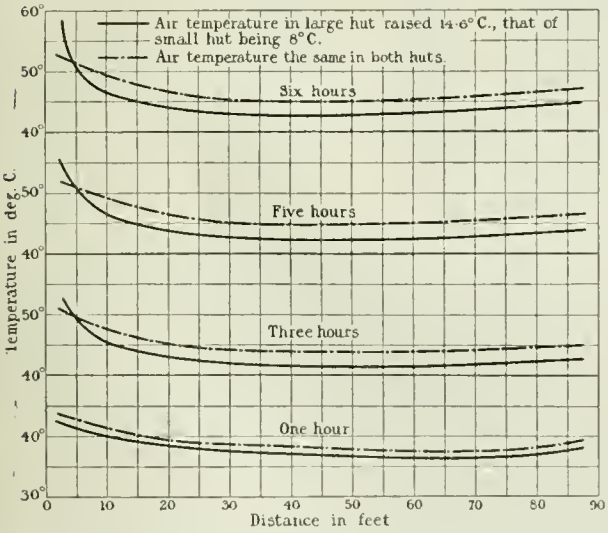
Air temperature in large hut raised 14·6° C., that of small hut being 8° C.
Air temperature the same in both huts.
Six hours
Five hours
Three hours
One hour
Temperature in deg. C.
Distance in feet

FIG. 10.—Curves showing temperature along the duct with a 0·1 sq. in. rubber-insulated single cable, carrying 300 amperes.

length or spaced at various points along the length to such a degree of accuracy as is shown to be necessary, and, after several trials, the method was abandoned.

Further, the results show that very little reliance

Since the use of thermo-junctions proved unsatisfactory, a 0·1 sq. in. cable with a number of potential leads fixed at various points along its length was drawn into the duct. The potential points were arranged so that the temperature of the ends in sections of 5 feet or 10 feet, and of the central 50 feet, could be obtained. The arrangement also allowed of each of the sections

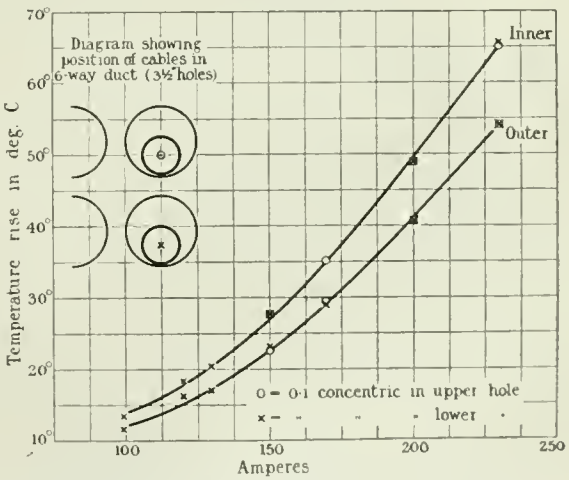
Diagram showing position of cables in 6-way duct (3½" holes)
Inner
Outer
o = 0·1 concentric in upper hole
x = " " " lower "
Temperature rise in deg. C
Amperes

FIG. 11.—Curves showing comparison between temperature-rise of similar cables in lower and upper holes of duct.

being tested independently or collectively with others. Initial tests in air showed that the rise of temperature of the various sections of the cable was quite uniform. When, however, the cable was drawn into the duct,

the results showed that the temperature along the length of the duct was not quite uniform, but rose slightly at each end. This is not due to heating at the ends, since the huts were at nearly the same temperature as the soil. The difference due to this apparent thermal irregularity does not affect the results appreciably.

The real end effect due to heating of the air in the huts above the temperature of the soil is shown in Fig. 10; the dotted lines represent the temperature of the cable in the duct, when the huts are at or near the ground temperature (6·5° C.). The full-line curves show the same cable under similar conditions, except that the temperature of the large hut was raised from 8·0° C. to 14·6° C. during the run. This represents a more extreme condition than normally occurs in practice; but, even so, the correcting factor on the value obtained for the cable is only 1·4 per cent.

The temperature conditions from one hole to another of the duct apparently did not vary appreciably, and in Fig. 11 is shown the heating of a 0·1 sq. in. cable in an upper and in a lower hole in the duct. The two curves are very nearly alike.

The end effect in armoured cables and in those laid solid in bitumen, while it could be eliminated for purposes of our tests, is nevertheless a serious factor in connection with the rating of cables under actual working conditions where the end of the cable is brought into a junction box or to the back of a switchboard. The extent of the difference may be judged from the following results obtained with the 0·2 sq. in. concentric armoured cable laid direct in the ground.

Current	Temperature-Rise of Cable in the Ground	Rise of last 10 ft. of Cable, 5 ft. in Soil and 5 ft. in Hut
350 amperes ..	33° C.	45 deg. C.
450 amperes ..	59° C.	84 deg. C.

Further results on this point are being obtained. The difficulty could be met in practice by joining the cable in the ground with a tail of larger section.

Tests with natural and forced ventilation.—Some tests were made with an air blast passed through the six-way duct. The air, however, did not all travel through the hole in which the cable was laid, and it was felt that before any definite results could be obtained, all the six cables should be heated together and measured nearly simultaneously. Further tests on these lines were held over, therefore, until a supply of direct current should be available.

The effect of natural ventilation on the heating of the cables in the duct was also investigated. For this purpose cables were run with the duct holes closed up, as well as under the normal conditions when they would be open. No difference could be detected in the temperature-rise obtained in the two cases.

Table 6.

I_1 = Current required to produce a given temperature-rise when tested in air under the conditions stated on page 184.
I_2 = Current required to produce the same temperature-rise when buried.

No.	Cable	Method of Laying	I_2	I_1	$I_2 I_1$
(1)	(2)	(3)	(4)	(5)	(6)
1	0·1 Single	Solid in bitumen	266	216	1·23
2	0·2 Single	Solid in bitumen	432	350	1·23
4	0·1 Concentric	Solid in bitumen	199	165	1·21
6	0·2 Concentric	Solid in bitumen	360	293	1·23
3	0·1 Concentric	Armoured, direct in ground	215	176	1·22
9	0·2 Concentric	Armoured, direct in ground	335	275	1·22
5	0·1 Concentric	Drawn into stoneware ducts	130	133	0·98
7	0·2 Concentric	Drawn into stoneware ducts	243	252	0·96
10	0·5 Concentric	Drawn into stoneware ducts	370	378	0·98
14	0·1 3-Core L.P.	Drawn into stoneware ducts	157	162	0·97
13	0·1 3-Core H.P.	Drawn into stoneware ducts	162	170	0·95

The factor given in Column (6) is based on at least four determinations at different currents on each cable. In columns (4) and (5) are given the values for one current only.

Effect of Conditions of Laying.

Comparison of results obtained.—The effect of the different conditions of laying is best shown by a comparison of the results obtained with the same cables when laid in air. In Table 6 the comparison is shown for different sizes of cables, the values in column (6) being the ratio of the current when laid under the three conditions to that in air.

The values of the factor defined in the Table as I_2/I_1 are remarkably constant for different sizes of cables, but it should be noted that nearly all of them apply to low-pressure cables. It is probable that in the case of cables drawn into ducts, the value of the factor for high-pressure cables would be nearly the same as that for low-pressure cables, but it is reasonable to assume that, comparing the heating of a low- and a high-pressure cable when laid in air and buried direct in the ground, while the low-pressure cable would be hotter than the high-pressure cable when tested in air,

the factor I_2/I_1 for the high-pressure cable would be lower than that obtained for the low-pressure cable. In the case of the low-pressure cable, the surrounding air is replaced by damp earth which has a lower thermal resistivity than either air or the cable dielectric, while in a high-pressure cable the greater thickness of insulation, which facilitates the heat dissipation when the cable is run in air, has a contrary effect when the cable is buried direct in the ground, the greater thickness of insulation tending to reduce the cooling effect of the earth.

This assumption is confirmed by some results obtained with a three-core 0·2 sq. in. split-conductor 20 000-volt cable buried direct in the ground. Definite results with this cable are vitiated to a certain extent by the fact that an amount of oil has been forced out through the sealing end and that, in consequence, the thermal characteristics of the dielectric are probably changed.

The value of the factor I_2/I_1 obtained, however, is more nearly unity as compared with 1·21, the figure found for a low-pressure cable buried direct in the

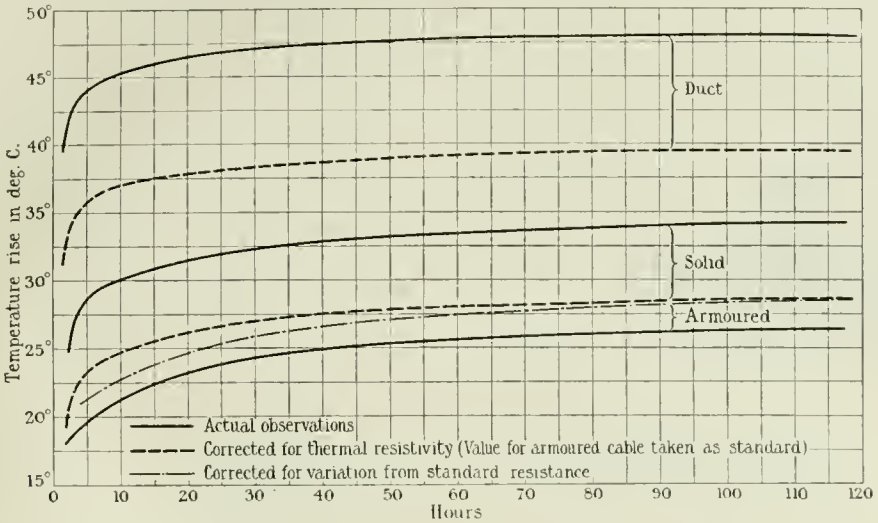

FIG. 12.—Curves showing comparison between 0·1 sq. in. concentric cables, carrying 200 amperes, and laid in three different ways.

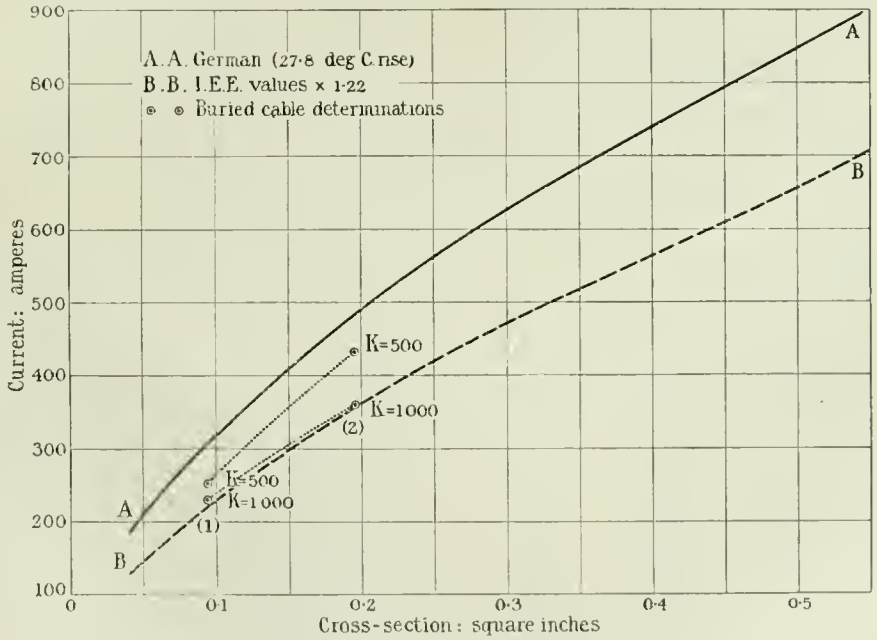

FIG. 13.—Curves showing temperature-rise of single low-tension cables, as calculated from different formulæ.

ground. Further tests, therefore, are being made with other high-pressure cables laid direct in the ground, and until these are available the factors given for cables laid direct in the ground should not be used for high-pressure cables.

Rate of temperature-rise.—A typical heating curve for concentric cables laid under different conditions is shown in Fig. 12, which refers to the 0·1 sq. in. cables. It will be seen that the actual results are given, together with those corrected to a common basis of thermal resistivity and nominal conductor resistance. When thus corrected, the final temperature elevation of the armoured and solid laid cables is nearly identical. The time required to attain to that temperature is, however, different for the three cases.

Assuming that after 100 hours the cable has attained to its maximum temperature, the following figures, the mean of a number of readings, show the comparative rates at which the temperature of cables rise when laid under the three systems.

Period	Per cent of Maximum Increase		
	Duct	Solid in Bitumen	Armoured direct in Ground
After 7½ hours ..	95	88	85·5
After 20 hours ..	98	92·5	91·4
After 40 hours ..	99·0	95·5	93·1
After 60 hours ..	99·8	97·0	95·8
After 80 hours ..	99·8	98·8	97·9
After 100 hours ..	100·0	100·0	100·0

Clearly the cables in the duct reach their maximum temperature sooner than either those laid solid or direct in the ground, although all of the cables reached to, or above, 90 per cent of their maximum heating in 20 hours. It will be seen from Fig. 12 that the temperature of the armoured cable is apparently still rising, even after 110 hours, but it should be pointed out that after 60 hours the rise is so slight that variations in the soil temperature begin to affect the results. It is possible that the continued rise is really due to the drying out of the surrounding soil resulting in a change in its thermal resistivity, but actually the changes are not more rapid than those of the soil temperature. Later results, in which closer attention has been paid to the changes in temperature of the soil, indicate that the temperature of a cable laid direct in the ground does not increase appreciably after 50 hours.

Comparison of values with data obtained elsewhere.—The values given in Table 3, multiplied by the factors given in Table 6, have been plotted for each type of cable together with the German figures. Fig. 13 shows the values for single low-pressure cables, Curve "A" being the German values corrected to a temperature-rise of 50 degrees F.; Curve "B" the present I.E.E. Wiring Rules Table multiplied by the factor 1·22, thus giving the current which would be required to produce the same temperature-rise when the cables are buried direct in the ground or solid in bitumen. The points shown on the curve are those now obtained for cables having thermal resistivities of 500 and 1 000 respectively. It should be noted that in the German tables the value of thermal resistivity is taken as 550.

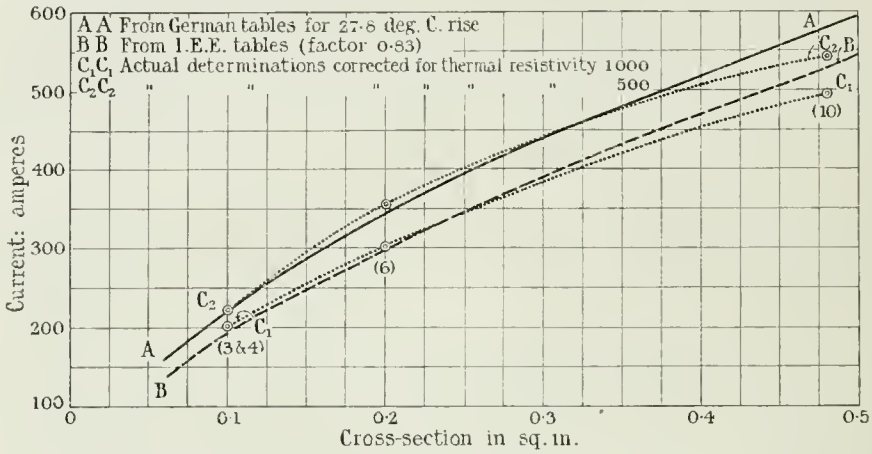

FIG. 14.—Curves showing temperature-rise of concentric low-pressure cables laid direct in the ground or solid in bitumen, as calculated from different formulæ.

The values for concentric cables are shown in Fig. 14. In this case the multiplying factor required to correct the permissible current for single cables to that for concentric cables was considered, and curve "B" is based on the I.E.E. figures for single cable multiplied by 0·83 and by 1·22 to give the equivalent results for concentric cables laid direct in the ground or solid in bitumen.

The factor allowed in the present Wiring Rules is 0·93, while that determined with three sizes of concentric cable, i.e. 0·1, 0·2, and 0·5 sq. in., was 0·86, 0·82 and 0·76 respectively. The value deduced from the German table is 0·70.

It should, however, be noted that the 0·5 sq. in. concentric cable, while supplied as low-pressure, was more nearly a 3 000-volt cable of high thermal resistivity. For a resistivity of 500, the calculated current

value for this cable is very nearly on the curve, but at 1 000, owing to the larger dimensions, it is somewhat low.

The three-core cables shown in Fig. 15 give the most variable and interesting results. It will be seen that results with low- and medium-pressure cables lie fairly smoothly on a curve. The E.H.P. cables, however, Nos. 17 to 22 and Nos. 23, 24 and 26, give results all appreciably higher if the factor for low-pressure cables be used. Actually, however, as is shown by the preliminary value for the E.H.P. cable No. 23, the values will be below the German figures and lower than those shown in the curve C_1. The values for these cables, Nos. 23, 24, and 26, when tested in air are shown separately in Fig. 16.

Figs. 13 to 15 should, however, only be considered

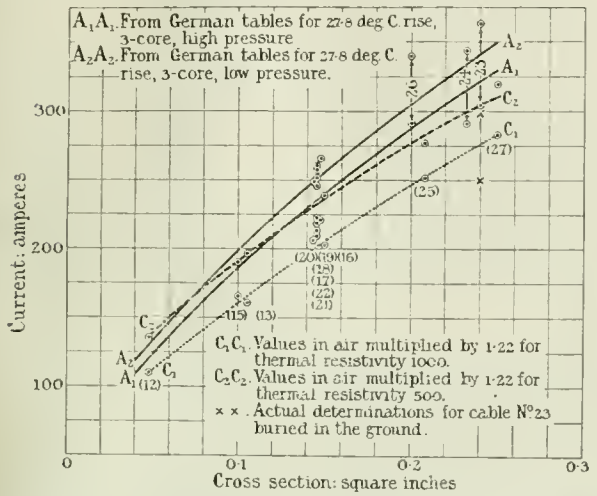
A1A1. From German tables for 27·8 deg C. rise, 3-core, high pressure
A2A2. From German tables for 27·8 deg C. rise, 3-core, low pressure.
C1C1. Values in air multiplied by 1·22 for thermal resistivity 1000.
C2C2. Values in air multiplied by 1·22 for thermal resistivity 500.
× × . Actual determinations for cable N°23 buried in the ground.
Current: amperes
Cross section: square inches
300
200
100
0
0·1
0·2
0·3

FIG. 15.—Curves showing values for 3-core cables buried direct in the ground. (Preliminary values.)

to be preliminary, until the final results with the largest size cables buried in the ground are available, and until the tests for dielectric losses are completed.

THERMAL CONSTANTS OF CABLES.

The results obtained with the cables already laid, and particularly those with the jute-insulated cable, suggested that the thermal resistivity of the insulating material might vary with different cables to a larger extent than had hitherto been supposed. This is also shown in Fig. 17, where the values obtained experimentally on a three-core cable show that the heating is very little greater than for a concentric cable, although 50 per cent more watts are being put into it. It was found, on investigation, that the differences were due to the value of the thermal resistivity not being the same for both cables. The dotted-line curve gives the value obtained by calculation, assuming the thermal resistivity to be the same as for the single and concentric cables. Further determinations were therefore made with all the then existing cables which were purchased, with a length of six-core, and with one of three-core concentric conductor, kindly provided by Glasgow Corporation. These were supplemented later by the six lengths of three-core cable, with

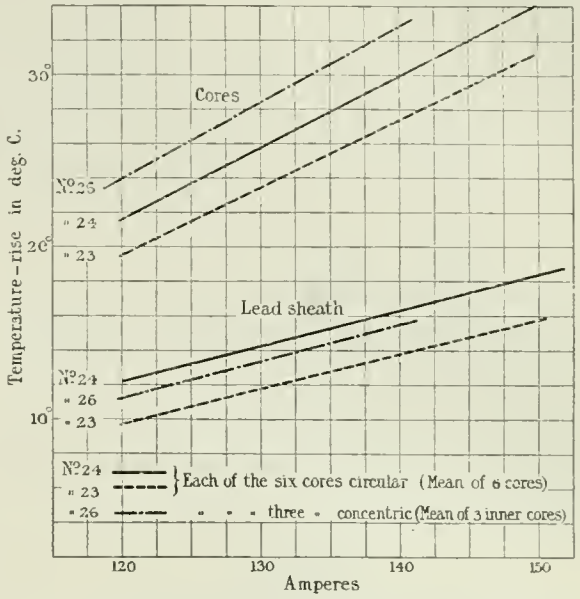

FIG. 16.—Curves showing temperature-rise of 0·2 sq. in. E.H.P. split-conductor cables tested in air, insulated as frequently supplied for 20 000-volt working.

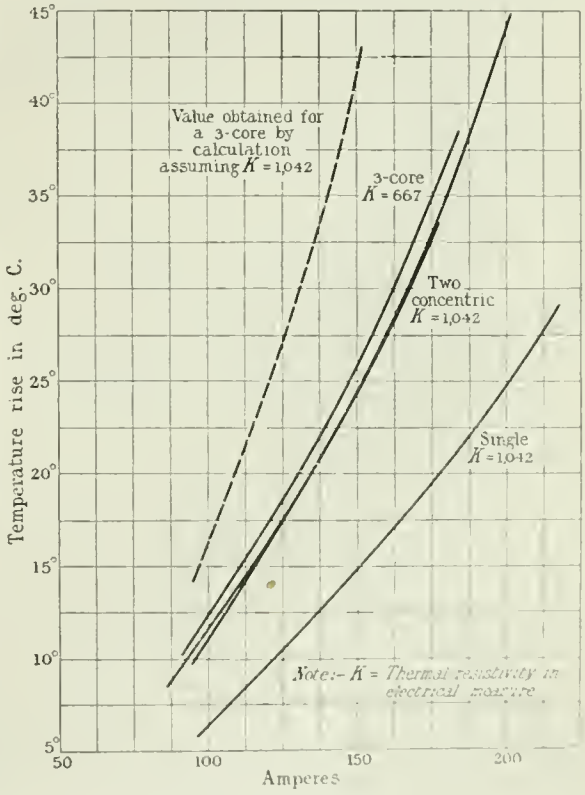

FIG. 17.—Curves showing temperature-rise of a 0·1 sq. in. cable, tested in air.

insulation as frequently used for 20 000-volt working of different makers, provided by the Cable Makers'

Association in connection with the research on the heating of high-pressure cables buried direct in the ground.

The thermal resistivity of the cables was determined by measuring the temperature of the core and the lead sheath by change of resistance, the dimensions of the cable being known. It was assumed that the heat distribution throughout the core was uniform; and also that there was no appreciable temperature gradient in the lead. All the American and German authorities have neglected any gradient in the lead sheath, and this is justified by consideration of the thermal resistivity of the lead as compared with that of a paper and oil dielectric.

The thermal resistivity of lead is 2·9, as compared with 500 for the dielectric; thus, with a temperature gradient of 20 degrees C. between the core and the outside of the lead sheath, the gradient in the sheath would be of the order of only 0·1 degree C.

The resistivity was calculated by means of the following formulæ adapted from those given by Dr. A. Russell:—

For *single-core* cables, K the thermal resistivity

$$= \frac{2\pi(\theta_1 - \theta_2)}{Q \log_e (d_1\ d_2)}$$

where θ_1 = temperature of the core,
θ_2 = temperature of the sheath,
Q = total heat energy in watts developed in each cm. length of cable,
R = resistance per cm. length of conductor at 15° C.,
d_1 = inner diameter of the lead sheath,
d_2 = outer diameter of the core.

For a *concentric* cable

$$K = \frac{2\pi(\theta_1 - \theta_2)}{Q[\frac{1}{2}\log_e(d_3/d_4) + \log_e(d_1/d_2)]}$$

where d_1 = inner diameter of the sheath,
d_2 = outer diameter of the outer conductor,
d_3 = inner diameter of the outer conductor,
d_4 = diameter of the core.

And for *multicore* cables:

$$K = \frac{4\pi(\theta_1 - \theta_2)}{Q\frac{2}{n}\log_e\left\{\frac{r_3^{2n} - r_2^{2n}}{nr_3^n\ r_2^{n-1}r_1}\right\}}$$

where r_1 = radius of each core,
r_2 = radius of circle on which the centres of the cores lie,
r_3 = inner radius of the sheath.

For three-core cables of clover-leaf pattern this equation can be simplified to:

$$K = \frac{4\pi(\theta_1 - \theta_2)}{Q\frac{2}{3}\log_e\left\{\frac{2r_3^3 - (b^3 + c^3)}{b^3 - c^3}\right\}}$$

where b = maximum distance between core and cable axis,
c = minimum distance between core and cable axis.

The thermal resistivity thus obtained is expressed in terms of the difference of temperature in degrees C. required to cause the flow of one joule of heat per second between opposite faces of a centimetre cube. One joule of heat per second expressed in electrical measure is one watt, and equals 0·24 gramme-calories per second.

Table 7 gives the values of thermal resistivity obtained and also includes some values for paper and

TABLE 7.

Thermal Resistivities of the Dielectric of Paper-insulated Cables.

Cable No.	Paper-insulated Cable	Mean Thermal Resistivity in Electrical Measure
	Single Cables.	
1	0·1 Single L.P.	1 200
2	0·2 Single L.P.	800
	Concentric Cables.	
3	0·1 Concentric L.P.	620
4	0·1 Concentric L.P.	1 060
5	0·1 Concentric L.P.	1 160
6	0·2 Concentric L.P.	1 060
7	0·2 Concentric L.P.	1 000
8	0·2 Concentric L.P.	870 (jute insulated)
9	0·2 Concentric L.P.	720
10	0·5 Concentric L.P.	1 090
	Three-core Cables.	
11	0·025 3-Core L.P.	1 050
12	0·05 3-Core H.P.	730
13	0·1 3-Core H.P.	720
14	0·1 3-Core H.P.	670
15	0·1 3-Core H.P.	420
16	0·15 3-Core H.P.	500
17	0·15 3-Core H.P.	460
18	0·15 3-Core H.P.	550
19	0·15 3-Core H.P.	470
20	0·15 3-Core H.P.	550
21	0·15 3-Core H.P.	460
22	0·15 3-Core H.P.	570
23	0·2 3-Core H.P.	600
24	0·2 3-Core H.P.	600
25	0·2 3-Core H.P.	650
26*	0·2 3-Core H.P.	710
27	0·25 3-Core H.P.	580

Bitumen	511
Paper (not impregnated) ..	960
Vulcanized bitumen	486
Bitumen cables: values determined by Dr. Marchant ..	510

* The inner and outer of each core were in parallel and treated as one conductor.

bitumen, etc., determined separately. In Table 8 are the values previously obtained by other observers.

An examination of the Tables shows that the higher values of thermal resistivity were more often obtained with the low-pressure cables, and this question will receive further attention. Whilst the possible occurrence of high values of thermal resistivity in cables of any size must be considered, it should be noted that in the case of the six cables Nos. 17 to 22, which represent the more modern E.H.P. three-core cable, the values were all low, the average being about 500. Thus, for this type of cable it appears probable that a value

TABLE 8.

Results obtained by Previous Observers.

Observer	Thermal Resistivity
Atkinson and Fisher	1 000
Atkinson and Fisher	833
Powell	1 235
Powell	877
Melsom and Booth	980
Melsom and Booth	746
Herzog and Feldman	477
Herzog and Feldman	685
Teichmüller* and Humann	642
Teichmüller and Humann	567
Teichmüller and Humann	905
Teichmüller and Humann	566
Teichmüller and Humann	679
Teichmüller and Humann	561
Teichmüller and Humann	590
Teichmüller and Humann	635
Teichmüller and Humann	526
Teichmüller and Humann	768
Teichmüller and Humann	418
Teichmüller and Humann	646
Teichmüller and Humann. Mean adopted for single L.P. cables	650
Teichmüller and Humann. Mean adopted for multicore L.P. cables	600
Teichmüller and Humann. Mean adopted for multicore H.P. cables	550
Apt and Mauritius	359
Apt and Mauritius	678

* Teichmüller apparently rejected the higher values.

of 500 or so might be taken as the normal figure, as compared with the accepted German value of 550 to 650.

For the purpose of showing the effect on the heating of a cable of both high and low values of thermal resistivity, in Table 3 the current-carrying capacity of various sizes and types of cables for a given temperature-rise is shown for thermal resistivities of 500, 1 000, and the actual value for the particular cable.

It will be noted that the current-carrying capacity of the cable is not affected by the thermal constants to the extent that at first sight might be expected.

It is not possible to determine the value of the thermal resistivity of a cable dielectric to a high degree of accuracy, and, consequently, the accuracy of the calculated figures for current, especially where these are given for a thermal resistivity widely different from the actual, is subject to an estimated possible variation of $\pm$ 3 per cent.

The thermal resistivity does not appear to be affected by temperature, within the limits of temperature attained in the cables.

CHARACTERISTICS OF THE SOIL.

Seasonal changes.—The observations on the cables buried in the ground have been duplicated at various times of the year. In particular, results obtained in midsummer and midwinter have been compared, but there is no evidence of any difference.

It is to be expected that seasonal changes, involving as they do a difference of initial temperature of about 10 degrees C., would have produced a change in the temperature-rise, but it would seem that any such change is masked by possible variations due to rainfall, etc.

Temperature of the soil.—It will be noted that all of the figures given in this Report refer to a temperature-rise of 50 degrees F. The actual observations covered a wide range of temperature and figures can be deduced for any desired temperature. The temperature of 50° F. was taken in order to facilitate ready comparison with existing figures.

The final permissible maximum temperature of a paper-insulated cable is at present stated in the I.E.E. Wiring Rules to be 150° F. This allows of an air temperature of 80° F., a rise due to current of 50 degrees F. with a margin of 20 degrees F. for possible overloading resulting in a fuse blowing.

The results of daily observations taken of the temperature of the ground at points near the cables are shown in Fig. 18. It will be noted that for a short period during August 1919, at the end of a long spell of hot weather, the temperature at a depth of 2 feet rose to 18° C., i.e. 68° F.

Moisture content.—The amount of moisture in the soil may affect the results appreciably. Previous observers have shown that while there is a very large difference of thermal resistivity between dry soil and that containing 5 per cent of water, there is no very large difference for a higher moisture content. These values are, however, to be checked. A preliminary value already obtained for fine soil containing 15 per cent of moisture was $K = 106$.

Regarding the actual amount of moisture present in the soil, the values obtained at Teddington at a depth of 2 feet at different periods of the year vary from 9 per cent to 15 per cent. In this case the soil is sandy, with water only about 12 feet from the surface. Professor Marchant found about 10 per cent in Liverpool, while Mr. Fawssett in Newcastle found that in winter the average moisture content was never far from 20 per cent. There are apparently no official records of the moisture content of the soil, and it has

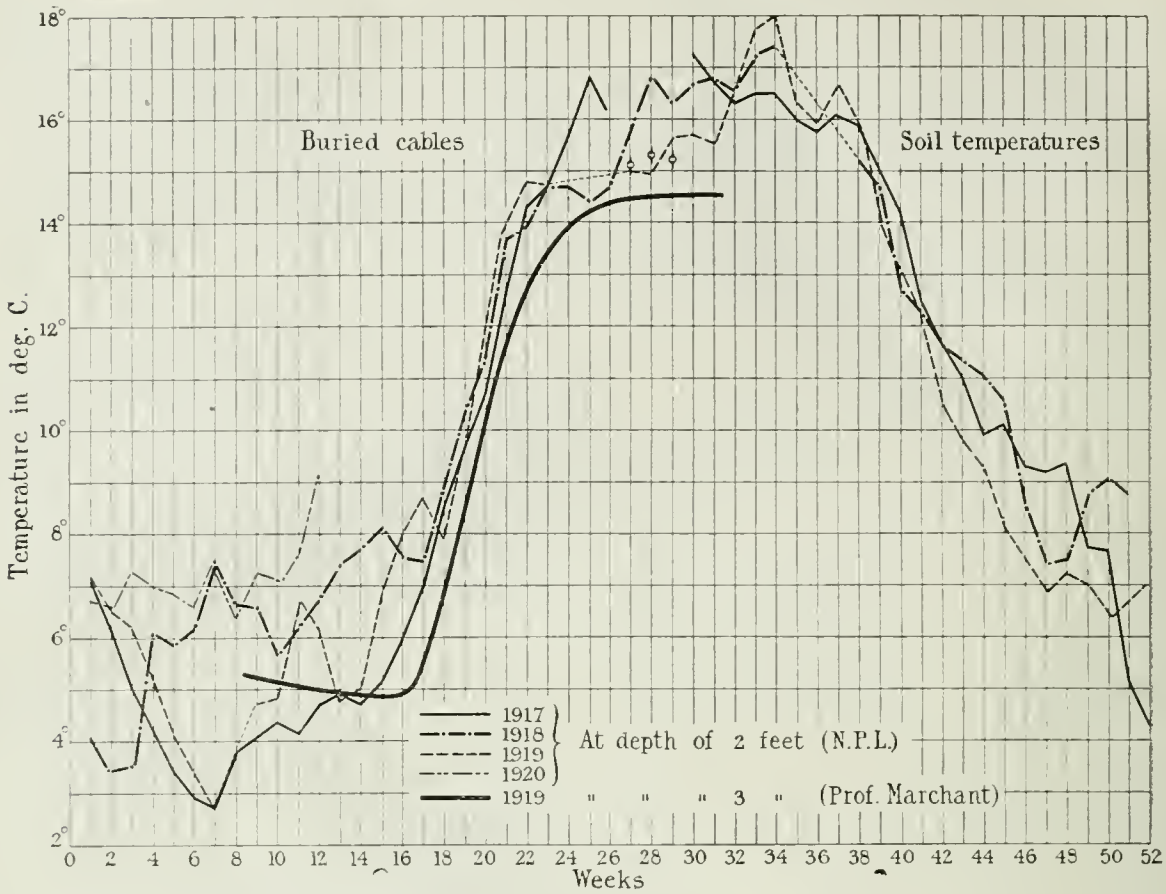

FIG. 18.—Curves showing temperature-rise of soil, at different depths.

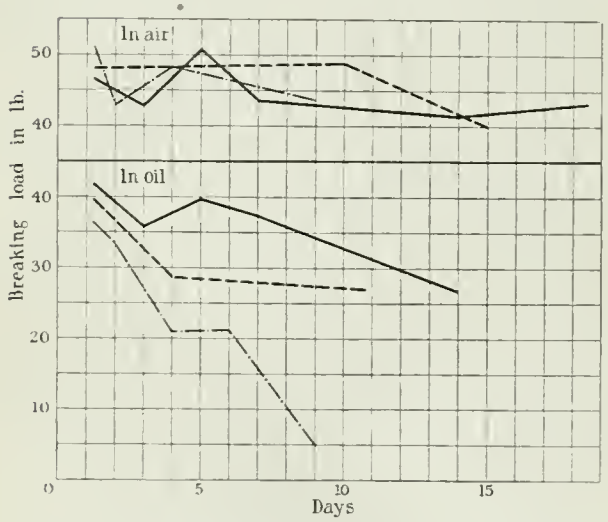

FIG. 19.—Curves showing preliminary tests on the breaking load of samples of paper kept at constant temperature in oil and in air.

been arranged that a series of determinations shall be made in Glasgow, Newcastle and Teddington, at periods extending over a complete year.

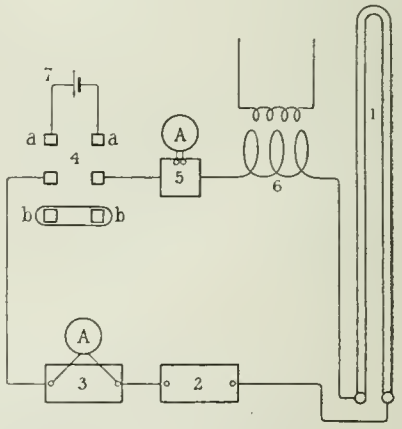

FIG. 20.—Diagram showing lay-out of cables for tests.

1. Cable.
2. Standard shunt.
3. Hot-wire ammeter and shunt.
4. Double-pole change-over switch.
5. Current transformer and precision ammeter.
6. Transformer.
7. Accumulators.

DETERIORATION OF THE DIELECTRIC.

The work hitherto carried out deals entirely with the rise of temperature in a cable, but it is obvious that, apart from this, there is the question of the maximum temperature which the dielectric will withstand without appreciable deterioration. An attempt

was made to investigate this point, and also the reason for the variable values of thermal resistivity, by making tests on strips of paper taken from the finished cable, but it was found that no definite results could be obtained with such samples. Pending the delivery of separate samples of paper and oil, a number of preliminary tests were made with one sample of paper to determine the factors that might affect the test conditions. The values given in Table 9 show in particular how important it is that the tensile tests should be made with the paper in a quite dry condition. Fig. 19 shows some results obtained with this particular sample of paper when exposed to various high temperatures.

TABLE 9.

Preliminary Tests on Paper to determine Factors that may affect Test Conditions.

Samples of the paper as obtained, without drying, were tested to determine the influence of rate of loading.

Rate of loading = 1 *lb. in* 10 *seconds :*

Breaking load = 32 lb., 34 lb., 34 lb., 33 lb., 37 lb. } Mean = 34 lb.

Rate of loading = 1 *lb. in* 20 *seconds :*

Breaking load = 36 lb., 35 lb., 36 lb. } Mean = 36 lb.

The slower rate of loading was used throughout the following tests.

Strips of paper as obtained, unimpregnated, 12 inches long, were put into oil and kept at a temperature of 120° C. ; similar samples were kept in air at the same temperature.

Time	Samples in Air *				Samples in Oil				
	Breaking load in lb.			Mean	Breaking load in lb.				Mean
After 1 day ..	51·0	51·5		51·25	35·0	40·0			37·5
After 2 days ..	45·0	42·0	40·0	42·5	39·5	27	32	41	35
After 4 days ..	49·5	49·5	45·0	48·0	(11)	20	23		21·5
After 6 days ..	49·5	45·0	46·0	46·8	15·0	29·5		18	21
After 9 days ..	39·5	45·5	44·0	43·0	5·5	9			7

* These samples were carried from the oven to the testing-machine in a desiccator.

Samples were placed in a desiccator to determine the effect of drying as distinct from heating.

Before drying : breaking load = 36·0 lb.
After 1 day : breaking load = 40·0 lb.
After 3 days : breaking load = 47·5 lb.

Samples in air were heated to 120° C. and kept in a desiccator till tested ; a test was made to ascertain how rapid was the change due to exposure to the air.

Straight from desiccator = 51·5 lb.
After 5 minutes = 48·0 lb.
After 15 minutes = 46·0 lb.

The temperature of 120° C. was merely a starting point for the experiments and is, of course, well above the normal maximum for a cable,

SECTION II.

REPORT, BY MR. E. FAWSSETT, MAY 1920, ON WORK AT NEWCASTLE-UPON-TYNE ON E.H.P. PAPER-INSULATED CABLES LAID DIRECT IN THE GROUND AT DIFFERENT DEPTHS.

SUMMARY.

An investigation has been carried out on the effect of depth of laying on the temperature-rise of a cable. Tests were made on three lengths of a 0·1 sq. in. three-core lead-covered and armoured low-pressure cable laid as shown in Fig. 22, at various times of the year, and more particularly during the winter months, as not only were the atmospheric conditions more favourable then, but it seemed to be the condition under which the cable would most probably be most heavily loaded. All the arrangements for making the tests and the precautions taken are outlined in a detailed description of tests below. The three depths decided on by the Sub-Committee were 1, 2 and 4 ft. from the ground surface to the top of the 2-inch concrete slab over the cable, corresponding to depths of $1\frac{1}{2}$, $2\frac{1}{2}$ and $4\frac{1}{2}$ feet approximately to the centre of the cable. In short, the results show considerable variation between tests on the 1 ft. and 2 ft. depths, but a fairly constant difference between those on the 1 ft. and 4 ft. depths. The average increase in temperature-rise for the same current for the cable at 2 ft. over that at 1 ft. is 4 per cent, and for the cable at 4 ft. over that at 1 ft. 15·7 per cent. This amounts to rating the 2 ft. cable at 2 per cent, and the 4 ft. cable at 7·5 per cent less current than the 1 ft. for the same temperature-rise on the core.

DETAILED DESCRIPTION OF TESTS.

The tests were carried out between 28th February, 1919, and 1st January, 1920, with the assistance throughout of Mr. Hubert Parry. All were in fairly

still weather and usually under what may be described as average winter conditions.

Tests were first made with the cable fed with a three-

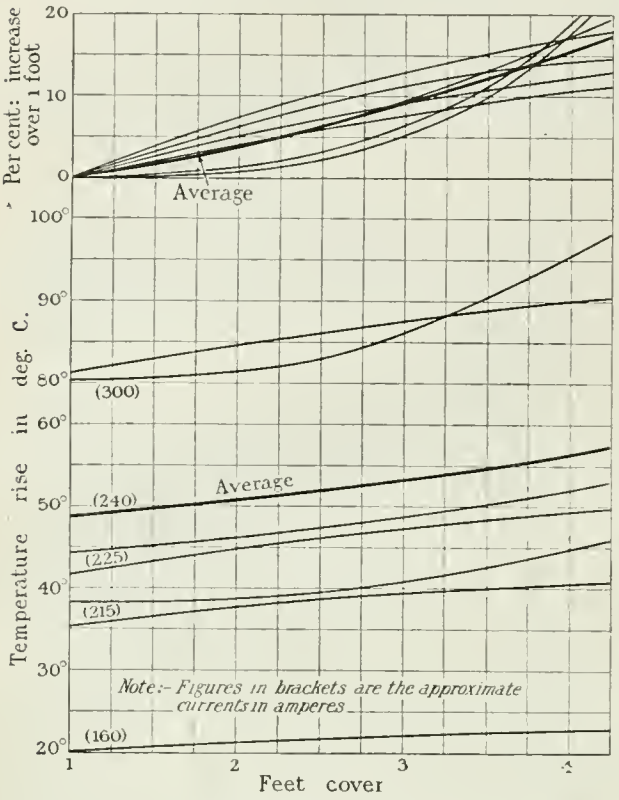

Fig. 21.—Curves showing temperature-rise with varying amounts of cover.

three lengths and all three phases in series, with a single return at the far end to the test-house. The results obtained in this way with direct current did not show any perceptible difference from those previously obtained with alternating current.

The diagram shows exactly how the cable was laid and connected up. The tapping points at "*a*" "*a*" gave a length of 100 feet of cable at each depth, and measurements of temperature-rise were made very accurately by referring the first section to a standard resistance in the circuit and the other lengths differentially to the first. This was done by means of a double bridge and a suitable arrangement of pilot leads terminating in large mercury cups.

The whole of the results obtained are given in Table 10, and are also plotted in Fig. 21, in which actual temperature-rises are shown as against depth, and at the top is plotted the percentage increase in temperature-rise with depth, over the rise of the 1 ft. cable. This shows clearly how the ratio of the rise of the 1 ft. and 2 ft. cables varies with the different tests, and it should be emphasized that this variation is not due to any uncertainty or inaccuracy in the measurements, as the quantities were such as to permit a relative accuracy of at least 1 in 1 000, or 1/20th degree C. The recognized formula was used for the relations between resistance and temperature-rise, taking into account the change in coefficient with change of basis temperature, and taking for the coefficient the average of the N.P.L. observations, viz. 0·0039, small variations being unimportant in this case.

The average curve, drawn somewhat thicker on the diagram, probably represents very fairly the average results to be expected from an armoured cable laid direct in the ground under average conditions in winter weather in a soil of average moisture content.

Table 10.

Test No.	Conditions: Date	Times, Start and Finish	Duration of test	Current	Ground Temp. 6 in. down	Weather Conditions	Results: Temperature-rises At 1 ft.	At 2 ft.	At 4 ft.	Per cent Increase 2 ft./1 ft.	4 ft./1 ft.	Remarks
			hr.	amp.	° C.		deg. C.	deg. C.	deg. C.	%	%	
1	28/2/19	10 a.m. 5 p.m.	7	160	1	Quiet, cloudy	20·25	21·25	22·75	5·0	12·35	—
2	3/3/19	5 p.m. 11 a.m.	18	215	0	Sharp frost	35·5	37·8	40·55	6·5	14·2	—
3	4/3/19	11 a.m. 8 p.m.	9	300	1	Quiet, clear, frosty	81·3	84·6	90·1	4·1	10·8	—
4	4–5/3/19	8 p.m. 4 p.m.	20	225	3	Quiet, cloudy	41·9	45·0	49·1	7·4	17·2	—
5	5–6/3/19	4 p.m. 4 p.m.	24	225	3	Quiet, cloudy	44·4	46·2	52·1	3·9	17·35	Equivalent to a run of over 70 hours
6	30/12/19	10 a.m. 5 p.m.	7	300	1	Quiet, cloudy	80·5	81·4	95·75	1·1	18·95	—
7	1/1/20	9 a.m. 3 p.m.	6	215	0	Quiet, bright, frosty	38·25	38·5	45·0	0·7	17·65	—
Average			14	240	1	Quiet, cold, average winter conditions	48·8	50·7	56·5	4·0	15·7	—

phase current as shown in Fig. 22, but it was afterwards found preferable to open the short-circuit on the three cores on the far end and connect the cable with all

Moisture Content of Soil.

Various samples of the soil were taken at different times while the tests were in progress, and the moisture

content was determined. This was never far from 20 per cent. The soil the cables were laid in consisted of made ground, ashes, builders' refuse, etc., having

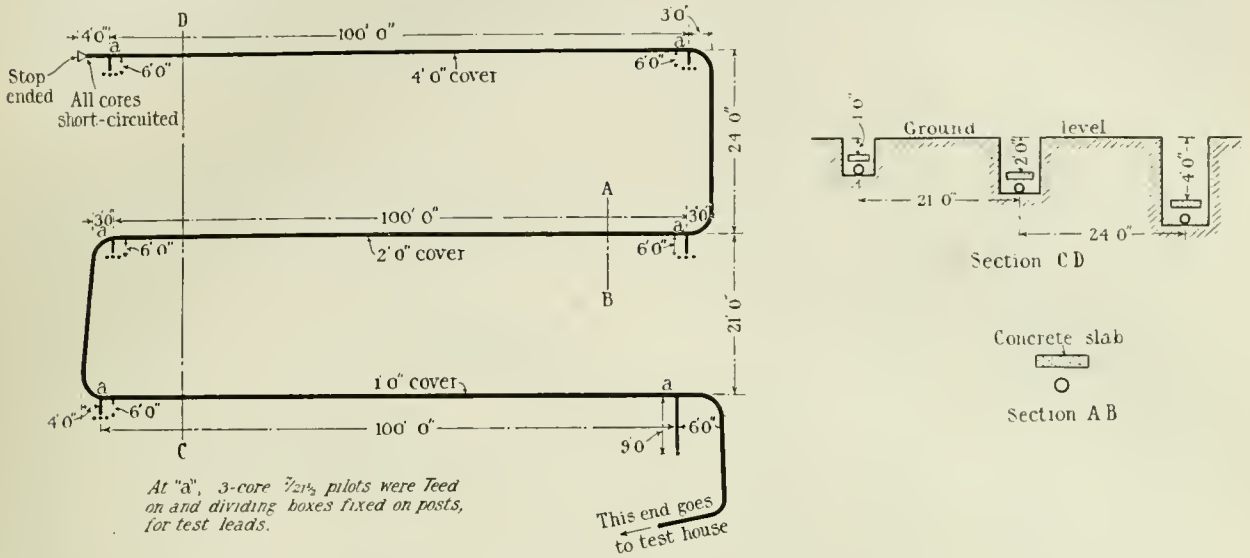

FIG. 22.—Diagram showing lay-out of cables for tests.

had some 40 years to settle. The three trial lengths of cable were in ground of closely similar composition.

SECTION III.

REPORT, BY PROFESSOR E. W. MARCHANT, ON WORK AT LIVERPOOL UNIVERSITY ON VULCANIZED BITUMEN CABLES LAID SOLID IN STONEWARE TROUGHING.

PARTICULARS OF CABLES, METHOD OF LAYING, AND GENERAL ARRANGEMENTS.

A set of four pairs of feeders with nominal sections of 0·1, 0·25, 0·4 and 0·6 square inch have been laid at a depth of about 3 feet below ground by the mains staff of the Liverpool Corporation. The general arrangement is shown in Fig. 23. The cable was insulated by two layers of fibre served on the conductor, then dielectric of vulcanized bitumen, then two layers of strong bitumen tape well compounded. The length of cable buried was 61 feet between the centres of the end pillars in each case, and was supported on porcelain bridges in stoneware troughs, filled with bitumen, and covered with flat tiles, the arrangements being the same as those employed by the Corporation authorities in their ordinary street work. The ends of the cables were brought out to street boxes similar to those used in connection with the tramway supply. The ground in which the cables were laid was a soft sandstone rock in which the moisture was usually found to be about 10 per cent. The ground was filled in and covered with 3-inch wood planks. As shown in the drawings, tapping points were connected to the cable. The tap wires were brought up through 3-inch pipes resting on the covering of the trough. The length of cable between points 1 and 2 (Fig. 24) was about 9 ft. ; between points 2 and 3, 2 ft. 3 in. ; between points 3 and 4, 2 ft. 3 in. ; and between points 4 and 5, 47 ft. 9 in.

The Kelvin double bridge method was used to measure the resistance of the cable and the resistance between the tapping points. The current was taken from cells in the early tests. It was regulated by hand, and measured by a Siemens millivoltmeter with a suitable series of shunts.

For the larger currents a special low-voltage 1 200-ampere generator was purchased by the Laboratory and, in all tests recorded, the current used was obtained from this machine. Current was regulated by a variable rheostat in the field circuit of the generator. Most of the " heat runs " in the preliminary tests were of about 6 hours' duration.

OBSERVATIONS OF TEMPERATURE COEFFICIENT AND OF GROUND TEMPERATURE.

It was suggested that the temperature coefficient of the copper in the cables might be measured by observing the resistance of the cable at the ground temperature, for different ground temperatures. The results obtained, however (see Fig. 25), did not give any definite relations between those quantities. In some cases the cable resistance was steady while the ground temperature, as read on a thermometer, varied. In other cases the ground temperatures were steady while the cable resistance varied. Again, variation in ground temperature occurs daily, and when a test was being made this variation had apparently no effect upon the cable resistance.

NOTE.—Mean ground temperatures at the depth of the cable (3 ft.) have been taken continuously (see note on Fig. 25). From these readings the ground temperature seems nearly constant at about 5° C. throughout

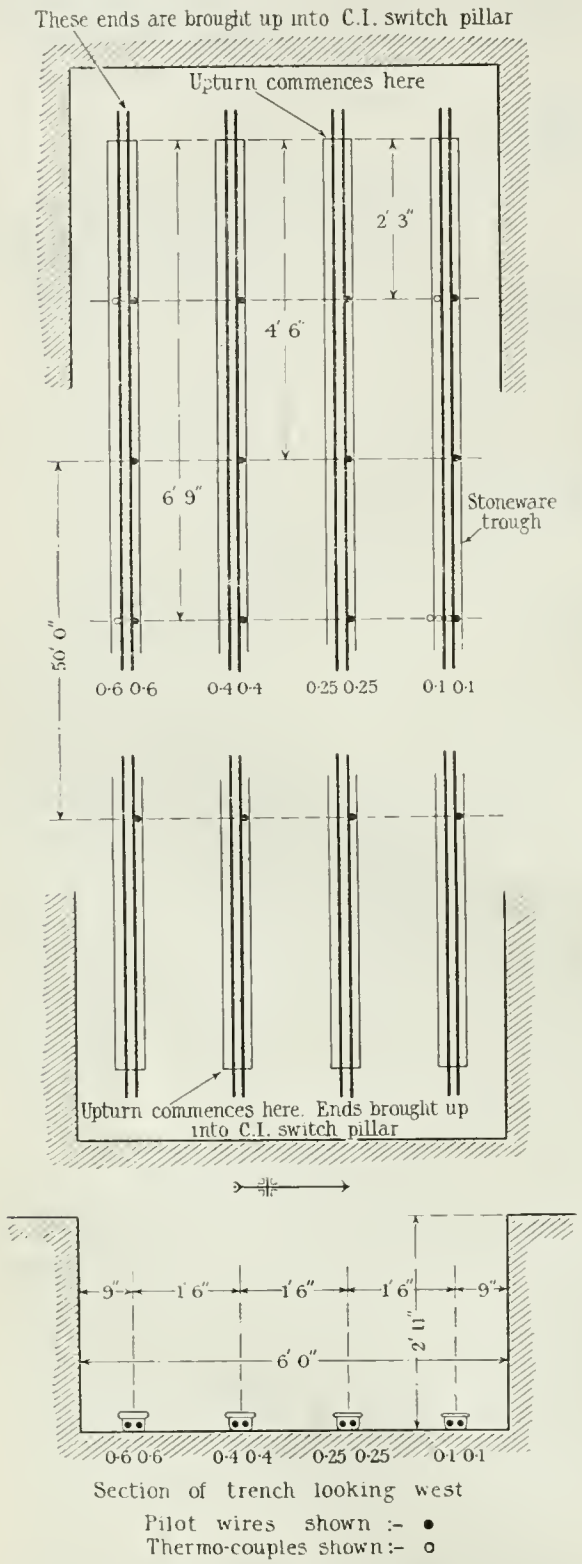

Fig. 23.—Details of cable trench.

the winter, until the middle of April. Then it rises steadily till at about the middle of June it assumes a nearly constant value of about 14° C.

Measurements were made of the temperature coefficient of the copper by taking sample strands from each end of the cables. The length of these strands was about 30 inches. The strands were soldered into

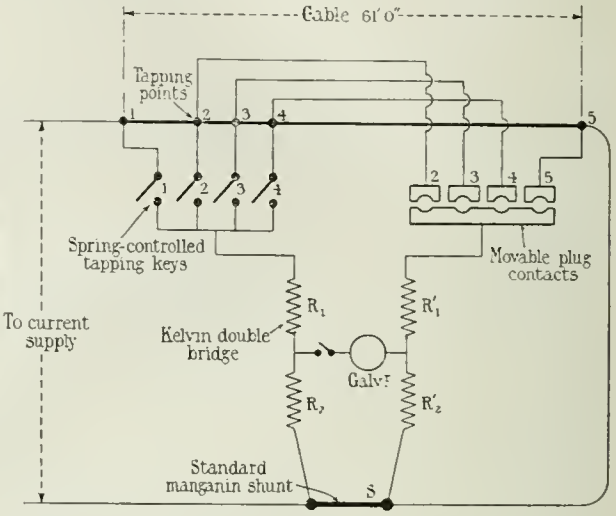

Fig. 24.—Diagram showing lay-out of cable for tests.

heavy copper leads, and the whole immersed in oil. The resistance was then measured by means of a Kelvin double bridge over a range of temperature from about 14° C. With suitable resistances in the bridge the temperature coefficients could be measured to about one part in 400. Table 11, containing these results,

Table 11.

Temperature Coefficient Measurements (Starting Temperature 10° C.).

Nominal Cable Section, sq. in.	Per cent Temperature Coefficient	Mean Per cent Temperature Coefficient
0·1	0·408 0·409 0·411	0·409
0·25	0·402 0·405 0·410 0·411	0·407
0·4	0·406 0·411 0 412	0·409
0·6	0·407 0·408 0·411	0·409

shows that for three of the cables the mean temperature coefficient was 0·409 per cent from 10° C., and this agrees with the value given by the British Engineering Standards Association for that temperature. The other

value, 0·407 per cent, on the 0·25 sq. in. cable differed from this by only ½ per cent. The differences between these values and those given by the British Engineering Standards Association are so small that it was considered better to adhere to the B.E.S.A. figures for the preliminary tests. Later, however, the actual figures obtained with each cable were used.

Method of Testing Temperature-rise.

A diagram of connections for a test run is given in Fig. 24. The shunts used were of manganin, and were compared with the N.P.L. standards. The resistance of a length of cable measured by two shunts agreed to within one part in 2 000 if the N.P.L. corrections were applied. The shunts were all designed to give 150 millivolts' drop when carrying their full current, and the same instrument was used for all the experiments. This was calibrated at frequent intervals and remained remarkably constant during the whole time occupied

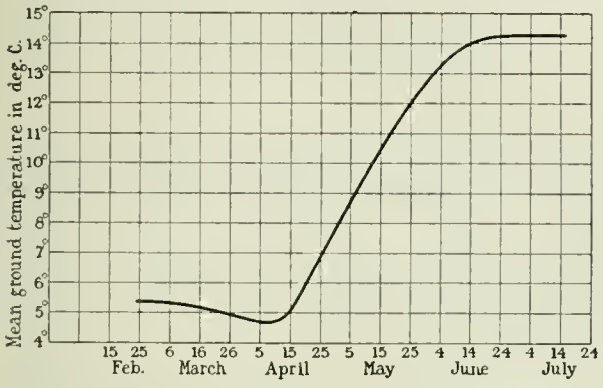

Fig. 25.—Curve showing mean ground temperature.

by the tests. The values of the coils in the resistance boxes used were checked from time to time against a standard resistance bridge. Although no special double bridge was purchased, the resistance boxes used to make it up were of exactly similar type and manufacture, and agreed with each other within one part in 10 000. The temperature-rise of the cable was estimated from the increase in its resistance. The ground temperature, or starting temperature of the run, was found by boring three holes, one at each end, and the other in the middle of the trench, to a depth of about 3 feet and lowering thermometers into these holes. The mean of the three thermometer readings was assumed to be the ground temperature. At the same time a sample of soil was raised from this depth and the percentage of moisture was calculated by weighing the soil, first wet and then after drying. It may be mentioned that the sandstone in this trench never contained less than 8 per cent of moisture. During the time that these tests were run, i.e. June 1916 to March 1917, the maximum amount of moisture was 13 per cent, the soil in this condition being non-friable.

When measuring the temperature-rise on a cable the method of procedure was first to measure the resistance between the tapping points marked in Fig. 24, 1–2, 2–3, 3–4 and 4–5 when the cable was at ground temperature. In making the tests, connection was first made between the movable plug contact (see Fig. 24) and the brass block connected with 5. The spring-controlled contact or key connected with 4 was then depressed, and the current switched on momentarily by closing the field switch of the generator. R_2 and R_2' were then adjusted simultaneously until there was no deflection on the galvanometer. The values of R_1 and R_1' were next altered so as to give suitable values for the resistances R_2 and R_2' when the resistance of the shorter section 3–4 had to be measured. After this was done, contact was made between the plug contact and 4, and key 3 was depressed; the current was again switched on and the resistances R_2 and R_2' adjusted until balance was obtained. The same process was repeated for the lengths 2–3 and 1–2.

In order to measure these resistances accurately, particularly the resistance of the short lengths, it was necessary to use a current of the same order of magnitude as that used in the actual tests, and it was important, therefore, that the current should not be kept on for too long a time, as the rate of increase of temperature of the cable was very rapid. The cable was allowed to stand for about 10 minutes after each of the preliminary trials, and was then re-tested, i.e. the resistances of 4–5, 3–4, 2–3 and 1–2 were found by the method above described, and the resistance of 4–5 was then re-determined as a check. The rise in temperature during the preliminary trials did not, as a rule, amount to more than 0·5 of 1 degree C., and was usually much less. A further check could be made by extrapolating the curve of temperature-rise, but this was not satisfactory as it was found that, during the first few seconds after switching on, the rise in temperature was slow, and it was only after 5 or 6 seconds that the steady rise in temperature began. There is an analogous effect in the temperature-rise curves of machines. As the effect is quite small, it was not deemed worth while to investigate it more fully.

In making measurements necessary for drawing "rise" curves it was found most convenient, before switching on, to set the two resistances R_2 and R_2' in the two variable arms of the double bridge, to a slightly higher value than that found in the preliminary trials. When the current was switched on, there was therefore a deflection, say, to the right, which gradually diminished, and the time at which the galvanometer spot crossed the zero on the scale could be determined with great accuracy. The resistances R_2 and R_2' were then increased and the time noted when the spot crossed the zero. The results so found give very good heating curves. This method of taking observations was only continued during the first half-hour of the run; after that period the readings of resistance were made at regular intervals, usually of half an hour, until the variation in resistance during the interval did not exceed about 0·05 of 1 per cent, that is, until the temperature-rise was about 0·125 degree C. per half-hour, when the total rise was about 20 degrees C. When the final temperature-rise was about 30 degrees C. the variation during the last half-hour was rather greater, about 0·5

degree C. per half-hour. The accuracy of reading with the bridge was such that the resistance could be found, if necessary, to one part in 10 000. This degree of accuracy was not necessary for most tests, as the variations in the current were of the order of ± 0·25 per cent, and would therefore correspond to variations in heating of about ± 0·5 per cent.

Several successive tests were made on the same cable under similar conditions, and the temperature-rises found agreed to within about 1 per cent.

Results of First Series of Tests.

Table 12 gives a complete record of the first series of tests made. In Fig. 26 has been plotted the temperature-rise against the actual watt loss per foot run of trough for the different sections of cable used. It will be noted how closely the points, for any given section, lie on a straight line. The results show, therefore, that the temperature-rise on any cable is almost exactly proportional to the power loss in it. The difference in the moisture of the ground surrounding the trough

Table 12.

Record of First Series of Tests.

Date	Current	Current Density	Time of Run	Ground Temperature	Moisture	Temperature rise	Final Temperature	Loss per ft. of Cable at Final Temperature
		Nominal 0·1 sq. in. Cable.						
	amps.	amp. per sq. in.		°C.	%	deg. C.	°C.	watts
8 January, 1917	300	3 000	5 hr. 30 m.	7·0	13·0	45·0	52·0	8·32
3 January, 1917	250	2 500	5 hr. 30 m.	8·0	11·7	30·0	38·0	5·48
23 January, 1917	200	2 000	5 hr.	5·5	10·4	18·0	23·5	3·34
5 January, 1917	150	1 500	5 hr.	7·8	10·7	9·1	16·9	1·84
31 January, 1917	100	1 000	5 hr.	5·0	8·75	4·1	9·1	0·789
6 February, 1917	100	1 000	5 hr.	4·5	—	4·5	9·0	0·787
19 February, 1917	70	700	4 hr.	2·2	11·1	4·5	6·7	0·378
		Nominal 0·25 sq. in. Cable.						
4 October, 1916	500	2 000*	7 hr.	12·5	—	43·6	56·1	9·71
21 February, 1917	500	2 000	5 hr. 40 m.	4·8	11·7	40·6	45·4	9·34
2 January, 1917	437·5	1 750	5 hr. 45 m.	7·8	10·8	30·8	38·6	6·95
18 December, 1916	375	1 500	6 hr.	5·0	10·6	21·2	26·2	4·89
8 January, 1917	312·5	1 250	5 hr. 30 m.	7·5	11·5	14·5	22·0	3·33
3 October, 1916	250	1 000	5 hr.	12·5	—	9·3	21·8	2·2
20 February, 1917	125	500	4 hr. 30 m.	4·5	13·2	2·2	6·7	0·501
		Nominal 0·4 sq. in. Cable.						
24 January, 1917	800	2 000	6 hr.	5·5	—	62·5	68·0	15·74
30 January, 1917	700	1 750	6 hr.	5·0	—	47·2	52·2	11·35
27 July, 1916	600	1 500	6 hr. 35 m.	13·5	10·0	35·0	48·5	8·33
7 February, 1917	600	1 500	6 hr.	4·0	10·0	32·9	36·9	7·91
31 October, 1916	500	1 250	6 hr. 37 m.	12·5	—	22·3	34·8	5·42
6 February, 1917	500	1 250	5 hr. 40 m.	4·0	—	21·2	25·2	5·26
29 September, 1916	400	1 000	6 hr. 10 m.	13·5	—	14·6	28·1	3·39
28 February, 1917	300	750	7 hr.	5·25	—	7·85	13·1	1·8
21 November, 1916	200	500	6 hr.	8·5	9·5	3·7	12·2	0·798
27–31 March, 1917	475	1 187	96 hr.	5·0	10·5	23·2	28·2	—
		Nominal 0·6 sq. in. Cable.						
22 January, 1917	900	1 500	6 hr.	5·25	11·0	45·8	51·0	12·85
29 January, 1917	750	1 250	6 hr.	4·0	10·7	31·2	35·2	8·4
2 October, 1916	750	1 250	5 hr. 30 m.	12·55	—	31·5	44·0	8·79
14 November, 1916	600	1 000	6 hr.	9·5	—	19·2	28·7	5·27
14 July, 1916	600	1 000	6 hr.	12·5	—	20·7	33·2	5·35
22 November, 1916	450	750	6 hr.	8·5	9·2	10·7	19·2	2·84
20 November, 1916	300	500	5 hr. 15 m.	8·5	12·2	4·5	13·0	1·24
22–29 March, 1917	600	1 000	58 hr.	4·8	11·2	22·2	27·0	—

* Old tappings with 7/14 S.W.G. tap wires.

appears to exercise very little influence on the heating of the cable. A thermometer placed in contact with the tile covering of the trough containing a cable giving

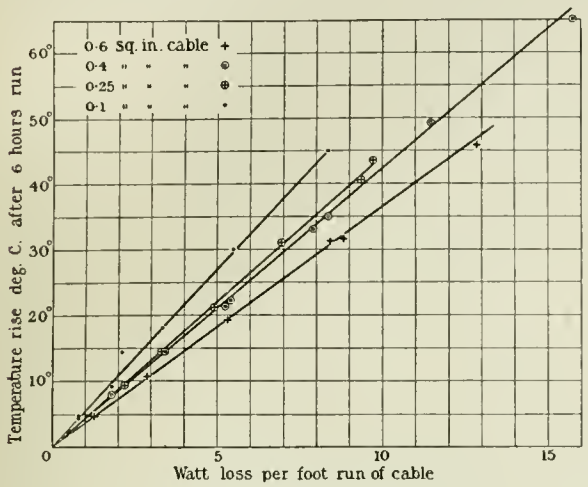

FIG. 26.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

a temperature-rise of about 20 degrees C. after a 6 hours' run, showed, after 24 hours' run, a rise of about 5 degrees C. above the surrounding soil. For longer runs in which the ground surrounding the trough becomes more thoroughly warmed, greater differences may be expected; but as these cables are usually run at densities of about 500 to 600 amperes per square inch or thereabouts, the results under actual running conditions would not, in all probability, be very different from those here recorded. The tests show that, within limits, the temperature-rise for any given size of cable can be estimated very closely from the rate of heat production in it, i.e. temperature-rise is almost exactly proportional to the power loss in the cable. If, therefore, a test has been made at a known current density for any given cable, the temperature-rises at other current densities can be found.

Curves of temperature-rise.—As stated above, for each test a series of figures was obtained from which it was possible to plot a temperature-rise curve for the cable under test.

In Figs. 27, 28, 29 and 30 the temperature-rise curves have been plotted for each of the four pairs of cables tested, the final rises in each case being nearly the same, i.e. 10 degrees, 20 degrees and 30 degrees C.

The object of these curves is to provide data from which the effect of momentary overloads can be calculated by the method already well known for determining the heating effect of intermittent loads on electrical machinery. It is, of course, unlikely that cables of this kind will be run at such densities as those em-

TABLE 13.

Showing Temperature-rises on End Sections of Cables.

Date	Current	Time of Test	Temperature-rise on Section			
			1–2	2–3	3–4	4–5
0·6 sq. in. Section. 7/14 Tap Wires.						
	amps.		deg. C.	deg. C.	deg. C.	deg. C.
2 October, 1916	750	5 hr. 30 m.	22·3	28·6	28·4	31·3
29 January, 1917	750	6 hr.	29·2	29	29	31·2
14 November, 1916	600	6 hr.	18·3	19	18·8	19·2
22 November, 1916	450	6 hr.	10·2	10·9	10·4	10·7
20 November, 1916	300	5 hr. 15 m.	4·4	4·2	4·4	4·5
22–24 January, 1917	600	58 hr.	20·1	21·6	21·6	22·2
0·4 sq. in. Section. 7/14 Tap Wires.						
5 February, 1917	500	5 hr. 40 m.	18·6	20·7	20·1	22·1
29 September, 1916	400	6 hr. 10 m.	12·2	12·9	12·4	14·4
21 November, 1916	200	6 hr.	6·1	4·1	3·9	3·7
27–29 March, 1917	475	96 hr.	19·4	22·5	22·0	23·2
0·25 sq. in. Section. 1/18 Tap Wires.						
21 February, 1917	500	5 hr. 40 m.	36·4	42·9	42·7	40·6
4 January, 1917	312·5	6 hr.	11·6	15·0	15·2	14·5
7/14 Tap Wires.						
3 October, 1916	250	6 hr.	8·5	7·0	6·8	9·1
0·1 sq. in. Section. 1/18 Tap Wires.						
23 January, 1917	200	5 hr.	18·0	20·2	20·2	18·0
5 January, 1917	150	5 hr.	9·5	10·2	10·1	9·1

ployed in these tests, except during rush hours, or on emergency.

End effect.—In order to find to what extent the cooling at the ends of the cables should be allowed for in making tests, the extra tapping points shown in Fig. 23 had been inserted. A schedule of the temperature-rises between these tapping points is shown in Table 13. For the two larger cables the tap wires were of 7/14 S.W.G. bitumen-insulated cables, and with these wires it was found that the rise on the end sections was considerably less than it was on the centre sections of the cable. This difference became much less for the longer runs when the bitumen surrounding the tap wires was thoroughly heated. There was, however, no measurable difference in temperature-rise between the points 2–3 and 3–4, though the rise on both these sections was slightly less than it was between the points 4–5. The difference was therefore attributed to the cooling effect of the tap wires themselves, and this proved to be the case. For the 0·25 sq. in. section, the original tap wires were

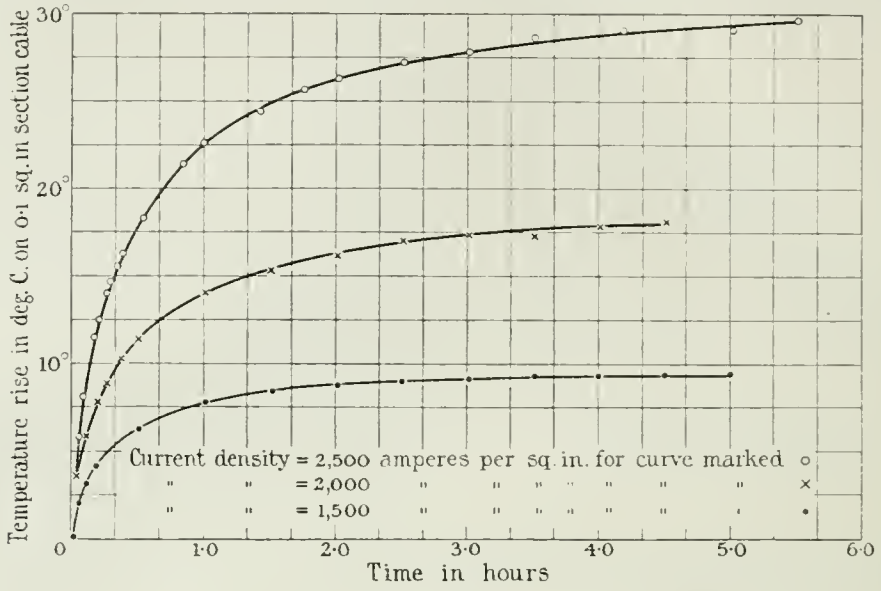
Temperature rise in deg. C. on 0·1 sq. in section cable
30°
20°
10°
0
1·0
2·0
3·0
4·0
5·0
6·0
Time in hours
Current density = 2,500 amperes per sq. in. for curve marked o
" " = 2,000 " " " " " " " ×
" " = 1,500 " " " " " " " •

FIG. 27.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

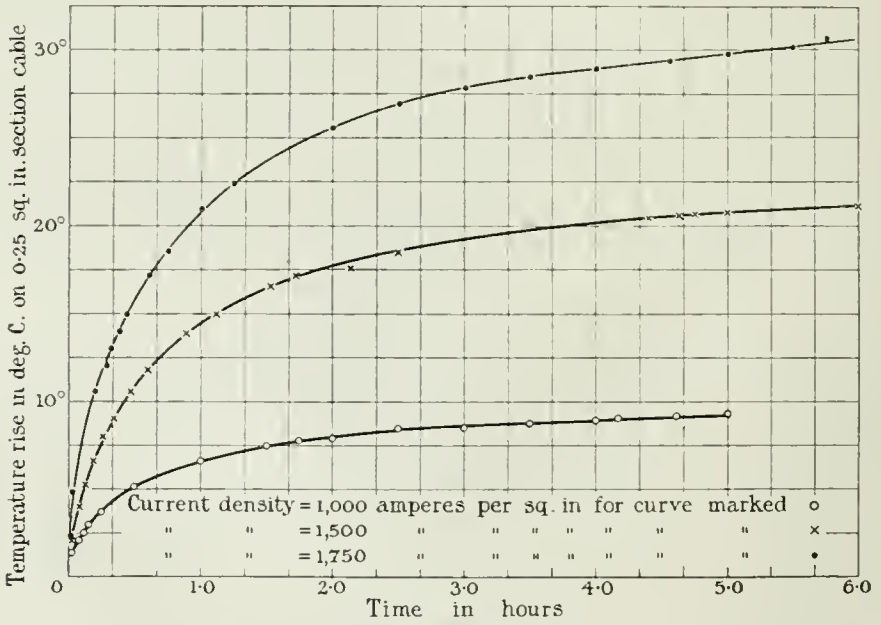
Temperature rise in deg. C. on 0·25 sq. in. section cable
30°
20°
10°
0
1·0
2·0
3·0
4·0
5·0
6·0
Time in hours
Current density = 1,000 amperes per sq. in for curve marked o
" " = 1,500 " " " " " " " ×
" " = 1,750 " " " " " " " •

FIG. 28.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

replaced by 1/18 S.W.G. wire (see Table 13). Under this condition the rises on the end sections were nearly equal to that on the middle section.

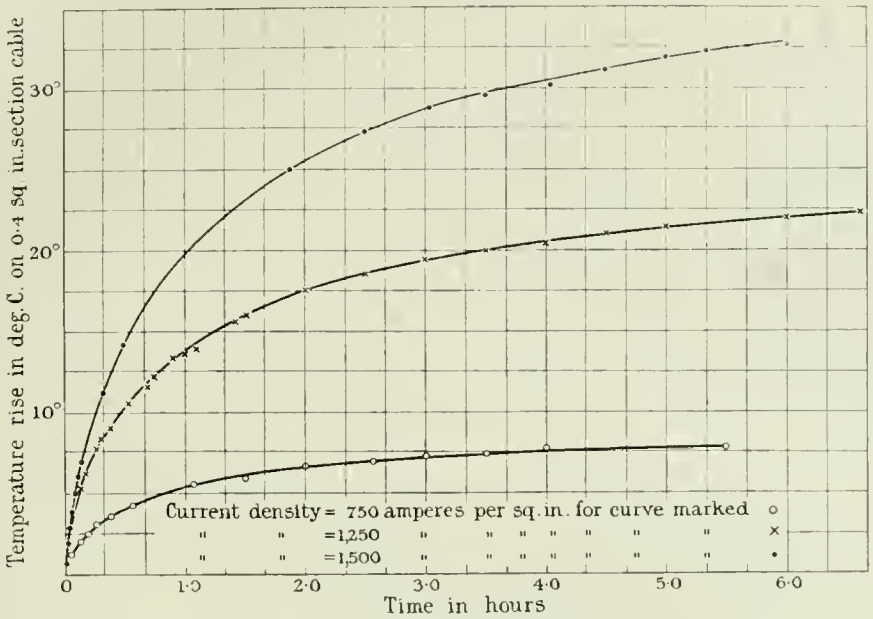

FIG. 29.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

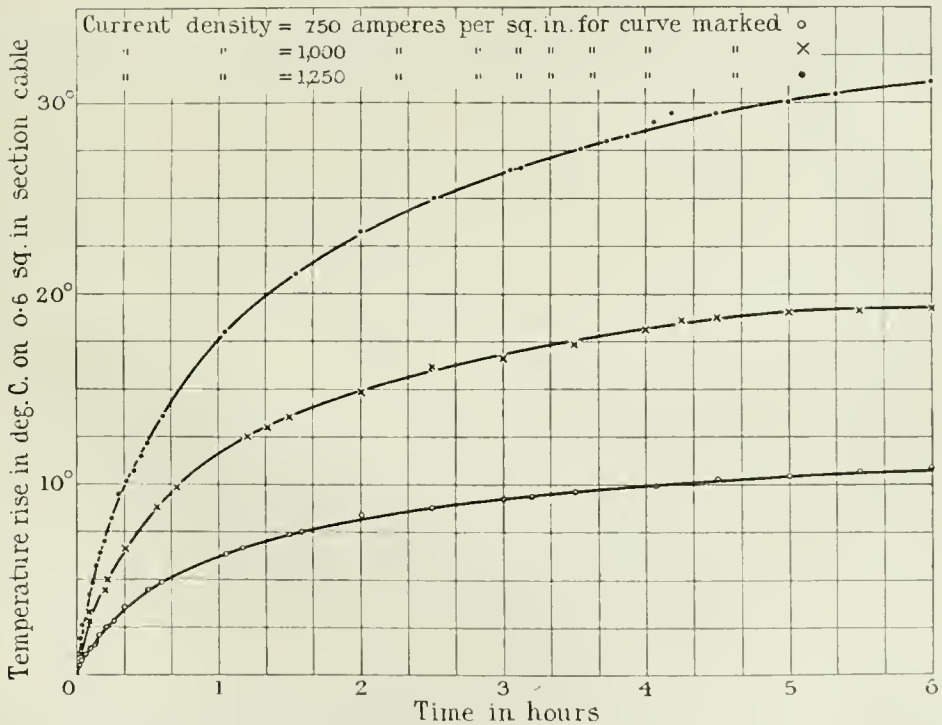

FIG. 30.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

The fact that the rise on 3–4 is the same as that on 2–3 shows that the cooling due to the ends of the cables, at this distance from the end, is negligible. A small correction has been applied, to allow for the cooling effect of the tap wires, when the differences were more than 2 degrees C., by assuming that the cooling effect extends inwards on the section 4–5 to the same extent as it does outwards on the end sections. This correction, however, in no case exceeds 3/10ths of a degree, and for all practical purposes may be neglected. For the smaller size of cable the end effect is negligible. The

rises on the end sections 2–3 and 3–4 are not quite the same, but in nearly every case the rise on the section nearest to the centre is rather less than on the outer section, showing that any small differences that occur are due either to slight modifications in the trough necessitated by the connection of the tap wires, or to experimental error. The observed differences are very little greater than those due to errors of observation. (With the larger currents the resistance between the tapping points 2–3 and 3–4 could be found to within one part in 2 000, corresponding to about 0·12 degree C., but for small currents the accuracy of observation was much less than this.) The temperature-rise on the outermost part of the cable, where it emerges from the ground through an earthenware pipe, depends on the atmospheric temperature. In the summer time, when the air temperature is considerably in excess of the underground temperature, the rise was found to be perceptibly greater on the end section than on the part of the cable laid underground. In the winter when the

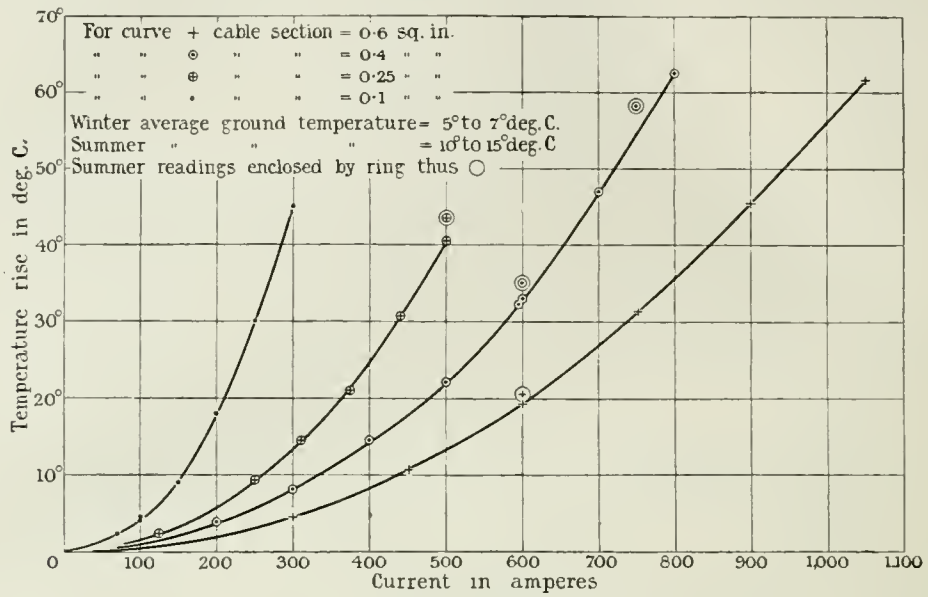

FIG. 31.—Curves showing temperature-rise (6-hour) of cables laid solid in bitumen in stoneware troughs.

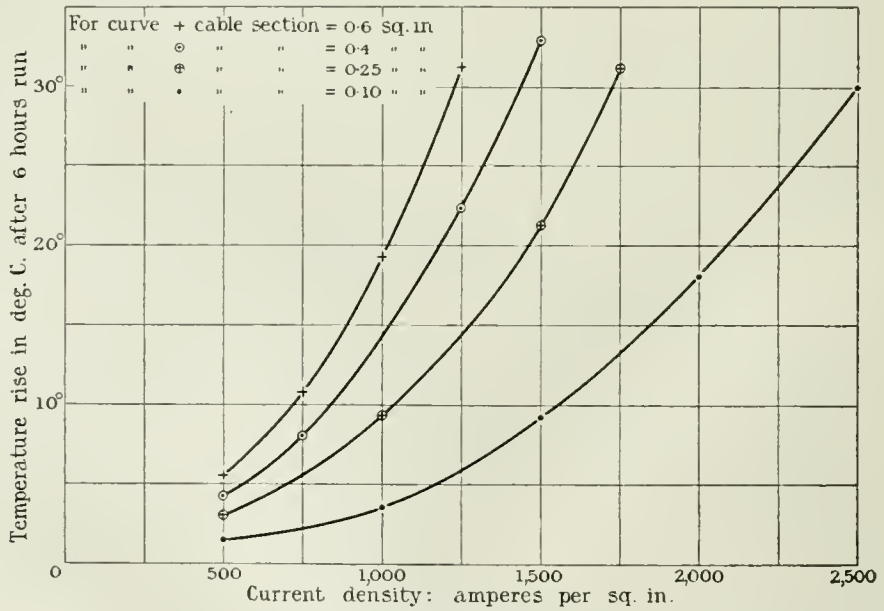
For curve + cable section = 0·6 sq. in
" " ⊙ " " = 0·4 " "
" " ⊕ " " = 0·25 " "
" " • " " = 0·10 " "
Temperature rise in deg. C. after 6 hours run
30°
20°
10°
0
500
1,000
1,500
2,000
2,500
Current density: amperes per sq. in.

FIG. 32.—Curves showing temperature-rise of buried cables laid solid in bitumen in stoneware troughs.

air temperature was usually lower than the ground temperature there was very little difference between the rises on all four sections.

Relation between rise (after 6 hours' run) and current density.—The rise in temperature on the four cables used (i.e. the temperature-rise after about 6 hours' run) against the current passing through them, has been plotted in Fig. 31. The curves are almost exactly parabolic, showing that the rise is very nearly proportional to the square of the current. For the purpose of comparing the carrying capacity of the four different cables, a further set of curves has been plotted in Fig 32 showing the relation between temperature-rise and current density. These curves show the much smaller temperature-rise obtained with cables of smaller section on given current densities, a fact which is well known for ordinary cased wires, and capable of simple theoretical demonstration.* The final rise is slightly less for the lower ground temperatures than it is when the ground is hotter. The most important factor in the cooling of cables *laid directly in the ground* is the moisture in the earth surrounding them. It was for this reason that the moisture was determined in these tests. It is shown in Table 12 that the extreme variations in moisture were from nearly 13 per cent to 9 per cent

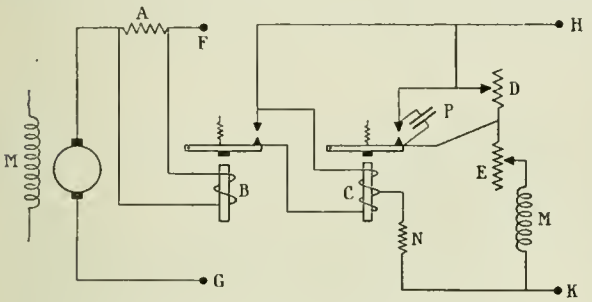

FIG. 33.—Diagram of Tirrill regulator connections.

A. Control shunt.
B. Selenoid operated by shunt A.
C. Differential relay.
N. Protective resistance for C.
M. Generator field windings.
D and E. Generator field resistances.
H and K. Generator excitation mains.
F and G. Main terminals of generator.
P. Condenser.

during a comparatively dry spell. The rainfall in Liverpool is fairly heavy (the normal value as given by the Meteorological Office is 28 inches), and there is usually a comparatively short period of drought in which the earth is able to dry. Conditions, therefore, were not favourable for determining the effect of variation in moisture on temperature-rise. The character of the troughing and its comparatively large heat capacity would lead one to anticipate that the effect of variations in moisture would not be very large except for long continued runs.

SECOND SERIES OF TESTS.

After the conclusion of the first series of tests, the 0·6 sq. in. cable was run for 58 hours at 600 amperes. The curve of temperature-rise for this run is shown in Fig. 37; the increase in temperature-rise at the end of this period, as compared with that after 6 hours, is nearly 20 per cent. A similar run was made with the 0·4 sq. in. section cable for 96 hours at 475 amperes (see Fig. 36), and a similar temperature-increase was observed in this case also. A further series of tests was therefore undertaken with a view to finding the ultimate rise in temperature after several days' run.

* MELSOM and BOOTH: *Journal I.E.E.*, 1914 vol. 52, p. 779.

Control of current.—The current for the tests was obtained as previously from the motor-generator set. In order to obtain good regulation, it was necessary to run the generator field at as high a saturation as possible, and therefore to vary the speed of the set. To do this the supply voltages for the motor, and therefore the motor speed, were chosen to suit each particular test.

The current in the cable circuit was kept steady by means of a Tirrill regulator (see Fig. 33), constructed by the British Thomson-Houston Co. The regulator is operated by a control coil connected across a low-resistance shunt in the main cable circuit. To set the regulator to operate at any desired current, the following procedure is adopted: The resistance D was short-circuited, and the resistance E was adjusted until the

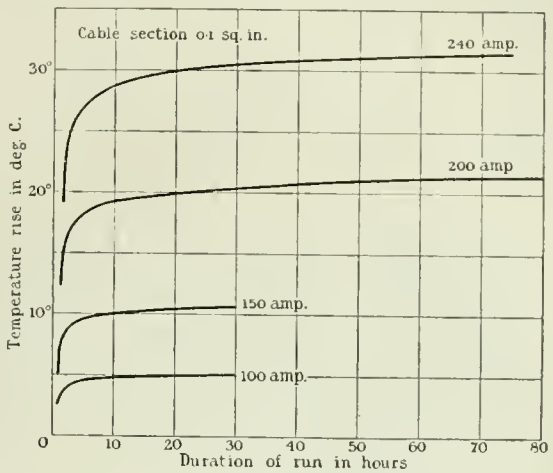

FIG. 34.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

current passing was about 20 per cent in excess of the required current. D was then put into circuit and adjusted until the current passing was about 10 per cent less than the required current. The actual values required depend upon the working conditions. The regulator was then finally made to work at the exact current required by altering the tension of the spring on the control lever over D. This spring was then fixed. The drop in pressure on the shunt connected to the control coil was about 0·25 volt.

Measurement of temperature-rise.—The temperature-rise of the cable under test was measured as previously by observing the increase in its resistance. A Kelvin double bridge was used for this purpose, and the arrangements were substantially those described above in Fig. 24. The standard resistance S served the double purpose of the standard for the bridge measurements and of the shunt for the millivoltmeter. This was in no way prejudicial to accuracy, as the reduction in the pressure-drop across the shunt due to the current in

the millivoltmeter was negligible, the resistance of the millivoltmeter being 1 ohm, and of the shunt either 0·0002 ohm or 0·0005 ohm.

The results have been worked out to show the ratio of the temperature-rises after a total time of 12, 24, 48, 72 and 96 hours, to the temperature-rise after a 6 hours' run, and are given in Table 14. It will be seen that the temperature-rise is very nearly the same after 96 hours as it is after 48 hours, i.e. the temperature of the cable reaches a nearly constant value after about 48 hours. The cable approaches its final temperature-rise more quickly when it is of small section, than when it is of larger section. It will be seen that the increase in temperature-rise between 6 and 96 hours is, for the 0·6 sq. in. cable, about 25 per cent of that obtained on the 6-hour rise, for the 0·4 sq. in. cable about 21 per cent, for the 0·25 sq. in. cable about 20 per cent, and for the 0·1 sq. in. cable about 19 per cent.

The accuracy of resistance measurement was one part in 6 000, giving an accuracy in temperature-rise of about 1/30th degree C. As the above ratios depend upon the difference of two quantities themselves nearly equal, and also upon the accuracy of the current meter and of the regulator which was not more than $\pm \frac{1}{4}$ per cent, they are correct only to within about 1 per cent.

TABLE 14.

Effect on Temperature-rise of Duration of Run.

Date	Nominal Cable Area	Current	Current Density	Total Duration of Test	Mean Ground Temp. during Test	Temp. Coefficient at Mean Ground Temp.	Temp. Rise after 6 Hours	Ratio of Temperature rise after Time given to rise after 6 Hours				
								12 hr.	24 hr.	48 hr.	72 hr.	96 hr.
	sq. in.	amp.	amp. per sq. in.	hours	° C.	per cent	deg. C.					
7/5/19	0·1	100	1 000	29·5	9·1	0·410	4·54	1·04	1·08	—	—	—
26/5/19	0·1	150	1 500	28·5	11·9	0·406	9·65	1·04	1·08	—	—	—
10/6/19	0·1	200	2 000	98·0	13·9	0·403	18·45	1·05	1·08	1·13	1·17	1·19
15/7/19	0·1	240	2 400	75·0	15·0	0·401	27·60	1·05	1·09	1·13	1·14	—
29/5/19	0·25	375	1 500	28·0	12·6	0·403	22·65	1·07	1·11	—	—	—
24/6/19	0·25	437·5	1 750	96·0	14·3	0·400	32·25	1·07	1·12	1·16	1·19	1·20
4/6/19	0·4	325	812·5	30·5	13·2	0·404	9·00	1·07	1·10	—	—	—
27/3/17	0·4	475	1 187·5	96·0	5·0	0·418	19·5	1·08	1·14	1·17	1·18	1·19
7/7/19	0·4	575	1 437·5	100·0	14·2	0·402	31·8	1·09	1·13	1·17	1·20	1·22
16/6/19	0·6	450	750	75·0	14·2	0·402	10·6	1·08	1·15	1·23	1·23	—
22/3/17	0·6	600	1 000	58·0	4·8	0·418	18·6	1·09	1·14	1·18	—	—
21/7/19	0·6	740	1 233	101·0	15·2	0·401	31·8	1·10	1·16	1·22	1·24	1·25

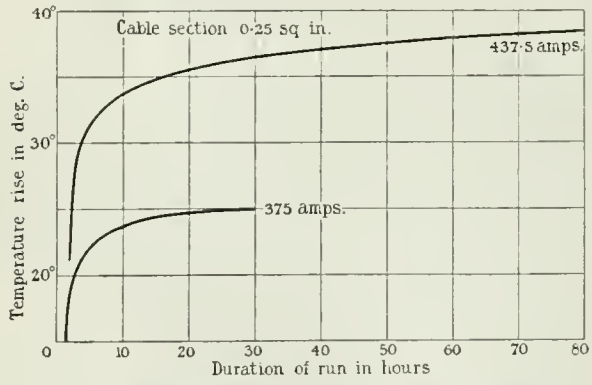

FIG. 35.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

The curves of temperature-rise for this series of tests are shown in Figs. 34, 35, 36 and 37.

It will be noticed that so long as the temperature-rise did not exceed 30 degrees C. the final rise was very nearly proportional to the watt loss per foot run in the cable, but that when the temperature-rise exceeded 30 degrees C. there was a slightly greater proportional

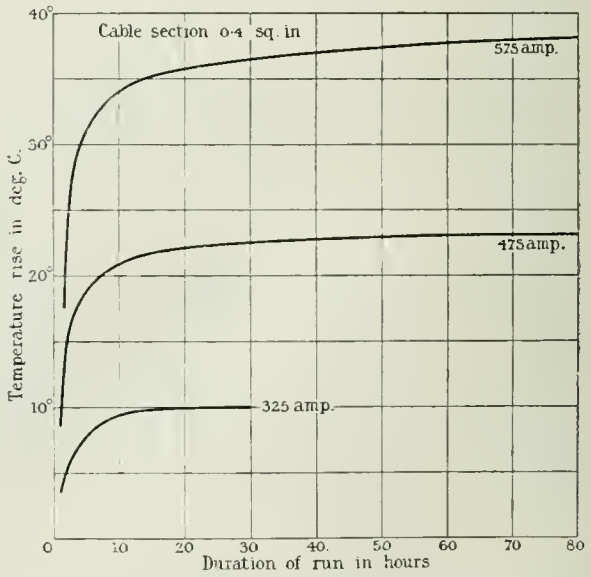

FIG. 36.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

rise, the temperature continuing to increase perceptibly after it had ceased to do so with the lower current densities. This result was probably due to the drying out of the ground in the neighbourhood of the trough and to the consequently increased thermal resistivity of the soil at the higher temperatures.

Theory and Determination of Thermal Constants of Insulating Material.

The simple theory for a cable buried in the ground given by Kennelly and quoted in a paper by Messrs. Melsom & Booth (*Journal I.E.E.*, 1914, vol. 52, p. 700), is clearly inapplicable to the case of a pair of cables laid solid in bitumen-filled troughs, since the isothermal surfaces must be irregular in shape at the points of contact between the troughs and the earth. In the original tests an attempt was made to trace out the

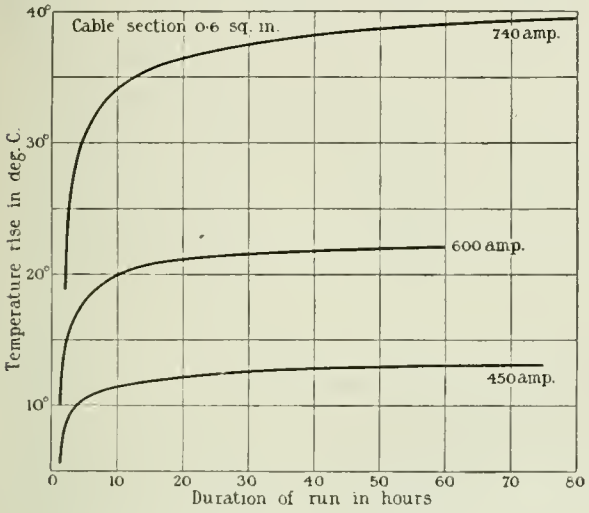

Fig. 37.—Curves showing temperature-rise of cables laid solid in bitumen in stoneware troughs.

temperature variation through the dielectric by means of thermo-couples, but the results were not satisfactory and, in the later tests, these observations were discontinued. There are clearly four factors which must affect the heating of the cable. The thermal conductivity of (*a*) the cable covering, (*b*) the bitumen in the trough, (*c*) the trough itself, and (*d*) the surrounding earth. The earth immediately above a trough carrying cables in which the temperature-rise was about 20 degrees C. was not more than 5 or 6 degrees C. above that of the normal earth temperature. The fact that there was little difference in cooling with varying degrees of moisture is therefore not surprising. An empirical relation is suggested for determining the heating of these cables. The thermal resistivity consists of two terms, one dependent on the cable, the other on the size and filling of the trough. Using the notation of Messrs. Melsom & Booth's paper, the heat transference may be expressed as

$$H = \frac{\theta}{(S + G)}$$

where θ is the excess temperature of the cable above the surrounding earth, S is the heat resistance due to the cable, and G that due to the trough and surrounding earth.

The curves in Fig. 26 show that the temperature-rise of each cable is proportional to the power expended in it over the working range of current densities employed. Hence the equation connecting current and temperature-rise at any given final cable temperature (t) may be expressed :

$$\frac{I^2\rho(1 + \alpha t)}{A} = K^2\theta$$

where I is the current passing, ρ the specific resistance of copper at 0° C., $\alpha = 0{\cdot}00427$, the temperature coefficient of increase of resistance at 0° C., A the cross-sectional area of the cable, and K a constant made up of two parts, one depending on the cable covering and the other on the trough and surrounding earth. Hence we may write :

$$I = K\sqrt{\frac{\theta A}{\rho(1 + \alpha t)}}$$

The values for K for the cables tested, after 6 hours' run, are :

148 for the 0·6 sq. in. section
142 for the 0·4 sq. in. section
136 for the 0·25 sq. in. section
124 for the 0·1 sq. in. section

where A, cross-section of cable, is in square inches, t is ground temperature $+ \theta$ in degrees C., and ρ, specific resistance, is in microhms per inch cube.

In determining the values of K from the observed results ρ has been taken $= 0{\cdot}63$ microhm at 0° C.

In order to determine the values of the thermal resistance for the covering and trough separately, some tests were made on single cable when surrounded by a uniform atmosphere of air. The cable to be tested was supported about 2 ft. above the ground by thin cords, and the temperature of the surrounding air measured by means of thermometers. The length of cable tested was about 9 ft., and the end effect was carefully measured (see Table 15).

Table 15.

Showing End Effect on Cable in Air.

Date	Nominal Section	Current	Temperature-rise		
			Outer 2 ft.	Inner 2 ft.	Outer 2 ft.
	sq. in.	amp.	deg. C.	deg. C.	deg. C.
17 April, 1917 ..	0·6	600	20·0*	20·6	22·4*
18 April, 1917 ..	0·4	500	18·3	20·5	18·0
3 May, 1917 ..	0·1	200	18·0	18·3	17·8

Total length of 0·6 cable .. 7 ft.
Total length of 0·6 cable .. 9 ft.
Total length of 0·1 cable .. 18 ft.

* These temperature-rises are due to heating of the brass lugs used in making connection to the cable. With the 0·1 cable the variations in temperature-rise are within the limits of experimental error.

The correction for end effect, even in these short lengths of cable, was comparatively small. The cable was run at such a density as corresponds to a rise of about 20 degrees C., and the temperature-rise on the core was measured, as in the other tests, by the increase in resistance of the copper core of the cable. In order to avoid the disturbance of the insulating material necessary if tap wires were sweated on, as in other tests, a hole was bored about 3/32 inch diameter through the outer covering as far as the inner conductor; into this hole was inserted a copper spike or spear made of No. 16 S.W.G. copper wire and shaped as in Fig. 38. The spike could be pressed firmly on to the core and kept securely in place by being tied through the loops. This method of tapping gave excellent results, the readings on the double bridge being steady and consistent. (It is necessary to use copper for the spikes to avoid the production of thermo-E.M.F.'s if the heating at the junctions is unequal.)

The temperature of the outer covering, after steady conditions had been reached, was measured by a thermometer, protected from draughts by a small cotton-wool pad, and placed in position for a sufficient time to obtain steady readings. The readings of the thermometer were taken at different positions along the cable and the mean value was determined. From the figures so found it is easy to calculate the thermal resistance of the covering material. The latter calculation is not quite exact as the covering is not homogeneous, but the result is sufficient for practical purposes. It was found during the tests that the temperature of the outer coverings, as determined by the thermometers, depended to a considerable extent on the time they were in contact with the cable. In each test this time was about 5 minutes and was approximately the same for all the tests, except in the case of the 0·6 sq. in. cable at 600 amperes on 17th April, 1917, when the thermometer was left in contact with the cable for 30 minutes. The figures given in Table 16 show the thermal resistance of the cable and troughing for the 0·1 and 0·6 sq. in. sections. It will be noticed that the thermal resistivity of the two sizes of cable is very nearly the same, though there is considerable variation in the value of σ for the 0·6 sq. in. cable. The general trend of the observations is to show that σ decreases slightly with rising temperature. (An apparent contradiction to this result is shown in the test already specified, where the method of measuring the temperature was different from that employed in the other tests.)

It is, of course, difficult to measure thermal resistivity by this method to a very high degree of accuracy; a difference of 0·5 degree C. in the external temperature of the covering makes a difference of 6 per cent in σ when the total rise is about 20 degrees C.

The cable covering did not show any serious signs of softening except in the case where the final temperature was 67° C. The working limit of temperature for this

TABLE 16.

Thermal Resistivity of Insulating Materials.

Date	Cable Cross-section	Current	Current Density	Initial Air Temperature	Final Air Temperature	Final Temperature	Tempera-ture-rise	σ	S	G (average)
	sq. in.	amp.	amps. per sq. in.	° C.	° C.	° C.	deg. C.			
17 April, 1917	0·6	600	1 000	9·9	12·7	33·5	20·8	487	43·3	
23 April, 1917	0·6	750	1 250	12·5	14·9	43·1	28·2	540	48·0	87
2 August, 1917	0·6	750	1 250	15·9	18·0	46·9	28·9	488	43·5	
30 May, 1917	0·1	300	3 000	16·3	19·1	67·2	48·1	507	73·9	
13 June, 1917	0·1	200	2 000	19·05	20·4	41·7	21·3	530	77·1	92
27 June, 1917	0·1	195	1 950	16·6	17·0	38·2	21·2	536	77·5	
10 July, 1917	0·1	160	1 600	17·1	20·1	35·2	15·1	531	77·0	

Thermal resistivity σ = difference of temperature in degrees C. required to cause the flow of one joule of heat per second * between opposite faces of a cm. cube.

Thermal resistance S = difference of temperature in degrees C. required to cause the flow of one joule of heat per second * per cm. length between core and outer surface of the cable.

Thermal resistance G = thermal resistance (in the same units as for S) for the trough filling and ground. G is here the thermal resistance of half the trough, since in these experiments two cables were laid in the same trough. For a single cable laid in a trough the value of G would be approximately half that given.

* One joule per second equals one watt.

FIG. 38.—Shape of copper spike used in end-connection tests.

material was therefore estimated to be about 50° C., a figure which is of the same order as that of 43° C. (110° F.) usually employed in specifications for this type of cable.

This work has been conducted at the Laboratories of Applied Electricity in the University of Liverpool under the general direction of Professor E. W. Marchant. The preliminary work and the general arrangement of the earlier tests were undertaken by Mr. F. J. Teago, M.Sc., the later tests with the automatic regulator were conducted by Mr. H. W. Jones, B.Eng., and the measurements of thermal resistivity by Mr. H. Berry, B.Eng.

Discussion before The Institution, 16 December, 1920.

Mr. E. B. Wedmore : This is the first Report published by the Electrical Research Association, and it deals with the research on buried cables which was initiated by the Institution. The expenditure on cables in the next few years will run into millions of pounds, so that I need not emphasize the importance of the research. It was pre-eminently a case where co-operative effort was required, and this has been made possible by the establishment of the organization which has now developed into the Electrical Research Association. This Research was controlled, in the first place, by a Committee set up by the Institution in London ; and later a second very active Committee was set up on the North-East Coast, which carried on the research during the difficult days of the war. We have now issued a Report with the object of obtaining the helpful criticism of members for our guidance, and also with the object of disclosing valuable data which have already been obtained and are capable of immediate application. The ultimate intention is to present certain tables with certain explanatory data and information, which will enable anybody to apply the results of the research to the various cases that arise in practice. Many parts of the work are not yet sufficiently complete to enable a report of that character to be prepared. I shall only touch lightly on the general character of the work which has been done. Experiments were made, in the first place, on a variety of cables exposed in the air, on the thermal conductivity of the materials, the electrical conductivity of the copper and the lead sheath, and, incidentally, data were obtained on the cable exposed in air. A number of these cables were afterwards buried in various ways, and tests were made on the buried cables. A comparison of great importance is made in the results obtained with the different methods of laying. It will be necessary to make detailed examination of the more complicated problems arising in the case where a number of cables are laid in parallel along a common route, and special attention will be paid to that part of the investigation at an early date, because it is one of those practical points on which reliable information is urgently required. On page 183 there is a schedule of the work which was outstanding at the time the Report was prepared. On the same page also, a number of matters are indicated on which it will be necessary to arrive at a decision before the research can be completed. There has been a general expression of opinion that the question of safe working temperatures for the various kinds of cables is most urgent. Work on deterioration and on the heating of multi-core cables will be kept in the front of the programme.

Mr. C. P. Sparks : Before referring in detail to the Report, I should like to say one or two words as to the importance of this particular research. Many supply engineers have their attention daily drawn to the importance of the works costs of big power stations, but the fixed charges in connection with transmission and distribution are sometimes overlooked. These charges are of ever-increasing importance, and it is only by really knowing the limiting temperature which the material will stand so as to give us a certain factor of safety that we can make the best use of this huge outlay on mains. If we can utilize the material to better advantage, it means a cheaper supply of power to the public, and that is, or should be, one of the main objects of such an Institution as ours. We are practically without knowledge as to the limit of temperature as affecting the factor of safety. Many of us feel that as a result of this investigation we shall be able to work at substantially higher temperatures in future without undue risk. That will lower the cost of supply. Each of the three sections of the Report is of especial interest. I am specially interested in that by Mr. Melsom and Miss Cockburn with regard to Table 6. The comparison of the three methods of laying cables, viz. solid in bitumen, armoured direct in the ground, and drawn into ducts, gives a very clear demarcation of the two methods in the one class, and the duct in the other. I feel that when we get a stage further, that is to say, when we group cables, we shall find a still greater discrepancy in favour of what one might term the solid systems. I understand that the curves in Fig. 12 are corrected for thermal resistivity, that for armoured cables being taken as standard. On page 197 of the Report Mr. Melsom says : "It will be noted that the current-carrying capacity of the cable is not affected by the thermal constants to the extent that at first sight might be expected." When we turn to Fig. 12 we find that, comparing the solid and the armoured systems, there is a difference of no less than 7 degrees C. in the constant temperature. This is very considerable, and the difference is wholly made up by the adjustment for the thermal resistivity of the cable. It appears to me that this point, which is of prime importance, is one for investigation by cable-makers, and I should like Mr. Melsom to let us know why he has, as it were, graded down this point. Mr. Fawssett has made, in my opinion, a very valuable contribution with regard to the rating of cables dependent on depth. We have to consider the question of the factor of mechanical safety, and, while it is of very great value to know that by laying a cable 4 feet

deep we are rating down the cable 7 per cent as compared with the rating for 2 feet, in the case of important cables we have to keep a certain definite distance from the surface in order to prevent mechanical interference. I very much appreciate the curve which Professor Marchant gives in Fig. 26, showing the temperature-rise on cables laid solid in bitumen in stoneware troughs. This curve is calculated from the watt loss per foot run of cable, showing the temperature-rise after a 6 hours' run. This method, namely, to plot the watts lost per foot run of cable, appears to me to be the proper way to approach the question of temperature-rise. Where we are dealing with a group of cables, everything will depend upon the watts lost per foot run, that is in cases where the cables are near one another. What we want to know is, how far apart to lay the cables, and what influence cables laid solid and close to one another would have on the curve in Fig. 26. The programme of investigation has grown, as the work of this research proceeds. For example, the question of dielectric hysteresis which was first considered here 20 or 21 years ago has become of importance, due to the use of higher pressures. Pressures are rising and, although when working with pressures of 6 000 to 10 000 volts the dielectric hysteresis is of small moment, yet with pressures of 30 000 and 40 000 volts and upwards, for which cable-makers are now constructing cables, this question becomes one of primary importance. On page 187 the question of temperature of conductors is referred to: "The final permissible maximum temperature of a paper-insulated cable is at present stated in the I.E.E. Wiring Rules to be 150° F." That figure was fixed when I was Chairman of the Wiring Rules Committee, not for buried cables nor for these large cables, but for the ordinary conductors used in houses. It was adopted as a safe figure. I hope that it will not be long before we are able to raise that figure, but it has no reference at all to these big underground conductors, and what we are relying upon obtaining from the Electrical Research Association within a reasonable time, is some pronouncement as to what is a safe working temperature for underground cables, and I am glad to hear from Mr. Wedmore that that is one of the points to which the activities of the Association will be specially directed.

Mr. C. J. Beaver: I have already contributed * to the discussion on this Report in Manchester, and will therefore endeavour to avoid repeating anything I said on that occasion. I should like to refer to the points raised by Mr. Sparks in connection with Table 6, where apparently the current-carrying capacity is found to be the same for cables laid solid and for similar cables direct. I am afraid that anyone drawing that inference—and I see no other to be drawn—is doomed to disappointment. There is bound to be a very considerable difference between the carrying capacity of a cable laid solid and one laid direct. One has only to take into account the thermal constants of the surroundings of the cables to see that that must be so. There is one other point which was mentioned by Mr. Sparks, i.e. with regard to the raising of the figure for maximum safe temperature. I should like to point out that there is some risk of not appreciating all the possibilities of danger in that connection, because paper and other organic substances may deteriorate fairly suddenly at some critical time according to the temperature to which they are heated. That is to say, for several hundred hours at the temperature (120° C.) mentioned in the Report there may not be any appreciable decrease in the strength of the paper, that being the criterion by which deterioration was judged in the test, but thereafter the strength may fall away very rapidly, indicating decomposition. That is very likely to happen; in fact, my own tests under similar conditions prove that it does happen in paper, and I mention it as a warning that tests should be taken over a sufficiently long period to make these figures quite reliable. Rubber is another case in point. We always refer to it as having a tendency to deteriorate naturally. In a given case with a given quality much depends on the temperature, and it would deteriorate less rapidly at a low temperature and more rapidly at a high temperature; that is to say, it largely depends on the temperature how long it is going to live. I would urge very strongly the other point mentioned by Mr. Sparks, namely, that the heating due to dielectric losses should receive first attention. It is, as Mr. Sparks said, a thing of primary importance to-day. I would point out that the manufacture of these extra-high-pressure cables has to be put on a very different plane from the manufacture of low-tension cables, and investigation of their behaviour should also be regarded as correspondingly important. It must be remembered that very high dielectric stresses are entailed in the design of these extra-high-tension cables in order to keep them within practicable limits of weight, diameter and cost, and these stresses cause dielectric losses which set up heating in the dielectric. Under present-day conditions of cable design these losses are inappreciable at working pressures below about 30 000 volts. They vary roughly as the square of the voltage, but in badly made cables are liable to vary much more rapidly with temperature, the loss/temperature curve having in some cases a very well-defined critical point at which they run up very rapidly. The avoidance of bad characteristics of this kind is a matter of the composition design of materials and methods of manufacture. When appreciable dielectric heating commences, the dissipation of the copper-loss heating is retarded and, in turn, the temperature of the dielectric goes up, and consequently its losses increase, thus creating a rapid cumulative effect resulting in the destruction of the cable.

Mr. B. Welbourn: The attention which has been paid to this subject by the Committee, and especially by the investigators, has been a measure of the extraordinary importance that the electrical industry attaches to this work. There is a certain amount of misunderstanding about this Report, which I should like to emphasize is a preliminary Report and is not in any sense as complete as the Committee had hoped to make it, because, as will be realized, the work has been carried out under war and post-war conditions, which made it extremely difficult to give attention to the work and to get the necessary samples and money

* See page 221.

with which to carry on the research. Mr. Sparks has quite rightly emphasized the importance of this work to the industry because of the saving in capital expenditure which it is possible to make. One speaker at Manchester mentioned 1 per cent, but I think that is an exceedingly low figure to take. I should hesitate to say what the figure is, but it is very much larger than that. The work is of tremendous importance because it must be considered that, from that point of view alone, about one-half of the money invested in the electric supply industry is invested in the outside work, whether in the form of underground cables or overhead work. There are one or two points in the Report which, if not quite final, at any rate approximate to final conclusions. The first is the superiority of the armoured cable laid direct in the earth as compared with the cable drawn into a duct. It is true that there are differences of opinion in regard to what cables laid solid will carry, but I do not think we need waste any time over the matter, because if the solid system is not already dead it is, I think, distinctly moribund. It will be seen from an examination of the tables and curves, that the armoured cable laid direct in the earth will carry something like 25 per cent more than the corresponding cable drawn into a duct. That point has not been commonly understood, except in certain large undertakings where particular attention is paid to the cable lay-out. Unfortunately, in many instances it is not always possible to take full advantage of this fact, because the cost of trench work and the re-opening of the ground make it imperative that a duct system should be used in some of our large cities. The point I am coming to is this: for the same current-carrying capacity in cables in ducts it is necessary to put in more copper than is required in armoured cables laid direct in the earth, and this extra copper should be about 25 per cent. Another point of very great practical importance, and one to which attention is already being paid in some networks, is the statement about varying the section of conductor according to the circumstances of the case, which Mr. Melsom makes. For instance, if a cable is laid through a patch of ground which is obviously of a dry nature it may be quite worth while, for these short distances, to put in a cable of larger copper section than obtains over the rest of the route where the cable is going through much wetter ground. He also calls attention to the necessity, in those cases where cables are suspended in air in substations or generating stations, to increase the copper section in order to get the full advantage from the copper on the main run of the cable. I should like to direct attention to Table 3, which deals with thermal resistivity. I believe that it will have a profound influence on cable manufacture, not only in this country but abroad. The necessity for reducing the thermal resistivity of cables is clearly brought out, and the supply authorities are going to benefit, and, in addition, I think there can be no doubt that, if attention is paid to this feature, other features, such as improvement in regard to dielectric loss, will automatically follow. In regard to Mr. Fawssett's work, I should like to refer to a table which he exhibited at Manchester, showing how one can arrive at the right average depth at which to lay cables. I hope that he will include that table in his reply to the discussion, because it is of an exceedingly informative nature; it shows that undertakers have correctly estimated the correct depth at which to lay cables, i.e. an average of about 2 ft. 6 in. There is one point in Professor Marchant's contribution to the Report which appears to establish the continuous working temperature of bitumen-insulated cables. He appears to give his approval to the continuous working temperature of 110° F., which is usually employed in specifications for this type of cable. The point needs to be emphasized because, although we are not considering them to-night, low-pressure bitumen cables are frequently used in colliery installations, where the conditions are usually hotter than in town installations, and it shows that the permissible safe rise that can be allowed must inevitably cut down the current-carrying capacity of the cable below that which is permissible in the case of low-pressure paper-insulated lead-sheathed cables working under the same conditions.

Mr. R. O. Kapp: I am particularly interested in the heating curves. It is pointed out in the Report that these heating curves are of value in estimating the extent to which buried cables may be overloaded, and in order that really definite conclusions might be come to on this subject it would be useful to be able to formulate some law from the heating curves. The theoretical law for temperature-rise plotted against time is generally taken to be an exponential one. I have therefore analysed a number of the heating curves which are given in the Report, to find out to what extent they approximate to an exponential curve, and I find that in some cases they approximate very closely indeed, whereas in some other cases there are departures of the order of 10 per cent, which would be rather too great a variation for accurate practical conclusions. It would be very interesting if, in the further work of the Committee, they would tell us under what conditions the heating of the cable follows or approximates to an exponential curve, and under what conditions it does not do so. I find that the curves at the end of the Report, given by Professor Marchant, depart most from the exponential curve, whereas Figs. 1, 3 and 4 at the beginning of the Report approximate much more closely. The only other fairly complete set of heating curves that I know of, were those shown by Mr. Beaver in the discussion on Messrs. Melsom & Booth's paper * on "The Heating of Cables with Current." I find that very nearly all those curves approximate very closely to exponential curves. To sum up, it seems that cables laid in air follow the theoretical law most closely, and buried cables depart most widely, and that is perhaps what one would expect, because the exponential law says that the heating curve is given by the formula, $y/T = 1 - e^{-x/C}$, where T = final temperature, Y = instantaneous temperature, x = time, and C = time-constant. This last depends upon the heat capacity of the cable, or whatever is being heated, and upon its power of dissipating heat. In the case of a cable in air these two factors should remain fairly steady

* *Journal I.E.E.*, 1911, vol. 47, p. 711.

and constant for all temperatures, but in the case of buried cables one would expect the earth around the cable to participate in storing heat as well as in dissipating it, and the longer the heating went on, the greater would be the temperature of the earth around the cable; in fact, the ground temperature would become, so far as the heating law is concerned, virtually equal to that of the cable. The effect would therefore be an ever-growing time-constant. The departure from the curve in the case of air is of the opposite kind, which one would also expect, because the radiation law is not a linear but a fourth-power law. It would be convenient if the heating curve for cables were logarithmic, and it may be that the explanation of these discrepancies is that, when the cable is laid in the ground and tests are taken on it, the ground is still cool, and, as the tests proceed, the ground gradually becomes heated up. It might be that a cable that had been laid in the ground for a few weeks would bring the ground to more steady temperature, and the heating of the cables above this average temperature of the ground would from then onwards follow the exponential law very much more closely. If the Committee could tell us something about that, I think it might be of practical importance. If we could obtain an exponential heating curve for each cable, which fitted the facts fairly closely, and kept on the safe side of the true heating curve, it would be extremely convenient for mathematical treatment. The exponential curve depends on one constant only, that is to say, if the time-constant is defined, everything about the curve is known. The alteration of any other constant involves a change in the position of the origin, but does not change the nature of the curve. One curve is an enlargement of another; the shape cannot be changed. In consequence, it would be possible, in the case of this Report, to define the heating curves by the time-constant instead of saying that an approximate uniform final temperature is reached after a definite period. That is unavoidable if the curve is of a vague nature, but it is not as scientific and practical as we might wish.

Mr. T. N. Riley: Although the work undertaken by the Committee is not completed, I think it is to be regretted that a summary of the results obtained to date has not been given in such a form that practical use can be made of it. In the form in which the results are given in the Report they are of value only for the particular cables tested under the particular conditions of test. In the preliminary Report in 1915 the two main stages in getting rid of the heat developed were clearly stated. These were the transfer of the heat from the core to the sheath, and the transfer of the heat from the sheath to the surrounding medium. In the present Report the first stage has been emphasized, and the second apparently neglected except in Professor Marchant's work in Section III. Values of temperature-rise are given for the whole cable, but they are not separated into two separate portions; one cannot tell what the temperature gradient was inside and outside the cable. Where the sheath temperature is given, the particular cable tested is not indicated, so that no deductions of general application can be made from the curves. For example, Fig. 3 refers to the heating of a 0·1 sq. in., 3-core 6 000-volt cable. There are two such cables in the list of those tested, one having a diameter of 5·66 cm. and the other of 4·3 cm. It is obvious that the internal and external temperature gradients would be very different in the two cases. Again, as Mr. Rosling has pointed out, in Fig. 2 the rise of temperature of the sheath is 70 per cent of that of the cores, whereas in Fig. 3 it is only 40 per cent. If one attempts to make use of Dr. Russell's formulæ to calculate the carrying capacity of a cable, as Mr. Melsom has done, one must first either assume or calculate the temperature of the lead sheath. I presume that Mr. Melsom calculated this on the basis that a given surface area of sheath would radiate a given amount of heat, and, if so, perhaps he will tell us what figure he took for the watts radiated per sq. cm. per degree C. rise. In Table 8 the relative carrying capacities of cables laid under various conditions and laid along the surface of the ground in still air are given. I suggest that, as temperature tests at the factory are likely to be required on high-voltage cables, and those tests will be taken with the cable coiled on a drum, the cable buyer would appreciate an authoritative statement from the Committee as to the relative heating of the cable coiled, say, in two layers on a perforated steel drum, and laid out along the ground. On the question of heating in ducts, some American engineers have found it worth while to increase the cooling of their cables by filling the ducts with crude petroleum jelly. The increased carrying capacity obtained avoided the necessity of installing extra cables, and the saving more than paid for the jelly.* Perhaps Mr. Wedmore will say whether the Committee contemplate making any measurements of this nature. In the report published in 1916, the opinion seemed to be that Teichmüller's formula

$$G = \frac{\sigma_e}{2\pi} \log_e \frac{4l}{d}$$

was most nearly accurate. I should like to know whether Mr. Fawssett's results confirm this opinion. If one assumes a sheath temperature and a value for σ_e, the variation of temperature with depth agrees well with Mr. Fawssett's results, if the two last curves, which he suggested at the meeting were unreliable, are removed.

Mr. J. R. Beard: As a member of the Sub-Committee I feel that the most valuable contributions to the discussion can come from those who have not had the advantage of being in touch with the investigators during the progress of their work, but I can perhaps usefully refer to the following four points in connection with the first two sections of the work with which I have been associated. First, there is danger of an implicit assumption by those reading the Report that cables should be run up to the maximum permissible temperature limits. This is a matter which has already been referred to in a previous paper this session, but I think it is desirable to mention it again, because there is every reason to think that it does not always pay to

* E. O. Schweitzer: "Increasing Current Capacity of Underground Cables," *Electrical World*, 1918, vol. 71, p. 613.

run cables up to their maximum temperatures, even if we definitely know what these are, except in the case of (1) so-called super-tension cables, (2) emergency overloads, and (3) where an undertaking is not in a position to raise capital easily at the moment, and wishes to carry on as long as possible with its existing network, a case which very often occurs in practice. Secondly, it is rather an extraordinary thing how readily engineers will agree to their lead-covered cables being loaded almost to their extreme limit. I and many of my colleagues are content to run fairly close to what we think is the safe limit, much more closely, if looked at from the point of view of factor of safety, than one would run with, say, the mechanical stresses on an overhead line. It seems to me that this is very largely due to there being no risk of injury to human life. It is a very satisfactory state of affairs, that one has got in a properly protected lead-covered cable a very safe piece of apparatus which, even if it does fail, is not likely to cause anything beyond ordinary material damage. Thirdly, I should like to emphasize the importance which should be attached to the results of tests on cables Nos. 17 to 22. As the Report states, these are tests on cables of the most modern type, supplied by different makers. Fourthly, in dealing with the relative carrying capacity of cables in ducts and cables laid direct in the ground, I would point out that there is a double disadvantage in the duct system in commercial practice. Not only is the steady temperature-rise greater for a given load, but also the rate of increase in temperature is greater than when the cable is laid direct in the ground, and these two effects are superimposed on each other in ordinary working conditions when it is very seldom that we get a constant steady load. Mr. Sparks pointed out that serious overheating of cables in ducts may occur due to ducts being generally run in clusters. Admitting that this may give rise to very serious heating, I do not think it is correct to say that the heating is *relatively* greater for grouped cables when they are in ducts than when they are laid direct in the ground with the same spacing, because, when a cable is laid in a duct, the greatest temperature-drop is in the air of the duct and not in the structure of the duct itself. I should therefore be inclined to anticipate that the difference in temperature-rise between cables drawn into a duct and those laid direct in the ground would be somewhat less on grouped cables than on single cables.

Mr. H. M. Sayers: I should like to suggest that to the work still outstanding might be added an investigation of the effects of expansion upon loaded cables. It is rather important in connection with plumbed joints. There have been a number of cases in which cables have cracked at the ends of plumbed joints, apparently due to repeated effects of expansion and contraction. I should say that in most cases those cables have been heavily loaded under compulsion. But it appears to me that the Association might very well investigate that matter and see under what conditions such excessive expansion and contraction effects occur, and how they may be prevented.

Mr. W. P. Digby: I should like to ask those who have carried out this research whether they have found any difference in the heating effect with paper-insulated cables when the paper has been impregnated to various degrees. If one pulls the ordinary paper-insulated 3-core cable to pieces, one finds that the paper itself has absorbed varying quantities of the oil which is used. There are, of course, occasional cases of cables the inner layers of which are not impregnated at all. Where, however, there is a thorough impregnation of the paper next to the copper, the oil medium has filtered through a large number of layers. The weight per square foot of the paper nearest to the conductor is also much less, and I think it would be of interest if the Association would state whether they are taking into account the method by which the paper is impregnated. I think that question refers equally to the preliminary tables which are put forward on page 189. These tables deal with strips of paper which have been kept in oil. The results obtained from the tests would be quite true in the case of a cable which had been insulated with paper separately impregnated before wrapping, but I doubt whether that would hold good for the inner layers in the case of the other type of paper-insulated cable which is impregnated as a wound core. Several years ago I made some experiments on the hygroscopic susceptibility of different wrappings of the ordinary paper-insulated cable, and I found that there was a distinct variation in the amount of moisture that could be absorbed in a vapour bath between the outer and the inner layers of paper.

Mr. G. L. Addenbrooke: After the paper by Mr. Mordey (*Journal I.E.E.*, 1901, vol. 30, p. 364) there was a good deal of agitation amongst those connected with electric power supply in regard to the effects which he described, and a certain number of tests were carried out and published by Professor Ayrton, I think, for the British Insulated Wire Company. About that time I was working on the electrostatic method which I had brought out for power purposes, and I was approached by Messrs. Henley's, who asked me if I could make a set of tests for them, and they stated that they could offer favourable conditions. I think they had about 9 miles of cable then in stock, which was being made for the London United Tramways Company. The result was that, after discussing the matter, a series of tests was made. Those tests, I think, were fairly accurate, and I am under the impression that some of that cable could still be traced. About a year afterwards, in connection with some work on large inductive resistances which I was making for Messrs. Willans and Robinson, it was arranged with the British Electric Transformer Company that a very large transformer with a variable air-gap should be tested at Wood-lane. After this was done, as the instruments were all there and ready for use, I asked Mr. Miller if he would have any objection to my testing one of the feeders, which I believe served Kensington Court. It was a 3-core cable, laid in the ground, and a set of tests was made on it under working conditions. It was connected to one of the generators that usually supplied current to it, and I presume that it has been under pressure, if it is still there, night and day since that time. I checked the tests recently, and I cannot see anything wrong with them, and I have still the arrangements and instruments by which they were

made, so the instruments could be tested by some other method to detect any percentage error. Those experiments could now be made over again if it was of sufficient interest to cable-makers and central-station engineers to know what had happened on such a cable during a period of 16 to 18 years' use.

Mr. P. Rosling: The term "temperature of cable" is often used, and I think it is not generally considered by users that this term should apply to the temperature of the conductor and not to that of the lead sheath. In low-tension cables this has not been an important point, but with high-tension and super-tension cables it is of considerable importance; it will be seen that in Fig. 4 a difference of 11 degrees C. for both the 6 000-volt and 20 000-volt cable is shown. This figure is also interesting in showing that a 20 000-volt cable has a lower temperature-rise than the 6 000-volt cable. These figures are for cables tested in still air. There is apparently nothing in the I.E.E. Wiring Rules to denote that the temperature of wires and cables is the temperature of the conductor and not the temperature of the external covering. I should like to raise a protest against the extraordinary confusion of terms in the formula on page 196 for arriving at K. The temperature of the core, and that of the sheath, are referred to, but R is given as the resistance of the conductor. The inner diameter of the lead sheath is given as d_1, and the outer diameter of the core as d_2. Then in the formula for concentric cable, d_2 and d_3 are given as the outer and inner diameters of the conductors. If the term conductor were adhered to for the actual copper, and the term core for the conductor after it is insulated previous to any other process, this confusion would be avoided. I think the most important point to be investigated is the thermal resistivity of the soil immediately surrounding cables. When the cable has been laid and in commission some little time, experiments should be carried out in different soils and under different road and footway surfaces. Information on this point will be far more valuable than to determine dielectric losses, which can usually be given by cable-makers for the different types of cables.

Mr. P. Dunsheath (*communicated*): At the present time there is a good deal of confusion in the minds of many engineers on this question, and there is a real need for a simple treatment of the subject which will enable the practical man to grasp the principles and apply them to his everyday problems. The Report, as it stands, lacks this fundamental basis. The obvious treatment, and one which has received attention from the American writers on the subject, is to consider the temperature-rise of the conductor as being made up of additive components in series. If we look upon the heat as flowing along this composite path, we see that the electrical circuit forms a useful analogy, in which the flow, or current, is replaced by the amount of heat generated in the cable per unit length, and the potential drop by the temperature difference. The third factor in what might be termed the "thermal Ohm's law," is the thermal resistance, which may be obtained from known dimensions and experimentally determined constants. In the case of an armoured cable laid direct in the ground, the heat generated in the conductor flows along the following path in escaping: dielectric—lead—inner serving—armouring wires—outer serving—earth in contact—to a part of the earth which is taken as the base for temperatures. The heat path for a lead cable in air is as follows: dielectric—lead—air. Values of the specific thermal resistance of dielectrics and soils have been obtained and published by various investigators, and several formulæ, such as that used in the Report, exist for the calculation of the thermal resistance of different parts of the circuit, so that, treated on this basis, the question of research on the heating of buried cables resolves itself largely into a determination of the values of thermal resistivities, the confirmation of existing formulæ, and the evolution of new and more generally applicable formulæ. As the Report ignores this fundamental aspect of the matter, I submit the following simple basis which I believe to be helpful in the consideration of what is a fairly compli-

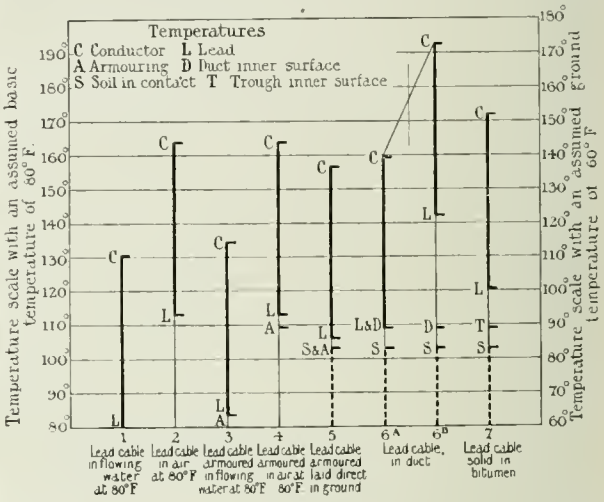
Temperatures
C Conductor L Lead
A Armouring D Duct inner surface
S Soil in contact T Trough inner surface
Temperature scale with an assumed basic temperature of 80° F.
Temperature scale with an assumed ground temperature of 60° F.
1 Lead cable in flowing water at 80° F
2 Lead cable in air at 80° F
3 Lead cable armoured inflowing water at 80° F
4 Lead cable armoured in air at 80° F
5 Lead cable armoured laid direct in ground
6A Lead cable, in duct
6B
7 Lead cable solid in bitumen

FIG. A.—Method of determining the temperature-rise of a 0·2 sq. in., 3-core E.H.T. cable, under different conditions.

cated subject. In Fig. A, the vertical lines represent the temperature distribution over the various portions of the composite path mentioned above for a cable under various conditions. The lengths of the lines are only approximate, but are based on calculations for a 0·2 sq. in., 3-core E.H.T. cable. All the lines refer to a cable which is the same as far as the manufacture up to the lead-covered stage is concerned. The current, which is the same in every case, was first determined from thermal considerations based on a temperature difference between the conductor and lead sheath of 50 degrees F., and this temperature difference is represented by the thick line CL in every case. On the left-hand side of the diagram is a temperature scale commencing at 80° F., an assumed basic temperature, and the top of the line in every case gives the maximum temperature of the conductor. When a lead cable is laid in water, particularly if the water is circulating, the lead assumes the water temperature, and the only rise of temperature of the conductor above that of the water is that across the dielectric

between the conductor and lead. These conditions are shown in the vertical line No. 1. With the same cable in air, there is a considerable difference in temperature between the lead and the ambient air, but, for the same current as in the first case, the temperature-drop across the dielectric remains unchanged, so that the total rise is as shown in line No. 2. Lines Nos. 3 and 4 show the effect of armouring, and the curious fact is evinced that, as the outer surface of the armoured cable is greater than that of the lead, the decrease of the resistance between surface and air may compensate for the additional resistance of the servings under and over the armouring. When a cable which has been tested in air is transferred to the ground, the heat dissipated raises the temperature of the soil in contact. This temperature-rise is shown by the dotted portions of lines Nos. 5, 6A, 6B and 7, and the other temperature differences must be added to this in order to obtain the maximum conductor temperature. Also, as the basic temperature of the ground may be taken as less than that of air, a new temperature scale is

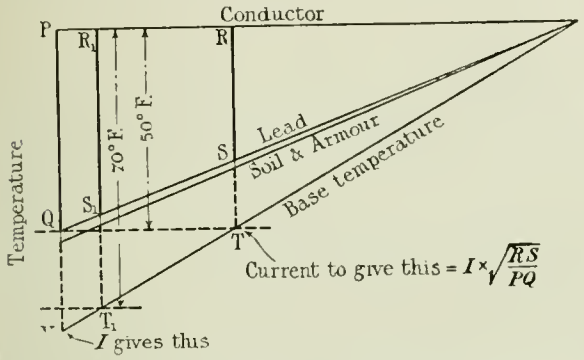

FIG. B.—Method of determining the temperature-rise of an armoured cable laid direct in the ground.

required. The one on the right is drawn on an assumed ground temperature of 60° F., and cases Nos. 5 to 7 should be referred to this scale. In most cases the thermal resistances, and consequently the thermal potential drops (temperature differences) can be added as shown, but, in the case of cables drawn into ducts, the case is different. Lines Nos. 6A and 6B indicate the temperature distribution in two directions, A downwards, with the lead in contact with the duct, and B, upwards, with the air intervening. The actual maximum temperature of the conductor probably lies somewhere between the two values along the sloping line, as shown. To carry the matter one step further, having built up the temperature line for any set of conditions in this way for the assumed one, and having so arrived at a maximum temperature which is considered unsafe for the cable, it is necessary to evaluate the current which will give a safe temperature-rise under the assumed conditions. This can be done as shown in Fig. B, where a temperature ordinate from the previous figure is plotted on the left-hand side, the case of an armoured cable laid direct in the ground being chosen as an example. The sloping lines are drawn to any arbitrary point on the horizontal line of maximum temperature, from the various points in the vertical temperature line PV. The vertical line RT, where the outside sloping line intersects a horizontal one for the permissible temperature difference (50 degrees F. in the first case), gives the new temperature distribution. Then, as the temperature-rise of any system is proportional to the square of the current, the permissible current for the cable under the conditions given is obtained by multiplying the original current (I) required to give a difference of 50 degrees F. across the dielectric, by $\sqrt{(RS/PQ)}$. As shown above, the basic temperature of the ground may be taken as 60° F., so that, for a maximum temperature of 130° F., a buried cable may be allowed a 70 degrees rise, compared with a 50 degrees rise for a cable in air. The construction line in this case is $R_1S_1T_1$, and the permissible current is $I\sqrt{(R_1S_1/PQ)}$. This diagram is instructive, as showing the two-fold effects of burying a cable. In the first place, owing to the heating of the soil, the permissible current is reduced (line RST); but owing to the lower initial temperature of the soil this value is increased (line $R_1S_1T_1$). If the initial temperature of the soil were much lower than 60° F., the ratio R_1S_1/PQ could be greater than unity, in which case the geometrical construction would have to be carried out on the sloping lines produced to the left of the vertical temperature scale. It is only by a consideration of the separate component factors that the safe current for any set of conditions can be predicted, and the value of the results of this research will depend largely on their application along such simple lines as those which I have indicated.

Dr. A. Russell (*communicated*): The data published in the reports will be useful to the electrician and instructive to the student. It is very satisfactory to find that the values of the thermal resistivity of paper dielectrics is very approximately constant. Doubtless, if the investigators had been able to take into account in their formulæ the fact that the cores of the cables were stranded and that they had a "lay," even more satisfactory results would have been obtained. It is interesting to notice that these problems, which have hitherto been academic, have now become practical. Kelvin pointed out, 80 years ago, the close relationship between the theories of heat, electrostatics, and current flow. For instance, if R be the electric resistance, R_h the thermal resistance, and C the electrostatic capacity per unit length of the cable, it is easy to show that

$$RC = \rho\lambda/(4\pi)\ ;\quad R_hC = K\lambda/(4\pi),\ \text{and}\ R_hK = R/\rho$$

where ρ is the volume resistivity, K the thermal resistivity, and λ the inductivity (specific inductive capacity). If these physical constants are known, then we can determine the value of any two of the three quantities R, R_h and C, when we know the value of the third. I had a difficulty in understanding how the authors computed the currents required to produce a given temperature-rise in cables suspended in air when the thermal resistivities of their dielectrics are known. I presume that the formulæ used were

$$RI_1^2 = \frac{A}{K_1}(\theta_1 - \theta_2) = B(\theta_2 - \theta_0)$$

and

$$RI_2^2 = \frac{A}{K_2}(\theta_1 - \theta_2') = B(\theta_2' - \theta_0)$$

where I_1 and I_2 are the currents in the cores, A and B are constants, θ_1 is the temperature of the cores, θ_2 the temperature of the sheath, and θ_0 the temperature of the air. It is easy to deduce that

$$\frac{I_1}{I_2} = \left\{\frac{K_2 + A/B}{K_1 + A/B}\right\}^{\frac{1}{2}}$$

where $$\frac{A}{B} = K_1 \frac{\theta_2 - \theta_0}{\theta_1 - \theta_2} = \text{a known quantity.}$$

The limiting value of I_1/I_2 is therefore $(K_2/K_1)^{\frac{1}{2}}$ and it has this value when the cooling of the sheath is very rapid. Mr. Fawssett's results show that, in the case he considered, the deeper the cable is buried the higher is the temperature of the core. This shows that, even when the cable is 2 ft. deep, the convection currents of air at the surface of the heated ground must have a very appreciable effect in keeping the cable cool. The small effect produced by the colour of the sheath on the rate of cooling in air indicates that the loss of heat is due mainly to convection currents, and that the loss of heat by direct radiation is small. It is, however, difficult to see from first principles whether the roughness or smoothness of the surface will increase the heat carried away by the convection currents. Roughening increases the surface exposed to the cooling draught, but it impedes its velocity, and it is difficult to see which cause will have the greater effect.

Mr. C. Turnbull (*communicated*): Cables with sector-shaped conductors are still commonly made by cable manufacturers, but it does not appear to be sufficiently recognized that, owing to their construction, such cables do not get rid of the heat as efficiently as cables with round conductors. It has even been stated that the sector-shaped conductors will carry only 90 per cent of the current taken by round-cored conductors. If this fact be confirmed by experiments, then it would appear that it is time that we gave up the use of sector-shaped conductors entirely, at any rate where cables are to be run at a high current density.

[The authors' replies to this discussion will be found on page 232.]

North-Western Centre, at Manchester, 7 December, 1920.

Mr. H. A. Ratcliff: So far as can be judged from the numerous curves and figures given in the Report, all the cables with which I have any acquaintance at the moment are quite safe. In connection with Clause (8) on page 183 I should like to suggest a rather different method of procedure, i.e. to obtain samples of the various types of cables and test them to find out what load they will stand, and then it may be possible within the limits of a single sheet to indicate the conditions under which the danger limits are likely to be approached in practice. In some cases it may be found that there are other limitations, and that in practice the danger limits will never be approached. For example, in the case of low-tension cables economic considerations may, in many cases, have to be considered long before the cables attain a dangerous temperature; the copper losses and pressure drop may become excessive. I expect that in the case of medium-pressure cables mechanical considerations will arise before the danger point is reached; that is to say, joints will be pulled apart, and other expansion and contraction troubles will occur. If that is so, I do not think one need worry much about the ultimate safe temperature of the cables. The most important work at the moment is on E.H.T. cables, for two reasons. One is that the greater includes the lesser; the other is that they are the cables of which we know least and on which information is most urgently required. Any information we can obtain relating to E.H.T. cables will be of very great value, as they are more or less an unknown quantity, whereas there are thousands of miles of low- and medium-pressure cables in successful operation. The information given in Mr. Fawssett's Report is very valuable, for, in order to obtain runs for cables at the present time in busy cities like Manchester, we frequently have to drive headings at depths of 10, 12, and even 15 feet. The streets are so congested that there is practically no room, and I do not know where we shall lay trunk cables in the future. It may even be necessary to go down nearly to the level of the sewers. The question of the temperature at various depths therefore assumes a very important aspect. I should like to refer to the tests which we have specified in order to ensure that our 33 000-volt cables shall be capable of withstanding the extreme working conditions likely to obtain in practice. Up to five years ago there was very little information available relating to E.H.T. cables, but it was evident that the factors determining the permissible loading of the cables were the extent of the dielectric losses and the rate at which they increased with increase of temperature. The first thing necessary, therefore, was to arrive at some idea as to the maximum safe working temperature of the cables, and, after consultation with the principal cable-makers in the country, and reference to all available sources of information, we arrived at the figure of 60° C., which I consider to be a very safe figure and one with a reasonable margin for contingencies. The next consideration was, how to apportion the total permissible range of temperature between the inside and the outside of the cable. Prior to this Report there was not very much definite information available relating to temperature gradients in the ground, and even if there had been I do not know that it would have been of much assistance, because it is very difficult to say exactly what the ground conditions will be throughout a 10 miles' run. We therefore took the maximum temperature of 60° C. and roughly divided it between the inside and the outside of the cable, giving the cable-makers a permissible internal temperature gradient of 25 degrees C. Experience has shown that they can keep well within that figure. We considered that the cable-maker was only concerned with the interior of the cable and that we as users were responsible for the external conditions. From actual tests we judged that we had safely distributed the

temperature gradient, and it only remained to be seen whether the maximum temperature decided upon would affect the dielectric losses; that is to say, whether they would rise above the critical limit and eventually burn out the cable. We had no means of measuring the dielectric losses, and there were only one or two cable-makers in a position to make the necessary measurements. The difficulty was overcome, however, by specifying that the dielectric losses should not exceed a certain percentage of the I^2R losses in the conductors under rated full-load conditions. We could not measure the dielectric loss, but we could measure the copper loss and the increased temperature-rise in the cable when the dielectric loss was superimposed on the copper loss. The cable was coiled on a skeleton iron drum in not more than two layers, and preferably in one layer. That gave the equivalent of very severe conditions indeed, because there were several coils of cable in contact with each other and surrounded by air, which is about the worst cooling medium. In order to have a factor of safety on the dielectric losses, and to make them more accurately measurable, the losses were doubled by increasing the superimposed pressure in the ratio of $\sqrt{2}$ to 1. The test condition was therefore full-load current on the conductors, plus a superimposed pressure of 47 000 volts. Under these conditions the cable was heated for 10 hours, as it was found that, as a rule, after about 9 hours the temperature conditions became fairly steady, and, moreover, 10 hours was about the longest test period that could be conveniently fitted into an ordinary working day. Under these conditions the temperature-rise of the conductors above the ambient air was carefully ascertained. We also corrected for seasonal variations to some extent by adopting a base temperature of 20° C., and correcting everything to that value by increasing or reducing the load, according as to whether the temperature of the ambient air was greater or less than 20° C. After this heat run with the combined dielectric and copper losses the cable was allowed to cool down for 12 hours, and was then heated up again with the copper loss only; the reduced value of the resulting temperature-rise provided a measure of the dielectric losses. My only authority for supposing that the method is correct is that, in one of the works where these tests were made, it was possible to make actual wattmeter measurements of the dielectric losses, and the results so obtained were found to be in very close agreement with the results arrived at on a heat basis. It should perhaps be emphasized that this is not claimed to be an accurate quantitative measurement, but is merely a means of making sure that the dielectric losses do not exceed a certain specified value. I am interested in the suggestion that the section of cable should be increased in situations where the temperature conditions are expected to be unfavourable, as that is what is being done in the case of the 33 000-volt cables in Manchester. The permissible current loadings are based on the formula $I = 300\sqrt{(aD)}$, where I = current, a = area of one conductor, and D = area over the lead. This formula takes into consideration the area of the cable, and allows for the fact that we anticipate a temperature gradient outside the cable comparable with that inside the cable. I do not know whether it is scientifically correct, but the cable-makers do not object to the current loadings derived from it.

Mr. C. J. Beaver: The ultimate object of this research is presumably the rating of all types of cables under the various conditions of laying. In my opinion, if the work is carried out on a thoroughly scientific basis, and sufficient experimental data is obtained, it ought to go further still and provide means of ascertaining maximum permissible current-carrying capacity for any condition of installation or use. The importance of acquiring very ample experimental support for any conclusions arrived at or deductions drawn, cannot be overestimated. When we study the work of various investigators who have been engaged on work of this kind we find that the tendency has been for them to do a certain amount of experimental work and then endeavour to convert their results into physical constants, which they proceed to apply to all sorts of cables and conditions of laying, and the result has not been at all successful. In fact it has rather discredited the mathematical interpretation of work of this kind. When the work of this Committee is completed it should afford so strong a basis of experimental evidence that it should be possible to construct formulæ which, although they may be of an empirical character, are on such a sound basis that it is possible to insert factors and values relating to the dimensions and physical constants involved in a given case, which will enable one to arrive at the temperature-rise in any kind of cable in any surroundings. I think the tendency will become more and more pronounced for tests bearing upon thermal properties to be embodied in the routine testing of a cable factory, and rightly so. There are one or two points which occur to me in connection with the work outstanding. As Mr. Ratcliff has said, this work is of much greater magnitude and importance than the work which has already been accomplished. Paragraph (1) of this Section states: "It is essential that the Association should settle upon the permissible temperature-rise." I think that should read "total temperature." The maximum temperature attained, and not the temperature-rise, is of importance. Under a given set of conditions of cable construction and surroundings, the final temperature will be, for a given current density, approximately the same, no matter what the initial temperature may be. That is to say, the temperature-rise may differ according to the initial temperature, but the final temperature will be, within limits, constant for a given set of conditions. Clearly, therefore, it is total temperature and not temperature-rise which matters. The thermal values in the Report are given in terms of resistivity. Personally I much prefer the reciprocal term "conductivity" as this enables one to talk of emissivity, which is in some cases an important component of the total conductivity, and, as a large proportion of the cables which have been referred to are cables tested in air, I should have thought it would have been preferred by the authors. An important point regarding emissivity is that it has a temperature coefficient. Fig. 8, which bears on emissivity values, gives results

which are apparently somewhat at variance with physical laws, in that a white-painted surface is shown to have a greater emissivity than a black-painted surface. The explanation is probably in difference of character rather than colour of the surface in question. I notice there is no reference anywhere to rubber-insulated cables. I do not know whether it is appreciated that from the heat-dissipation point of view vulcanized rubber is a very good material. Its resistivity varies from about 515 or thereabouts for a compound containing 70 per cent of rubber, down to something in the neighbourhood of 325 for a 30 per cent rubber compound. These figures compare very favourably with the resistivity of the insulating materials mentioned in the Report. As regards variations in thermal resistivity between the different lengths of cable operated upon, there is a fair amount of difference between the materials used, and the manufacture will vary to some extent. The manufacture and assembly of the component parts have a very powerful effect, especially with material of a laminated character. In the Section entitled "Work Still Outstanding" the authors say, "The question of the relative rating of high- and low-tension cables requires to be investigated further." The American practice referred to in this Report is quite different from anything we have in this country. The American basis for current density is a maximum temperature of 85° C., less 1 degree C. for every thousand volts in working pressure. This means, in effect, that on a 30-kV cable the maximum temperature would be reduced to 55° C. in order, presumably, to allow a margin for dielectric heating. In English practice no such margin is found to be necessary, the normal tendency of increasing current density with increasing diameter of cable holding good at least up to this pressure. Further on the authors say: "The amount of moisture in the soil is to be determined in three towns, over long periods." I think that is not quite the best way to get the information desired. The thermal conductivity of the soil varies tremendously, not only as between different types of soil but also according to the moisture content. For example, I have found that in the case of ordinary river sand the addition of 10 per cent of moisture decreases the resistivity by about a fourth, as compared with the dry state. The only practicable way to obtain information on this subject is to test a wide variety of properly classified soils and obtain a large mass of data so that the results may be of maximum usefulness to the man who is going to lay cables and who, after all, has to remember that he is going to lay them through probably a dozen types of soil. In regard to paragraph (8), Mr. Ratcliff very rightly laid stress upon the determination of dielectric losses, or at any rate their bearing on the total heating. The eminently practical test to which he referred, which is specified by Mr. Pearce for all 30-kV cables, in which the heating with current only is compared with that due to the same current plus $1\frac{1}{2}$ times the working voltage superposed on the dielectric, is not only cleverly designed but conclusive as a criterion of the behaviour of the cable under working conditions. I notice that the cables Nos. 17 to 22 referred to in Table 1 have not yet been tested in soil, although they are armoured cables. They are constructed for a particular method of use, i.e. laid direct in the ground, and the results under those conditions will therefore be much more to the point than tests in air. I notice that in the tests where the cables were laid solid in bitumen the only type of troughing used was stoneware. Obviously quite different results would be obtained in troughing of other materials, iron of course being most favourable from the heat-dissipation point of view, while Howard asphalt troughing also gives surprisingly good results. I am sorry to have to throw very considerable doubt on the results shown in Table 6. For example, the apparent demonstration that the current-carrying capacity is closely similar for a given temperature-rise on a given type and size of cable, whether laid solid or direct in the ground, clearly cannot be true, even if one only looks at it superficially from the aspect of thermal constants of the immediate surroundings of the cable. For rough comparisons regarding the relative heat dissipated due to different methods of laying, etc., I would refer those interested to some curves based on my own experimental results.* Among a number of deductions which may be drawn therefrom, the current density for a certain cable laid solid can be calculated as of the order of 71 per cent of that in the case of the same cable laid direct. I am glad to see the figures on page 196 for thermal resistivities of paper dielectrics, because the figure I have obtained experimentally is about 560, which agrees fairly closely. In the same Table the value for bitumen is given as 511, which I think needs further description. I think it cannot be Trinidad bitumen because the value for this, according to my determinations, is about 200 lower. A recent paper † gives a thermal resistivity for paper of something like 900. With regard to Table 9, I should like to point out that those tests would depend very largely on the quality of paper taken. With some papers my own experiments show that there is a critical point at which the tensile strength falls off rapidly after about 600 hours at the temperature, and under the conditions of test, adopted by the authors. This is a very important matter, and one on which absolutely reliable information is required. With regard to average ground temperatures, I am interested to find that my own records, taken at 3 ft. depth in Trafford Park, coincide exactly with Professor Marchant's observations as recorded in Fig. 18.

Mr. H. Allcock: Were the authors precluded from taking into consideration the various methods of dissipating the heat generated by buried cables? I ask this because I have seen references to proposals for the circulation of air and even water through the cable ducts for the purpose of dissipating this heat. Perhaps the Committee may be able to give some attention to the problem of heat dissipation when they have carried out the important work indicated in the Report which lies immediately ahead of them.

Mr. J. Frith: Instead of measuring the different

* *Journal I.E.E.*, 1911, vol. 47, p. 746.

† R. W. Atkinson: "The Current Carrying Capacity of Lead Covered Cables," *Journal of the American Institute of Electrical Engineers* 1920, vol. 39, p. 831.

temperature-rises with the same currents, alternating and direct, to find the dielectric losses, I should have obtained the same temperature-rise by increasing the direct current; the dielectric loss would then have been equal to the difference between the two copper losses. With reference to the addition of direct current to alternating current, for the purpose of measurement, I have used the converse method, i.e. the addition of alternating to direct current, to measure the resistance of an electric arc.

Mr. B. Welbourn also took part in the discussion. The substance of his remarks will be found on page 214 in connection with the discussion before the Institution.

[The authors' replies to this discussion will be found on page 232.]

North-Eastern Centre, at Newcastle, 13 December, 1920.

Professor W. M. Thornton: The resistivity measurements show clearly a total variation of 4·7 per cent (101·2 to 96·5). Is that considered to be satisfactory in the light of the British Standards requirements? The value of the temperature coefficient given by the N.P.L. is 0·00390 at 20° C., but Professor Marchant's value is 0·00393 at the same temperature, the difference being 7·7 in 1 000. These measurements were made with double bridges at Teddington, Liverpool, and Newcastle, and in view of the fact that an accuracy of 1 in 1 000 is readily obtainable by double-bridge working, I should like to know whether the bridges were compared at the N.P.L., as all instruments in a joint research of this kind should be. The work on the influence of the state of the surface of cables in air gives interesting results. Cables with aluminium-painted surfaces run 22 per cent hotter than when the surface is dull. As might be expected, at low temperatures the colour makes no difference so long as there is no bright metallic surface exposed. But if the heat is lost by low-temperature radiation alone, I should expect a much greater difference between bright and dull surfaces. My own observations on the heat lost by roofing, confirming other N.P.L. results, would lead one to think that the amount would be five or six times more when dull than when bright. That this is not so points to the fact that the greater part of the heat is lost by conduction and convection when the cables are in air, and a comparatively small amount by radiation. The relation between temperature and current, as given in the Report (Figs. 6, 7, 8 and 11) is approximately a square law, but the index rises. In Fig. 8, writing $\theta = ai^n$, $n = 2$ at 110 amperes, 2·16 at 140, and 2·25 at 165, and shows signs of still higher figures. Now, if the temperature rises faster than the heat generated, it seems that the rate at which heat is got rid of falls, i.e. the cooling is proportional to some law with an index less than unity. The temperature gradient in Fig. 9 is uniform from the inner conductor to the outside of the sheath, and the triangular curve fits even better if, instead of being drawn to a point, it is set at the diameter of the inner conductor apart. The conclusion to be drawn from the curve is that the copper or lead in the cable has no perceptible influence on the gradient, or that the drop of temperature in the metals is negligible. The curves of rates of rise of temperature (Figs. 1 to 5) are approximately logarithmic, but appear to be complicated by variations of the loss coefficient. They should be logarithmic, and any departure from that form can be attributed only to the variation of the coefficient of dissipation to earth. The equation of heating and absorption is, in heat units:

$$ri^2/J = W/J = m\sigma(d\theta/dt) + ks(\theta - \theta_0)$$

where m is the mass of unit length, s its outer surface, σ the specific heat, and k the coefficient of cooling to the ground. Where there is cooling to air there is radiation to be considered, perhaps to be included in the linear cooling law, and convection which follows the same law at low temperatures. The rate of transfer of heat is relatively rapid through the insulation, the thickness of which is small, and there is no need for the introduction of a term covering the thermal conductivity of the dielectric when the temperature θ is taken at the surface. The solution of the above equation is:

$$\theta = \theta_0 + \theta_1[1 - e^{-(ks/m\sigma)t}]$$

the final running temperature being $\theta_0 + \theta_1$. If the curve of rise is not simply logarithmic it can then only be by variation of the ratio $m\sigma/ks$, that is, of σ or k. It is unlikely that the specific heat will change, and the curves, therefore, provide a means of observing the changes in the value of k from time to time, that involving the conductivity of the surrounding earth as a factor in the rate of cooling, but all included in the numerical value of k. The thermal time-constant of such a rise is $m\sigma/ks$, which can be obtained in the usual way by drawing a tangent to the curve at zero and dropping a perpendicular to the time line from the point where it cuts the steady value. Fig. 5 contains final steady values drawn to a base of copper area × outer diameter of sheath, but it might be simpler to plot θ_1 against diameter alone. It is always difficult to estimate with any degree of certainty when the steady state is reached, but by taking $\theta = 0{\cdot}95\theta_1$, or any other suitable ratio approaching unity, one can find the corresponding time t_1 from the curves, or by calculation to a much closer degree of approximation. Having chosen some such ratio to be used in all cases, the log term is constant, that is, $kst_1/m\sigma$ is constant; t_1 therefore varies as $m\sigma/ks$. For any given type of cable, however, m varies as the total sectional area, and s as the diameter, so that, as a first approximation, t_1 varies as the diameter. The ordinates of Fig. 5 might then come in a straight line if the abscissæ were taken as overall diameters. If the time of reaching a steady state were approximately proportional to the diameter of the cable it would simplify matters, but there would always be the difference of soil or other conditions to be considered as affecting the value of k. The observed time-constants of the given cables suggest that the cables might fit into such a

scheme of time-constants in a fairly regular manner. Although this Report is very interesting, I think all work on buried cables will lead to a knowledge of deterioration as the most important factor in the problem. Is there enough evidence of the effect of continued temperature and electric stress, direct or alternating, on the life of insulating materials? One can make a treated paper cable last as long as necessary by enclosing it in lead. Vulcanized indiarubber cables, with a vegetable gum as a basis, must deteriorate in time. Paper is more durable, and I see no reason why paper cables should not last 5 000 years, if lightly loaded, in the same manner as the papyrus of ancient Egypt. The supply authorities' problem is to find the most economical current at which to run a cable, having regard to its earning power, life and cost of renewal.

Mr. C. Bertram : One of the most interesting points raised, from a practical point of view, is that which concerns the end effect of temperature-rise on buried cables. The rise of temperature for the last 10 feet or so is somewhat remarkable, amounting, as it does, to something like $33\frac{1}{3}$ per cent. The Report does not give the reason, but states that further results are being obtained, and suggests using cables of larger section at the ends. While this is practicable in many cases, what I have in mind is a tramway feeder which passes through manholes spaced from 100 to 150 yards apart. As it would be difficult to joint larger sections at every manhole, it would appear from the results obtained in the Report that in the case of a feeder laid under these conditions it would be necessary to use a larger section of conductor throughout, which would greatly add to its cost.

Mr. P. F. Allan : The problem seems to revolve round the cooling medium and the final safe temperature at which one can run various dielectrics. It now appears probable that the cooling effects of different natures of soils will vary more widely than has been appreciated by engineers in the past. The greater number of the cables to be laid in the future will be laid direct in the ground, and therefore the problem obviously hinges on the thermal resistivity of the soil. As the Institution is endeavouring to deal with this subject in all its bearings, I should like to suggest that the results obtained at home should be sent to our members in, say, Australia, South Africa and India, with a view to ascertaining the soil conditions for different parts of these countries. In extreme cases the evaporative effect of the sun extends to a considerable distance below the surface of the ground. This indicates the importance of the proposed investigation. The different figures obtained for the thermal resistivity of paper and bitumen are very interesting and suggestive, but it would perhaps be unwise to draw too definite conclusions from them at this stage of the investigations. I would suggest that with 3-core cables it is highly important to determine the temperature of the insulation lying between the cores, i.e. at the centre of the cable, as it is obvious that this temperature may be considerably higher than that at the surface of the cable, and it is, after all, the effect of heat upon the dielectric, no matter where placed, which has to be considered. The summary of the Report draws attention to the fact that the question of dielectric loss is being considered; there seems to be every reason to believe that this will be an important consideration in the design of E.H.T. cables, or super-E.H.T. cables, which we hope will be developed and required in the future. An interesting contribution to the consideration of this problem was made by Professor W. M. Thornton in his paper on "Distribution of the Electric Stress in Three-phase Cables," read before the British Association at Winnipeg in 1909. Some important figures showing how much the power factor of the cable, and therefore the dielectric loss, increases with increase of temperature in paper-insulated cables are given by Professor A. Still, in his book on "Electric Power Transmission."

Mr. N. W. Prangnell : We must all recognize the extreme value of the results obtained in these very thorough investigations, but I should have liked to see some summarized results which would be of immediate help to those who are responsible for the lay-out of distributing networks. It is understood that an investigation of such a subject as this must necessarily take a long time, but with the data now issued it should have been possible to say, for instance, that, assuming cables are to be run in such a way that precautions have to be taken to dissipate heat, they may, if of the armoured type, with advantage be surrounded when laid in the ground by a special type of material such as Thames ballast or fine sand. Also we might have been told whether it is detrimental or advantageous to have the armouring of the cable covered with a layer of impregnated jute yarn. It should also be possible to indicate whether special precautions should be taken, when pulling plain lead-covered cables through ducts, to ensure that the cable-drum sets in the cable should be as far as possible eliminated, so that the cable, when lying in its duct, shall have its lead directly in contact with the duct and thus obtain as much advantage as possible by conducting away any heat that has to be dissipated. It would be interesting to know whether this particular point has prominently shown itself in the investigations so far carried out. It may be that sets in the cable, which would result in only point contact between the lead and the duct, are an advantage, and that it is better to encourage heat dissipation by radiation rather than conductivity. It may be of interest to illustrate an occurrence that happened 15 years ago and tended to show that even this question is important. A length of E.H.T. cable failed, and when the faulty length was examined it was found that it contained a number of dry patches in the insulation, i.e. that the impregnating oil or compound had disappeared from these dry patches. Further investigation indicated that the parts where the compound had accumulated coincided with the positions at which the lead of the cable had been in contact with the stoneware duct in which it had been lying. This was thought to indicate clearly that the cable, which had been run at a high density, had dissipated its heat by conductivity to the duct, and that this local heat dissipation had accumulated the oil from other parts of the cable. If the Association can issue one or two rulings, on the strength of their investigations, I submit that the earlier

this is done the better. I would also ask that detailed specifications of the various cables used in the investigations should be issued. The thickness of insulation and lead is given, but it is also important to know, with regard to paper-insulated cables, what percentage of impregnating compound is present in the insulation. It is also essential to know, with regard to the armoured cables: (1) Whether there is a layer of jute between the lead and the armouring, (2) if the armouring is of the wire or steel type, and (3) whether a final layer of jute yarn is served over the whole cable.

[The authors' replies to this discussion will be found on page 232.]

North Midland Centre, at Leeds, 14 December, 1920.

Mr. C. A. Gillin: The Report is extremely interesting as far as it goes, but this is not very far, for practical results at any rate, in spite of the very large amount of laborious work it represents. It seems to me that the investigations most desirable are those referred to on page 183 under the heading "Work still outstanding," and in particular paragraph (8). What we desire to know is to what extent and for how long any cable may be loaded without risk of future injury. To my mind, before any practical use can be made of the results shown in the Report, the physical and chemical characteristics of cables in use for long periods of time and under working conditions must be known, and should therefore be the first problem to tackle. This appears to have been left to the last. Table 9 and Fig. 19 indicate the importance of this, but, unfortunately, they also seem to illustrate the comparatively small amount of research performed on what should be really the first problem to elucidate. The effect of dielectric losses, to which reference has already been made, on the temperature of cables at very high pressures, is another important matter on which there is no information. Dielectric losses vary according to the composition of the compound used in impregnating paper-insulated cables, and any future results of research in this direction should specify very clearly the insulating material, its texture, and grading. Another question requiring very careful investigation is the increase in temperature due to various numbers of cables laid adjacent to each other, and the effect of their various spacings. I think the spacing of cables at voltages over 20 000 will be a very difficult problem, that is, unless cable manufacturers fulfil their promises and produce cables with a negligible dielectric loss. Some good has already resulted from the research, in showing the end effect in cables, and the suggestion (on page 192) that tails of larger section than that of the cables in the ground should be used where cables leave the ground, is valuable, as it is now evident that in heavily loaded cables the weakest point is what one has been accustomed to regard as probably the strongest. The difference in carrying capacity of cables laid in duct and underground is also definitely shown. Another item of great interest is the fact that a cable laid 4 ft. deep must be rated 15·7 per cent less than if laid 1 ft. deep. If there are any people nowadays who can afford to lay cables 4 ft. deep with an eye to "safety first," this fact should act as a decided deterrent.

Mr. B. Martin-Cooper: There are one or two points not covered by the present Report which, I think, might be investigated. Engineers responsible for the laying of cables are chiefly concerned with long cable runs, and I think the tests on short lengths laid at uniform depths will hardly give them sufficient information. For instance, the question of joints will arise, and, although it is agreed that a joint is electrically strong and that there is, therefore, less heat generated in it than in the cable itself, one would like to know what effect the dielectric used in the joint has on the heat dissipation. On E.H.P. mains the joints are not very numerous, but, in the case of a low-pressure distributor with service tee-joints at frequent intervals, these joints might considerably affect the loading possible. In Table 2 the currents for a 50 degrees F. rise are given for E.H.P. cables as frequently used for 20 000-volt work, but, as all the tests were taken with alternating current at 250 volts, I should like to know what difference there would be in these currents when the voltage applied is exactly that for which the cables are designed. On long runs it is practically impossible to lay the cables at a uniform depth. Normally the cable is laid at a depth of about 2 ft., but in places a depth of 4 ft., and even more, is necessary for comparatively short distances. To what extent will these short lengths limit the current for the whole run? In the tests taken by Mr. Fawssett it would be interesting to have two more pilot points to cover the centre portion of the test length at a depth of 4 ft., in order to see how the temperature-rise over this portion compares with that over the portion where one end gradually rises to a 2 ft. depth. In this way it might be possible to determine how much of the heat generated in the cable laid 4 ft. deep is dissipated through the cable laid 2 ft. deep. With regard to the depths of cables laid direct, all the tests taken at the National Physical Laboratory were for a depth of 2 ft. to the top of the cables, whereas at Newcastle one of the depths was 2 ft. to the top of the concrete slab, which is equivalent to about 2 ft. 5 in. to the top of the cable. The comparative figures might be more useful if these depths were identical. Tests have been taken on cables drawn into stoneware ducts and laid solid in bitumen, but it would be interesting to have some tests on cables drawn into cast-iron or steel pipes, as these are often used to protect the cables where very little cover is available over railway bridges, etc., or at greater depths under awkward obstructions.

Mr. W. M. Selvey: Some 17 years ago I spent much time on similar experiments under the direction of Professors Ayrton and Mather. In that work, many of the difficulties experienced by Mr. Melsom

were met with, but much more simple apparatus was used because of the difficulty of getting a high accuracy in the measurement of the integrated I^2R losses. I am quite in agreement with the conclusion that most of the work on heating can be done with direct current, except in the case of E.H.P. cables. As in the work with which I was connected, Mr. Melsom has adopted a "control" experiment as a base. His particular form of control experiment is to lay a cable on the floor, and he states how he controls some of his variants; others, such as colour and condition of surface, the nature of the contact of the cable with the floor, the surface of the floor, the temperature of the envelope, and the exact relative position of the two limbs of the cables are omitted. It may be gathered from one of his experiments that these two limbs were in contact, because he states that he could not use the lead sheath for measuring temperature-rises. I suggest, therefore, that his control experiment is not really in an efficient form, and that it should have been under the only satisfactory conditions for a control experiment, namely, that it could be repeated with reasonable certainty by any investigator in any part of the world. In the experiments I have referred to, Mr. Alexander Siemens, who was interested in them, suggested laying the cables on iron plates as well as on a wooden floor. The results were undoubtedly different, and ultimately a more satisfactory control experiment was obtained by suspending the cable in the air. The object of the control experiment is very clear. It is to define the physical constants of the cable itself without reference to its surroundings. As expressed in the Report, the object is to find the K value of the cable without reference to the K value of its environment, which varies fairly considerably according to the surroundings, such as air, air plus contact with a surface, bitumen and iron envelope, earth, etc. I think, therefore, that Mr. Melsom's values for K for the cable itself are not really true values compared with the actual K value of a cable when buried, as in Mr. Fawssett's and Professor Marchant's experiments. What is brought out by Mr. Fawssett's experiment is, that the value of K according to the environment depends on the depth of the cable below the boundary line between the solid and the atmosphere. The experiments show that this solid changes its physical condition under the influence of the experiment. It seems a pity that Mr. Fawssett has taken 1 ft. as his standard for comparison, whereas Mr. Melsom's standard is a 2 ft. depth, which Mr. Fawssett also has experimented upon. This points to the advantage, in the final Report, of having the whole matter collated by a single editor. I should think that some advantage could be got by studying the data from the properties of soil which is in the possession of the workers in agricultural science. There is a very considerable difference between soils, with regard to their capacity for absorbing and retaining moisture. It is quite possible that these properties may vary with the depth below the surface. If so, some kind of selection may be made in filling-in the excavation. I suggest that the top soil, with its higher proportion of organic matter, may be the best material immediately to surround a cable. Apparently it is quite certain that a buried cable can dissipate more heat than the same cable suspended in air, as in the suggested control experiment. If this is established, it is a comparatively simple matter to suspend part of a length of cable in the air and to bury a part, and then to compare the temperature-rise between the two parts. In this case, the determination of the I^2R losses is less important in accuracy, and the difference between the temperatures of the two parts can receive more particular attention. In this way an average K value for the environment can be definitely compared with that of air, and any individual line of cables can actually be tested out on the lines of the control experiment. While it is true that the authors are dealing with buried cables, they do make suggestions as to future work. Might I point out that what causes trouble is generally what is known as the "worst case." The authors' worst case is a cable in air, but there are many cases in industrial work where quite a number of cables are run parallel with, and vertically over each other, on racks on the side of a wall. This is probably a much worse case, but we have no definite information with regard to it. There is another case, probably not so important, but still fairly frequent, where a number of cables are run horizontally under a floor or roof. There are a number of small points, more or less literary, which the authors could conveniently deal with when the final Report is issued. For instance, they have occasionally used figures and constants which are not defined until considerably later on in the Report. I suggest also that their statement of Russell's formula does not prove that the value of K there given is the value as defined by the thermo-resistivity of multi-core cables. While the use of such a formula is excellent as a basis, surely these very experiments give the opportunity of establishing the formula on an experimental basis. There are other small matters which can readily be dealt with on reading over the Report from an editorial point of view. I would suggest a further explanation as regards the figures given for the concentric cables as to what the conditions are under which these experiments were made. There is no doubt that this is a very valuable contribution to our knowledge, and that it ought to be continued, especially along the lines of greater collaboration between the three different sources of research, presenting the results in an agreed and standard form of terms and units.

Mr. W. E. French: The reading as well as the discussion of this important Report is made rather difficult, owing to the frequent changes from one system of measurement to the other, which recur throughout. I would, therefore, strongly support Mr. Selvey's suggestion of one editor for the future issue of reports. The practical significance of K, the thermal resistivity, quoted in Tables 7 and 8 of the Report is not clear to me. In other words, do these constants furnish a definite criterion as to what extent a cable may be loaded, and as to what higher current loading a cable, having a low thermal constant K, may be subjected to, than one having a high thermal resistivity? Judging from Table 3, this would seem to be approximately

the case when comparing cables for practically the same specification. This criterion would, of course, alter with the different methods of laying, owing to the relative values of the thermal resistivities. I should be glad to have further information on this point, and the authors' views on the practical meaning of the thermal resistivities. The discovery of the end effect is an important result. Accepting for the moment the N.P.L. figures, the end effect appears to be the limiting condition for which a cable may be put into service. However, comparing the results obtained by Professor Marchant with those of the N.P.L., one would have suspected the higher thermal resistivity of air to be the cause of the end effect, according to the N.P.L. figures (where a temperature-rise at both ends is recorded). This surmise is rather contradicted by Professor Marchant's tests, which record, with two exceptions, a lower temperature-rise at one end, a fairly uniform temperature-rise in the middle, and the highest temperature-rise at the other cable extremity; in no case did he find such a severe temperature-rise as that recorded by Mr. Melsom. I presume the cables were laid solid in bitumen in both series of tests, and that the test conditions were practically identical. In view of the important practical bearing of this effect, I should be glad to have further information on this point. There is no reference in the Report to the heating curves of the cables, whether in air or buried, with regard to their thermal time-constants. The knowledge of this time-constant has proved so useful in the judgment and estimation of the thermal performance of electrical machinery and apparatus for any required duty, that it is rather surprising that the authors have either missed this point or have made no mention of their efforts in this direction. The thermal time-constant can be defined as the ratio of the heat capacity to heat-dissipating capacity of an electric machine or apparatus. Expressed in its simplest form for stationary apparatus, the thermal time-constant is:

$$t_0 = \frac{WC}{SK_r}$$

where t_0 = thermal time-constant,
W = total weight of material heated,
C = constant for the specific heat of the material, and constants for conversion to electrical measure, etc.,
S = area of surface of apparatus,
K_r = watts dissipated per unit surface.

The heating curve being logarithmic, the thermal time-constant will appear on the abscissæ at the temperature corresponding to 63 per cent of the final theoretical temperature-rise. The time-constant may also be regarded as that time interval during which final temperature is reached, if no heat were dissipated, and the whole of the energy were employed in raising the heated body to its final temperature. The value of K_r is fixed by the final permissible temperature-rise. The constant C can, for any known line of designs, be easily determined from the heating curves. Hence, the time-constant can be calculated for any tested line of apparatus, and, knowing their time-constants, the thermal performance for any required duty can be predicted with ease and accuracy. The question then arises, whether this formula or a similar one is applicable to cables. The answer can only be given by a series of tests with this point in view. In order to get some idea of the applicability of the method to cables, I have used some of the heating curves of the Report, and have assumed that they are logarithmic. The time-constant was determined for each case, and from this the time interval to reach 99 per cent of final temperature was calculated. This was compared with actual time-intervals of the curves to reach the same temperatures. The results are quoted in the following table:

(1) *Time to rise to* 99 *per cent of Final Temperature when suspended in Air.*

Calculated	From Curve
3 hrs. 7 min.	3 hrs. 5 min. (Fig. 1)

(2) *Time to rise to* 99 *per cent of Final Temperature when laid Solid in Bitumen in Stoneware Troughs.*

	Calculated	From Curves
(*a*)	2 hrs. 54 min.	3 hrs. 7 min. (Fig. 27, middle curve)
(*b*)	5 hrs. 12 min.	5 hrs. 20 min. (Fig. 28, middle curve)
(*c*)	4 hrs. 18 min.	4 hrs. 20 min. (Fig. 28, lower curve)
(*d*)	4 hrs. 20 min.	4 hrs. 35 min (Fig. 29, middle curve)
(*e*)	5 hrs. 44 min.	5 hrs. 20 min. (Fig. 30, middle curve)

While these figures must be accepted with reserve owing to the errors due to the smallness and uncertainty of the scales of the curves in the Report (no other reliable data for cables being at my disposal) they may serve a useful purpose in a general way. The figures given in my table, for cables in air, show a reasonable agreement between the calculated and tested figures. With reference to the figures for cables laid in troughs, in my table, making allowance for errors due to paucity of scale data, there is a fair general agreement between the two sets of time intervals. In all cases, except case 2(*e*), the time values obtained from the curves are larger than those calculated from the thermal time-constants, suggesting that either the calculated time-constants are too small, or that the heating curves of buried cables are not truly logarithmic. Allowing for the moment that the assumption of the logarithmic law is correct, then the thermal time-constants found by inspection from these curves are much too small, indicating too rapid a temperature-rise in the early parts of the heating curves. It may also point to some subtle and obscure error in the earlier parts of the heating curves, although it is evident that great experimental care has been exercised. The constant C for cables could be found experimentally in the same way as indicated above, and K_r would be

fixed by the permissible heating, with due regard to the mechanical and insulating properties of the dielectric. It seems worth while to investigate this method for cables, as it would provide the engineer with an elegant and accurate method to predetermine and check the thermal performance of the network under his care.

Mr. C. J. Jewell: There are many points in the Report which are both new and enlightening, when one considers that up to now we have been laying cables in a haphazard manner, drawn in, solid and laid direct, mere as the roadway suited us, with no very clear idea of which system was the most efficient from a distribution point of view. Now we shall have a set of tables and rules to which we can refer, to find out the most suitable method of laying our mains, taking into consideration all the local features we can expect to meet.

Mr. R. D. Spurr: This Report needs more than a casual glance to obtain its true perspective; we must take each individual Table, study and apply it, and then we shall get some tangible results. The subject is one of very great importance, especially, as Mr. Selvey says, in regard to cables on racks in power stations, where cables are heavily loaded and working under very different conditions.

[The authors' replies to this discussion will be found on page 232.]

Liverpool Sub-Centre, at Liverpool, 7 February, 1921.

Mr. J. A. Morton: In the "Introduction" on page 182 the research is stated to be an investigation "to provide definite values of current-carrying capacity for cables laid underground in the conditions prevailing in practice in this country." When I first read the Report, I considered it to be too diffusive and an undigested mass of isolated facts, but later I appreciated it more. I recognize that all these preliminary experiments were necessary to clear the ground. For instance, on looking at the values of K for the insulation one wonders at first why so many cables are dealt with, but it would of course be necessary for the authors of the Report first to ascertain the limit of variation of this value of K for cables as at present manufactured. The ordinary engineer wants tables or simple formulæ; for instance, such tables as are given in the I.E.E. Wiring Rules. Now the ground has been cleared in this preliminary Report the question is what next to do to bring about this desirable consummation. It will probably be necessary to obtain lengths of armoured cables (preferably all by one maker)—a small, medium and large size of each type. Such types might be: low-tension single cable, low-tension twin with shaped conductors, low-tension concentric, low-tension 3-core with shaped conductors, 6 600-volt 3-core with shaped and round conductors, and a length of 3-core extra-high-pressure cable with round conductors and with, say, $\frac{1}{2}$ inch of dielectric for working at 30 000 or 40 000 volts. Temperature-rises could be obtained in earth with a certain moisture content and depth. Corrections would be made to certain agreed standard values of K for dielectric and soil, and tables constructed like those prepared by Mr. Melsom in the I.E.E. Wiring Rules giving currents for, say, 60 degrees F. and 90 degrees F. temperature-rises in the different types of armoured cables, laid direct. I should imagine that Mr. Melsom could arrive at multiplying constants for different thermal resistivities of dielectric and soil, by which the standard figures in the table could be modified as required. These experiments would also show how long the different types of cable took to get to their steady temperature, a point of some importance not sufficiently dealt with in the Report. There should also be some cooling curves; for the rate of cooling would probably not be the same as the rate of heating. I think that from three to six 3-core cables should be laid in the same trench with, say, 6 inches between centres; this would enable constants to be obtained for cases where more than one cable is in a trench. The tables in the Wiring Rules provide for multiplying constants based on single cables in air, and I think similar constants could be arived at based on results obtained with buried cables but to give the temperature-rise for armoured cables in air on vertical walls. It has been suggested that the Committee might go into the question of dielectric heating at 20 000 volts or over, but I do not think that is necessary, because different makers use different compounds for impregnating their cables, and these have different electrostatic capacities and dielectric power-factors. In regard to these extra-high-pressure cables, makers could draw up their own tables for safe currents in the copper, based on the Committee's tables, making allowance for dielectric heating in their own cables. I think the part of the Report dealing with bitumen cables is very complete so far as single cables laid solid are concerned, and that nothing more need be done on these lines. Bitumen cables are being superseded for street work, but there is something that might be done by the Committee. These cables are largely used for mining work, for running down pit shafts, and I think experiments might be made on such armoured cables run vertically, say, 6 inches from a wall, as would happen in a pit shaft, so as to find out what current could be carried for a temperature-rise of 40 degrees F., which is about the limit for this class of cable. Referring now to the ground temperature, it has been usual to take this as 60° F., although it will sometimes be rather higher in summer, but as the heaviest loads usually occur in the winter months when the temperature is lower than normal, I think 60° F. would be a good figure to work upon. There is also the question of the permissible maximum temperature-rise to be allowed; and, so far as paper cables are concerned up to 11 000-volt working, 90 degrees F. rise (from 60° F. initial temperature) is quite safe, but one has to take account in this connection of the longitudinal expansion of conductors on the lengths of cables now made. Generally a rise of 60 degrees F. is sufficient, because the expansion in a 220-yard length of copper

with such a rise is 4½ inches, and this is quite enough to deal with if expansion joints are not used. Of course all the expansion does not take place at the end of the cable; if it did, a rise of 60 degrees F. would cause trouble. Large expansions distort the lead, which is not elastic, and will probably cause trouble. For cables built for, say, 20 000 volts and over I think that probably a rise of 60 degrees F. (i.e. a maximum working temperature of 120° F.) is sufficient, because above 120° F. the dielectric losses tend to rise quickly, though possibly this may be remedied by cable-makers in the near future. As Mr. Melsom pointed out, there is a very great difference between the values of K for the low- and high-tension cables given in Table 3, and this brings into prominence for the first time the importance of the thermal resistivity of the dielectric. In the low-tension cables the values of K are something in the nature of 1 000, and for high-tension cables about 500. Probably the reason for this is that the extra-high-tension cables are better dried and compounded, and there is possibly less enclosed air. Cable-makers are aware of the importance of the air question in extra-high-tension cables, but as this makes a difference of 10 per cent in the carrying capacity it is obvious that a lower thermal resistivity means a better cable, not only electrically but from the point of view of heating, and I do not see why the values of K for the low-tension cables should not be reduced to be more in line with those for the high-tension cables. The values of K for the jute servings over and under the armour are not mentioned in the Report. I should have thought that the thermal resistivity of these had something to do with the case. Apparently from Table 4 the effect of armouring is of little importance on the heating, although it relates to cables in air. It shows that when the armour is removed the temperature rises slightly. I should have imagined that if the cable were buried in the earth the armour would act as a lagging and we should get the opposite effect, because the value of K for the jute servings is much higher than that of the soil, which is not the case when the cables are in the air. Apart from this, however, it does not follow that the dielectric round the conductor has a lagging effect. It has been found from experiments with bare cables in air that a braiding of cotton soaked in ozokerite tends to cool a small wire, while with a very large cable having a similar braiding the braiding acts as a lagging, and on some intermediate sizes the braiding has no effect at all upon the carrying capacity of the wire. A little more information would be useful on that question. There is also the question of the value of K for the soil, and I think for general work in Great Britain this figure may be taken as 100. This means that the soil contains about 12½ per cent of moisture. In the summer the value of K is higher, but the load is usually highest in the winter when the soil is wettest. It is obvious that this K value as applied to the soil is of importance, and a very dry soil might reduce the carrying capacity of cables by 25 per cent. There are places in this country where that might be the case. The paving apparently does not have much effect. Mr. Fawssett's experiments are rather striking. One would not think that various depths would make so much difference to the carrying capacity of the cables. It shows that we must put cables as near to the surface as possible. Three-core cables with shaped conductors will carry about 3 per cent more current than the same cable with round conductors, as shown in Table 3, and this has been found to be the case in America. With regard to cables in ducts, these carry less current than armoured cables laid direct, the actual amount depending on the diameter of the cable in relation to the size of the duct. It is obvious from the Report that an armoured cable buried in the earth is better able to deal with peak loads and, in fact, it seems that armoured cables buried in the earth are better from every point of view. The Report states that there is no appreciable difference between the heating effect with alternating (50 period) and direct current, but Major Taylor discussed this question at Birmingham and apparently showed that the sheath loss of alternating-current 3-core cables might be 50 per cent of the I^2R losses in the copper of an unarmoured cable, but his conclusions seem to be at variance with all previous work on the same subject. He passed 230 amperes, three-phase alternating current (25 periods) through each of the cores of a 3-core cable, and noted the temperature-rise. To give an equivalent copper loss he then passed 400 amperes direct current through one core only of the cable and left the other two cold, and again noted the temperature-rise. I do not think that is a true comparison. He should have taken 230 amperes direct current per core, i.e. in all three cores as in the alternating-current test (instead of 400 amperes in one core) and I am sure he would have obtained a different result, a result which would approach more nearly what the Association say in the Report in regard to this matter. Professor J. T. Morris some years ago went into this question and deduced a formula and, if we assume that Major Taylor used an 0·15-sq. in. cable, Professor Morris's formula would give a sheath loss of under ½ per cent (i.e. negligible) compared with the copper loss in an unarmoured cable. Mr. W. Brew made some experiments on an actual cable in Dublin, which are mentioned in his book on "Three-phase Transmission," and he obtained a sheath loss of about 4 per cent compared with the copper loss at 50 periods, which would be equivalent to, say, 1 per cent at 25 periods. In 1904 Mr. M. B. Field discussed the same question and gave figures for a 3-core cable carrying 545 amperes per core at 50 periods, and the sheath loss was about 4 kW per mile, which was practically the same result as that which would be given by Professor Morris's formula.

Mr. G. H. Nisbett: The Report is of great interest to cable-makers, and it will open up new trains of thought which will eventually lead to improvements in manufacture. In a 33 000-volt cable with a load of 167 amperes per phase, the annual cost of the energy lost and the interest and depreciation added together approach a minimum figure for sizes between 0·1 and 0·25 square inch of copper, but between these sizes the total annual charges do not vary very much, even with considerable variations in the price of copper and the cost of energy. This being so, it is evident that a supply engineer would install the larger cable,

and in this way avoid all questions of possible overheating. Perhaps it is fortunate that this is so, because it would be most difficult for the supply engineer to determine the temperature at which his cables are working, as he would need to know every detail about them and the manner of laying, as well as the type of earth in which they are laid. Mr. Melsom draws attention in the Report to the consistency of the lead sheath as regards resistance and temperature coefficient, and it is suggested that the measurement of the resistance of the lead would be of considerable use in practice to ascertain the temperature of cables already laid. I think that Mr. Melsom is too sanguine and does not consider the difficulties to be met. In the first place, there is the problem of confining the measuring current to the lead and dealing with the stray currents nearly always present in the lead. In addition, the relationship of the temperature of the lead to that of the copper in the cable would have to be determined for each size and type of cable and each method of laying, and for the time the current had been passing up to the point of maximum temperature for that current. That this difficulty is a practical one is shown in Fig. 2, which gives the temperature-rise of a 0·1-sq. in. concentric cable tested in air. The inner copper has a temperature-rise of 45 per cent more than that of the lead. Comparing this with Fig. 3, which shows a test on a 6 000-volt 3-core cable, we have the copper 142 per cent higher in temperature than the lead. In Fig. 4 the ratios are 50 and 73 per cent. In Fig. 6 we see that with 200 amperes the temperature of the copper is 84 per cent above that of the lead, whereas with 275 amperes the ratio is 67 per cent. It is evident that the relationship of the temperature-rises of the copper and lead will depend upon the thermal resistivity of the insulation relative to that of the surrounding medium. I should like to draw attention to the relative temperatures of the copper and lead in Fig. 3, that of the lead seeming to be much too low, attaining a temperature-rise of only 16·5 degrees C., whereas that of the copper is 40 degrees C. This appears to be wrong, because in this case the cable is loaded to 1 700 amperes per square inch, whereas in Fig. 4 a cable for a similar working pressure loaded only to 1 333 amperes per square inch has a temperature-rise in the lead of 18 degrees C., i.e. $1\frac{1}{2}$ per cent higher. On page 192 attention is drawn to the relative heating of high- and low-tension cables of the same copper section, and it is pointed out that a high-tension cable in air is the cooler of the two, but the opinion is expressed that probably when the cable is laid the difference will disappear. This is probably true, but attention should be drawn to the fact that the test was not sufficiently long to produce heat saturation in both the cables, as the curves were approaching each other when the test was discontinued. Most of the tests in the Report refer to cables in air, and these, of course, are not of very much practical use as regards supply mains, except for short ends of cable which are behind switchboards or entering substations, and it is evident that for these a considerably larger section of copper must be used than for the portion underground if the latter is to be worked at its maximum temperature. The tests show that if the medium surrounding the cable has a high resistivity relative to the insulation, the addition of more insulation would keep the conductor cooler; conversely, if the surrounding medium has a low heat resistivity compared with the insulation, more insulation would keep the copper hotter; so that there is a point at which, if the heat resistivity of the insulation is the same as that of the soil, the addition or subtraction of insulation will make no difference to the temperature. On page 199 some tests are given of paper used for insulation after heating in air and after soaking in oil, and any user of cables who saw these tests might be very disturbed that the strength of the paper had in nine days gone down from 39 to 7. But it must be remembered that the temperatures used were practically destructive. The object of such a test is not very apparent, the temperature used being far above the limit at which any cable-maker would guarantee his cable.

Mr. F. J. Teago: I think that the results could be co-ordinated by a further series of tests on cables of the types under consideration. This could be effected by carefully investigating the manufacture of each cable tested to find out the intimacy of contact between the various layers composing the insulating material, and between this material and the conductors. Also, the nature of the surfaces between the layers might be profitably considered. In Table 3 there are differences in the value of K which can be accounted for only by differences in manufacture, which probably exist as thin air films between consecutive layers. These air films cause the temperature difference between the core and the outside surface of the insulation to bank up very considerably. It is fairly certain that a series of tests on these lines would co-ordinate results which at present are rather disconnected.

Mr. W. P. Fuller: In Table 3 the authors give the currents corresponding to a temperature-rise of 50 degrees F. for different cables and for different thermal resistivities, but there is no indication how these figures are calculated. The currents given in column 6 of Table 3 do not agree in all cases with those in Table 2. The formula for the resistivity of 3-core cables is the same as that given by Russell for the capacity of three cores to the lead sheath, with the addition of the constant 4π. This formula, however, gives capacity figures which are too low by 20 to 30 per cent, and it seems possible that if the correct relationship between the size of conductor, etc., and the thermal resistivity, were known, the figures for 3-core and for single-conductor cables would more nearly harmonize. A great deal of attention has been paid to thermal resistivity, but specific heat is not mentioned in the Report, although its effect may be very important. Suppose we take an 0·15-sq. in. 3-core 20 000-volt cable working at 250 amperes and raise the current to 300 amperes; I calculate that, even if none of the extra heat generated were conducted away from the cable, it would take $1\frac{1}{4}$ hours to raise the temperature of the cable 10 degrees F., although the final temperature of the cable (from Fig. 6) would be enormous. It seems to me, therefore, that in estimating the overload capabilities of a cable, the thermal capacity must be considered. The thermal

capacity of the ground, in the case of buried cables, is also as important as the resistivity. I do not believe that a cable laid in the ground ever reaches a steady temperature, and, as a matter of fact, all the curves in the Report are still rising. In the case of cables suspended in air, convection enables the heat to be carried off as rapidly as it is generated, and so a steady temperature is reached, but I do not think this is the case with a cable buried in a fixed medium. It might be a good plan to give curves of temperature-rise and time for different diameters of lead piping with various values of watts lost per square centimetre of surface. One could then work back and find the temperature of the copper, by using the steady results obtained with the cables in air. I notice that the authors anticipate making experiments on cables with full pressure applied. In the case of a cable carrying current alone the maximum temperature occurs next to the copper, but when voltage is applied it is probable that the maximum temperature will shift to the centre of the cable, and I should like to know if the authors contemplate checking this by using, say, a special cable with a central wire to measure the temperature.

Mr. W. Holttum (*communicated*): The most interesting question is to what extent this research will facilitate the attainment of the maximum degree of economy of outlay, combined with security of operation. The outstanding consideration is that the temperature of a cable is that of its hottest part, and in this connection the figures given at the bottom of page 192, showing that the temperature of the unburied ends of a cable may be from 12 to 25 degrees C. higher than the buried part, are very striking. It is clear that, if the full benefit of this research is to be obtained, much more detailed consideration will have to be given to the method of laying and the nature of the ground at every part of the length. The research, up to the present time, has been mainly directed to the cables, but it appears that there is considerable room for further investigation, not only as to the nature of the ground in various places, as proposed on page 183, but as to the behaviour of the ground when occupied by warm cables. In many cases cables in continuous use will maintain the ground in average weather in a condition always differing in its heat-dissipating qualities from the normal. It seems likely that a special piece of apparatus for ground tests would be more useful than an ordinary cable; for example, a spherical unit containing a heater and means for taking continuous observations of its surface temperature, also a number of buried thermo-couples or other convenient apparatus, for taking the temperature at various distances from the heating unit, and some form of buried moisture indicator, if such could be devised, would be a very useful adjunct. In this way much needed information would be obtained of the thermal resistivity of different kinds of ground, at varying temperatures and with varying degrees of moisture. The fact that there is a difference in current-carrying capacity of 20 or 25 per cent between cables buried and suspended in air goes to prove that the nature of the contact of the cable with the ground is of considerable importance, but there seems room for trials with identical cables in identical earth, but with the latter in different degrees of contact with the cable; also the effect of slightly wetting the ground while laying and before ramming down might be tried, tests being taken after normal dryness has been reached. It seems possible that the practice of watering the ground with a solution such as calcium chloride during laying might, in certain cases, yield profitable results. It would be interesting to know on what grounds the statement at the bottom of page 197, column 1, that the current-carrying capacity of a cable does not vary greatly with the thermal resistivity of the dielectric, was made, and how the figures given in the last columns of Table 3 were arrived at. It is noticeable that there is not a constant ratio between the figures in columns 7 and 8. With regard to determinations of the thermal resistivity of the dielectric, is it not possible that the wide variation in the results given in Tables 7 and 8 is accounted for, not by actual differences of thermal resistivity, but by differing degrees of intimacy of contact between the paper and the copper and lead? It would appear desirable to eliminate the factor by a number of artificial test-cables in which the copper and lead are replaced by mercury. It would then be ascertained whether the variation in thermal resistivity with temperature mentioned on page 212 was really due to temperature or to varying mechanical pressure between the parts of the cable. Future economies may be looked for in two ways:—(1) By making sure that cables are run at their maximum continuous loads; and (2) by permitting a load above the continuous rating for a limited time. The second question was discussed by Mr. R. O. Kapp in his recent paper,* and further investigations might well be guided to some extent by the suggestions put forward therein. For this purpose information on the heating time-constant of cables is required. Although this figure cannot be determined accurately, since a buried cable, even in homogeneous ground, does not heat up according to an exponential law, yet the curves of heating given in Figs. 1 to 5 are sufficiently near to the standard form for an approximate figure to be derived. The statement at the top of page 194 that the temperature of a cable laid direct does not appreciably increase after 50 hours seems to require qualifying according to the size of the cable. It would be useful to determine the figures for different sizes of cables up to the largest, and derive a formula, probably containing a term for the thermal resistivity of the ground, for calculating the time-constant; from this a determination of the permissible overload could be made. It is not at present obvious, however, how the cable could be adequately safeguarded if worked on these lines. A series of observations on a cable in actual service, with a heavy peak load, could be made, arrangements being provided for taking the temperature from potential points over a given length during working conditions, and for observations of ground temperature at the same time.

* *Journal I.E.E.*, 1921, vol. 59, p. 94.

Mr. E. B. Wedmore (*in reply, communicated*): On behalf of the Association and of the authors, I have to thank members for the full discussion and the large amount of useful criticism and suggestions which have been made. The various points raised are being carefully considered by the Association,

Messrs. S. W. Melsom and **E. Fawssett** (*joint reply, communicated*): In order to avoid unnecessary repetition, it has been found convenient to combine the replies made at the various Centres and arrange them under appropriate headings.

Thermal resistivity.—Both the actual figures and the formula used for their evaluation have been commented upon by several speakers. It should be made quite clear that the larger differences in the value of the thermal resistivity are not due to the formula, some of the largest variations having been found with cables of the same type but coming from different sources. As regards the formula for three-core cables, this may admittedly in certain known cases give results which are inaccurate to the extent of about 20 per cent. Mie's formula * is liable to differences of somewhat the same order, and experiments are now in progress to check them and, if possible, to evolve empirical formulæ to give the required results. It should, however, be pointed out that the first observed series of differences between various cables were very large indeed, and the formula served a useful purpose by providing a basis for comparison between the various types, i.e. single, concentric and three core, to a degree of accuracy a long way within the extreme limits of differences observed. It is suggested by Mr. Welbourn that knowledge of these large differences will lead to an improvement of the cables in other respects, and the suggestion made by Mr. Teago and other speakers regarding the cause of and the cure for the very high values will no doubt be most useful to cable manufacturers.

Mr. Sparks and others questioned the validity of the statement regarding the effect on the current-carrying capacity of the thermal resistivity. It was not intended that the point should be graded down, but it appeared to be desirable to make it clear that the current-carrying capacity was not inversely proportional to the value of the thermal resistivity.

Grouping of cables.—Mr. Sparks and other speakers have laid stress on the importance of having information covering the case of when two or more cables are laid up near together. It is agreed that this very necessary information should be available as early as possible, and work in this direction is now in progress.

Expansion of cables.—It is suggested by various speakers that the amount of expansion is likely to be the limiting factor in regard to temperature-rise. This is a point which requires full consideration, and it would be interesting to have information regarding actual instances of breakdown due to abnormal expansion. Lead-covered cables have now been in use for a large number of years and have at times been run at a high temperature, but the Committee have not hitherto had any instances of trouble due to expansion.

* *E.T.Z.*, 1905, vol. 26, p. 137.

Limits of permissible temperature.—This question has been fully discussed and various interesting suggestions have been put forward. At present the limiting factors appear to be: (1) mechanical and electrical strength of the dielectric; (2) dielectric losses in high-pressure cables; (3) effect of abnormal expansion. The investigation of mechanical strength appears to be the first line, and it is interesting to find in a recent issue of the *Journal* of the American Institute of Electrical Engineers no less than five papers giving results of work done on these lines. It is a matter of great regret that it has not been possible to make the arrangements necessary to proceed with this part of the work.

Time-constants and variable load rating.—Various speakers have called attention to the importance of the determination of the time-constant of cables and the divergence of the heating curves from the logarithmic form. The question is undoubtedly of importance in connection with the intermittent or variable load-rating of cables, but the suggestions made regarding the use of the early part of the heating curve as a means of determining the final temperature by means of the time-constant are not likely to give very accurate results. It should be pointed out that the form of the curve will not be logarithmic, since the temperature being measured is that of the core only, and therefore will rise more quickly, especially in the initial stages, than the simple exponential formula would indicate. The increase in the number of watts developed as the cable heats up is a further reason why the curve will not be strictly logarithmic. The question of intermittent rating will, however, be dealt with experimentally and be the subject of a further report.

Depth of laying.—The Report covers only a very small part of the work in progress in Newcastle and elsewhere on the conditions external to the cable. Since the Report was in print it has been possible to make further tests at greater depths up to 10 feet, and these, by showing only a small increase in temperature-rise over 4 feet, confirm our doubts of the two curves in Fig. 21 which show the greatest change with depth. These two curves were obtained 9 months after the others, and no doubt the relative moisture-content at the various depths has altered fractionally in the interval. It is most difficult to make a true "control experiment" on actual laid cables, as it is almost impossible to narrow the control down to any one variable. It has been suggested that the percentage figures in Table 10 should be referred to 2 feet as a base in order to conform with the rest of the Report. If this is done, and only Tests Nos. 1 to 5 are admitted, we get an average reduction in temperature-rise of 5 per cent at 1 foot depth, and an average increase of 8·3 per cent at 4 feet. Then the multiplying factor for current-carrying capacity for varying depths, with standard tables calculated for 2 feet depth, would be: 1 foot, 1·025; 4 feet, 0·96. While it is quite true that cables cannot always be laid throughout the route at the most economical depths, the Committee thought it essential to know what effect change of depth has, as the carrying capacity of the cable is settled by its hottest point.

This general explanation is in reply to points raised by Messrs. Sparks, Gillen, Martin-Cooper, Ratcliff, and Selvey.

Mr. Welbourn asks for some figures of the joint effect of seasonal soil temperature variation, and depth of laying, on current-carrying capacity. We give figures in a table below in answer to this question. These serve also to illustrate the difficulty of making control experiments in the ground with any certainty of repeating the conditions over a period of time. It is established by the meteorological records in different parts of the world that soil temperature increases with depth during the cold months, and reduces with depth in the hot months, the actual values varying largely in different countries. As the real object is to run a cable to a certain total temperature (as distinct from temperature-rise) the combined effect of seasonal temperature change and depth is very marked, and for the range shown in the Table there is no less than 14 per cent variation in the current-carrying capacity of the selected cable between the extreme conditions. For the purposes of this example 80° C. was selected as the total temperature of the conductor, which was 0·1 square inch, three-core lead-covered and armoured, laid direct in the ground. This example is worked out by applying the results of Table 10 in the Report to the available rise left after subtracting the various soil temperatures from 80° C., and we get the following figures :—

Currents for an Actual Conductor Temperature of 80° C. with Various Depths and Seasons.

Depth of Laying	1 Foot			2 Feet			4 Feet		
Month	Soil Temperature	Available for Cable Rise	Current	Soil Temperature	Available for Cable Rise	Current	Soil Temperature	Available for Cable Rise	Current
	°C.	deg. C.	amps.	°C.	deg. C.	amps.	°C.	deg. C.	amps.
February	3	77	293	5	75	284	6	74	268
May	11	69	278	10	70	275	9	71	262
August	17	63	266	16	64	263	15	65	252
November	8	72	284	9	71	277	10	70	261

Thus, the maximum current for 1 foot depth in February must be reduced by 14 per cent for 4 feet depth in August, with the same conductor temperature : but the same change of depth affects the maximum current 8·5 per cent in February but only 5 per cent in August. These are only two of the external variables ; others that have to be taken into account, apart from grouping, are change of moisture content, nature of soil, and air pockets in filling in. Work is at present proceeding on these, and it is evident the combined effect of the possible variables external to any given cable will be very large. In reply to Mr. Riley, a series of tests are now in progress in homogeneous material under cover, to determine the lines of heat flow under such ideal conditions, which will provide proof or refutation of the Teichmüller formula.

When the Committee has settled the safe total temperature for any class of cable, it will be everyone's aim (apart from questions of voltage drop) to run their cables nearer that temperature, or, at least, to know how near they are to it. The live conductor of a cable may be at the limiting temperature due to :

(*a*) Heavy load and excellent cooling, i.e. large gradient within the cable, and little externally ; or

(*b*) Very small load and very bad cooling, i.e. small gradient within the cable, and a large external drop.

The two main variables which determine the conductor temperature in each particular case are : (1) the temperature of the sheath, and (2) the current in the conductor, and it should be practicable to obtain a satisfactory live-cable thermometer by a device adding electrically the temperature of the sheath to the rise of the conductor over the sheath obtained artificially from a dummy having similar thermal characteristics to the cable, supplied by a current transformer in the cable circuit.

Professor Thornton suggests that the difference between the temperature coefficients may be due to inaccuracy in the resistance box, but we think that the comparatively small difference is due to variations in the samples of copper. He also states that the rates of rise of temperature in Figs. 1 to 5 are approximately logarithmic, but that they appear to be complicated by variations of the loss coefficient. A simpler explanation is that in the case of the core temperatures the core is the hottest part, and the dielectric and sheath lag behind, so that the exponential formula which assumes thermal homogeneity is not strictly applicable. The time-temperature curve, especially at the beginning, would therefore be steeper than the theoretical curve. As regards the heating curves of the outer sheaths, the reverse will be the case, the slope of the curve being less steep than that deduced from the exponential formula. An examination of the curve in Fig. 3, for

example, shows this quite clearly. These facts and the difficulty of accurately determining the tangent to the curve would therefore seriously invalidate any value of the time-constant obtained in this way.

The rise in the value of the index in Fig. 8 has been noted, and the deduction that Professor Thornton draws from this, that the cooling is proportional to some law with an index less than unity, is very interesting and deserves further investigation, in view of the fact that the predominating factor is undoubtedly the loss by convection currents. It would no doubt have been better in Fig. 5 to plot the time taken to attain the maximum temperature against the outer diameter of the cable, as on the assumption that the heat capacity of the cable is proportional to the diameter the points should then have lain on a straight line.

In reply to Mr. Bertram, the particular case which he mentions is a difficult one. Obviously, if the dimensions of the manhole are small, the short length of cable in it would not necessarily rise to the maximum temperature, but it appears to be a matter for actual temperature observations in the manholes.

Mr. Allan will be glad to know that the Committee are already in communication with members in various parts of the world with a view to obtaining data regarding the characteristics of the soil. In reply to the point regarding the temperature of the insulation between the cores, our actual measurements have been made both on the cores themselves and on the lead sheath, and the final figures refer to the temperature of the core.

Mr. Prangnell has raised a number of questions of interest, although the point of them is not always clear. The effect of the laying medium is indicated fairly clearly in the Report, as is also the fact that, from the point of view of heating, the addition or otherwise of a layer of impregnated jute yarn will make very little difference to the results. With regard to his questions regarding the contact of the cable with the duct, it is certainly better to lay the cable flat and to obtain as good a contact as possible. Theoretically, the contact with the duct can only be at a series of points, but it is almost impossible to say what will happen with the combination of rather soft compound and expansion due to heating.

Mr. Ratcliff is to be congratulated on having evolved a method which has given satisfactory overall values for core heating plus dielectric losses, and his contribution in this respect is of great value to the Committee.

Mr. Gillen touches on a very important point in regard to the determination of dielectric losses. It is one that will be investigated, but it is doubtful whether it will be possible to supply, as he suggests, a precise specification of the texture and grading of the insulating material involved.

Mr. Selvey refers to previous work with which he was connected, and criticizes both the apparatus used and the method adopted of laying the cables for the tests in air, and we must join issue with him on most of the points he raises. First, he states that "much more simple apparatus was used," but does not say what this was. Speaking with some knowledge of electrical measurements, we must say that the apparatus which we used was the most simple that has yet been devised for determinations such as we had to make, bearing in mind the factors which any experimentalist has to consider when selecting apparatus for a particular piece of work, e.g. (1) accuracy, (2) ease and certainty of operation and observation. With his criticisms of the "control" experiment, we are likewise in total disagreement. A wooden floor is a moderately well-known piece of apparatus and scarcely requires any scientific definition; the same applies to the surface of the floor, which is usually reasonably flat and smooth. The other points which he suggests require further definition are: (1) the temperature of the envelope, (2) the exact relative position of the two limbs of the cable. The first is dependent on the thermal resistivity of the dielectric and thickness of the particular cable, and is not greatly affected by the contact with the floor. Moreover, Figs. 2, 3, 4, etc., do give the temperature of the lead sheath for certain of the cables. Regarding the second point, it is correct that in one or two cases the cable was laid back on itself in order to compare results with others previously obtained with cable laid in this way, but, apart from these, the cables were not necessarily laid in two limbs. Where they were, the limbs were sufficiently far apart so that one did not affect the heating of the other.

The main question, however, is whether the cables should have been suspended in air or laid along the floor. We have made some tests and are making others to determine the exact difference between the two methods; present indications are that there is no great difference between the two methods, but, even though there were, we think that the method we adopted is much more easy of reproduction than to suspend the cables in air. Wood floors are used all over the world and, in any case, the area of contact of a cable on the floor is so very small that it appears probable that very much more complicated apparatus would be required to determine the difference between the effect of a wooden floor and, say, a concrete or iron floor. We are open to be convinced on this last point, but should like to see results of actual observations with different types of floor before agreeing that there is a substantial difference. What is likely to produce appreciable discrepancies in results, is small air currents. The effect of these is much greater than is usually appreciated, and they are much more difficult to exclude with a wire or cable suspended in mid-air, and particularly when it is susceptible to a slight swaying motion.

Mr. Selvey also casts doubt on the values of K determined in this way, but we would point out that several of our determinations of K were made both in air and with the cables buried under the three standard conditions of laying, and the results were in every case in good agreement.

The suggestion that the Agricultural Departments might have information regarding the characteristics of the soil was fully gone into in the early stages of the work, and much inquiry revealed the fact that agriculturists are apparently concerned only with the upper few inches of the soil. Also, the collation of

the results at the various depths is not so difficult a matter as would seem from Mr. Selvey's remarks—it is obvious that full information is required as to the effect of laying at various depths over a wide range, and this fact has been borne in mind throughout, there having been the closest co-operation between the various investigators.

Mr. Selvey's later remarks regarding the thermal resistivity of multicore cables and the grouping of cables are dealt with elsewhere, but we are not very clear as to the further explanation required regarding the concentric cables. In general, however, we hope it is clear that the results are not dependent on the experimental conditions, but rather that the experimental conditions were such as to afford a true comparison between different samples of concentric cables.

It appears very likely that the use of factors which Mr. Morton suggests for the different types of cables and soil, etc., will be the manner in which the final values will have to be stated, and all of his suggestions will be valuable in this connection. We quite agree with Mr. Morton that the dielectric heating will not be constant for all samples of cable. It would be most helpful if the cable-makers would determine other figures for the various types and grades and supply them to the Committee, so that they could be embodied in the final figures. With regard to his point about the effect of braiding and armouring, some information on this point is given in a previous paper (*Journal I.E.E.*, 1911, vol. 47), and a great deal more in a paper by Professor Porter relating to steam-pipes (*Philosophical Magazine*, vol. 20, p. 511). We found that in every case where definite observations were made of the cooling, the cooling curve was very nearly identical with the heating curve.

Mr. Nisbet refers to Kelvin's law of cable cost and energy losses, and suggests that it is only the larger cables that are likely to be loaded sufficiently to approach the limit of working temperatures. Speaking without the full data necessary to deal with this point, one can only say that there is apparently a very wide divergence of opinion as to the point at which it becomes cheaper to lay a larger cable, and some detailed calculation would be welcome.

With regard to the suggested method of determining the temperature of a cable by means of measurements made on the lead sheath, it should be made clear that this method was put forward as an alternative to the way largely used of putting a thermometer in a duct and taking the indication as representing the true temperature of the cable. For a cable loaded continuously, the method may give good results, but owing to the time taken for the heat to reach the sheath it is not so satisfactory for short time-rating. The temperature at which the paper was tested was not taken as being that at which cables should be run, but as enabling a discrimination to be arrived at quickly.

We agree with Mr. Fuller that the thermal capacity of the ground requires consideration, but, from an experimental point of view, it is easier to determine the watts lost per square centimetre of surface on an actual cable than to work with lead water-pipes. The point mentioned regarding the maximum temperature of a cable under normal working pressure appears to be most important and, although there is at present no cable available with a special central wire, we shall endeavour to investigate the matter.

We are sorry that Mr. Riley considers that the effect due to laying a cable in the ground has been neglected in the first portion of the Report. It is true that the temperature difference between the inside and the outside of the cable is not given for every case, but since this difference will depend (*a*) on the thermal resistivity of the dielectric, and (*b*) on the nature and temperature of the surrounding medium, it was thought that a full table of the values of thermal resistivity and a few curves showing the effect of the surrounding medium, together with the formulæ used for the calculations, would be sufficient. The temperature of the lead sheath was in every case measured; this is clearly stated in the Report and therefore assumptions were not required. The point regarding tests on cable drums will, no doubt, be fully appreciated. Since, however, the purpose of the investigation is to provide data for cables laid under practical conditions, it will be obvious that the tests on drums, suggested by Mr. Riley, would involve very full investigation into the type and size of drum used.

Mr. Digby has dealt with the question of varying quantity of oil in the cable, and it seems probable that there is likely to be a difference both in life and in initial cooling, due to the degree of impregnation. It would seem that the solution of the difficulty is to ensure that all cables should be completely filled and impregnated.

We have to thank Mr. Rosling for drawing attention to possible confusion in the use of the terms "conductor" and "core." This will receive attention in the final Report. His point regarding the change in thermal resistivity of the soil has been under consideration for some time past, and an arrangement has now been designed which it is hoped will enable us to obtain observations of the thermal constants of the soil actually surrounding the cable.

Mr. Dunsheath has contributed a most ingenious method of analysing the results, which will undoubtedly be of value when the final evaluations have to be made.

We have again to thank Dr. Russell for his interesting and helpful contribution. It should, however, be made clear that the fact that three-core cable had a "lay" was recognized and taken into account in the calculations.

Mr. Beaver has raised a number of points of great value, which require full and careful consideration by the Committee. We fully agree with him that it is essential to obtain a great deal of information regarding the thermal characteristics of various types of soil, and this is being carried out. The alternative figure given for the thermal resistivity of bitumen may go a long way to explain the results we obtained with cables laid in bitumen as compared with those laid direct in the ground. Further determinations have now been made of various bituminous mixtures, with results as follows: Bitumen, 508; Pitch compound, 250; Asphalt, 120.

The differences, while they confirm Mr. Beaver's

remark *re* cables laid in asphalt troughs, are very much larger than we should have expected, and it seems clear that while the results now obtained are valuable as showing the extent of the possible differences, further and more detailed examination of the various mixtures will have to be made, since it is evident that small variations in the nature of the compound may make large changes in the thermal resistivity.

Professor E. W. Marchant (*in reply, communicated*): Mr. Wedmore has mentioned that this is the first report which has been submitted by the Electrical Research Association. I should like to express my satisfaction that this Report contains the results of some investigations that have been carried out in university laboratories. There is no doubt that a large proportion of the work undertaken by the Electrical Research Association will, by its nature, have to be done in works and power stations where large-scale plant is available, but I think that certain branches of it may well be done in the university laboratories, which happen to have the necessary equipment to enable them to carry it out. With reference to Mr. Sparks's remarks as to the straight-line relation which we have found between heating and watts loss per foot run, this is exactly what one would expect to get from theory. We found that the temperature-rise of the ground immediately surrounding the troughs in our test was comparatively small, not more than 3 or 4 degrees, when the temperature-rise of the cable was 30 or 40 degrees. It seems unlikely, therefore, that the grouping of a number of troughs carrying conductors laid on the solid system will affect their current-carrying capacity by more than a few per cent. With regard to the question of deterioration, it may be of interest to record that, although the final temperature in some of our tests was as much as 68° C., the cables did not appear to have suffered and have since been dug up and are being used in connection with the Corporation supply. With regard to the question of loss due to dielectric hysteresis, I hope that it may be possible for us to make some measurements. This subject seems one which a laboratory, such as ours, might well undertake.

Mr. Welbourn has expressed the view that the solid system of laying cables is moribund. I am not sure that all central station engineers will agree with that view, and if I may make a reference to the Report from the National Physical Laboratory, I should like to point out that, from the point of view of heating, the cable laid solid in bitumen is, if anything, rather better than the cable armoured and laid in the ground, besides being less liable to accident and injury. With regard to his statement that the continuous working temperature of bitumen-insulated cables is 110° F., I have expressed the opinion that this is a very safe figure to take; actually it might be increased, I believe, by 10 degrees without running any serious risks.

Mr. Kapp points out that the usual assumption is that the temperature-rise curve is exponential. The exponential curve of increase of temperature with time holds only for the case in which the heat energy supply is constant and the rate at which heat is dissipated is strictly proportional to the temperature-rise. The resistance of a cable, when it is cold, is considerably less than when it has been heated: consequently, if the current through the cable is maintained constant, the heat generated in the cable will increase as the temperature rises. If the temperature-rise is, say, 30 degrees C., the amount of heat developed per second when the cable is hot will be about 12 per cent greater than when it is cold. This, obviously, will have an effect on the shape of the heating curve. Further, the rate at which heat is taken away from the cable is not strictly proportional to the temperature-rise. When the ground surrounding the trough is heated, its thermal resistivity will change, possibly through drying out, and this also will affect the shape of the curve. This point has been raised by Professor Thornton in his contribution to the discussion. It must be emphasized that if the exponential law holds, there would be no object in making this research at all, because we should know definitely the temperature that the cable would reach, as the result of a short run. The point which is of greatest interest in our investigations is the fact that, after the cable has been run for 6 hours, the increase in temperature-rise is as much as 20 per cent. If one takes the curve in Fig. 27, for example, and assumes that it is an exponential curve, the heating time-constant, as ordinarily understood, is the time taken for the cable to attain 63 per cent of its final temperature-rise, and from these curves that would be approximately 35 minutes, so we should expect that a comparatively short run would enable one to find the final temperature. Actually, the cable has to be run for 3 or 4 days before a proper estimate of the final temperature can be made. Although departure from the exponential curve may be very considerable, it should be possible to obtain an approximate figure for the heating time-constant, which would enable a rough estimate of the temperature-rise to be made for intermittent loads. I am afraid that Mr. Kapp's idea of having a definite heating time-constant for a given cable is hardly practicable. It is obvious from the three curves given for the same cable with different temperature-rises in Figs. 27–30, that the shapes of these curves are somewhat different. This, I think, is due to the fact that the thermal resistivity of the material varies slightly; although it is undoubtedly due also to the increased heat developed at the higher temperatures.

Mr. Riley has referred to the fact that it is desirable to obtain separate figures for the thermal resistivity of the cable and of the trough in which the cable is laid. I hope that when this investigation is complete, it will be done in all cases, as it will obviously make the results of much more general application. I am inclined to agree with Mr. Beard's remark that the difference of temperature-rises between cables drawn into a duct and those laid in the ground would be somewhat less on grouped cables than on single cables.

With regard to Mr. Dunsheath's criticism of the Report, I should like to point to the comparatively simple formula which has been included near the end of the Report, in which the temperature-rise of any section of cable can be approximately estimated. The basis of the formula is that the temperature-rise is proportional to the watts loss in the cable, and that,

as I have already pointed out, is the really sound basis on which to work. It would be interesting to plot curves showing the way in which the temperature decreases from the core to the ground. Unfortunately, in our tests, the cables were unsheathed, so that we were unable to measure the temperature at the surface of the cable when it was laid in the ground. This was estimated from the results that were made subsequently, when the same cable was suspended in air.

Mr. Ratcliff has referred to the fact that with low-pressure cables economical considerations are the limiting factor before the cables reach a dangerous temperature, and also to the fact that the pressure drop may become excessive. From this point of view, the results that we have obtained are more or less of academic interest; at the same time, the figures given as to the watts loss per foot run are of general application. If a high-pressure cable were laid under similar conditions to those employed in our tests, and the watt loss per foot run produced the same temperature at the sheath as was found at the surface of our cables, the loss of heat through the trough and ground would be the same, that is, the figure for G would be the same, hence it follows that if tests were made on three-core cables and the sheath temperature were found with a given current passing through the cores, the rate at which heat was dissipated from the sheath to the ground could be estimated and hence the current-carrying capacity of the cable could be found when buried in the same way, that is, when laid in stoneware troughs filled with bitumen. Another point in the Report which may be of value is the series of curves showing the rate at which the temperature will rise after the current first passes through the cores. The application of these curves, of course, is to estimate the effect of overloads on these cables. For example, if a cable broke down, so that another cable had to be loaded to two or three times its normal rating for half an hour or more, then it will be possible from these curves to estimate the temperature-rise which the overload would produce, and whether the overload would be likely to injure the cable. It is interesting to notice that the formula given by Mr. Ratcliff is similar in form to the one which we have given at the end of the Report, that is, that the current-carrying capacity is proportional to the square root of the area of the core. We have not included a term for the diameter of the sheath as we have not sufficient data to enable us to estimate it. It seems, however, reasonable to suppose that the larger the diameter of the sheath, the greater the amount of power that can be wasted in the cable, before producing a given temperature-rise on the sheath. The area from which the heat could flow would be proportional to the diameter and, assuming the heat loss per unit area to be the same, it follows that the current through a given section of conductors inside the sheath to produce a given temperature-rise would be proportional to the square root of the diameter of the sheath.

Mr. Beaver has also referred to empirical formulæ. There is always a danger, in work of this kind, that the investigator should be led to study the physical constants involved in a given case rather than the conditions existing in the cable itself. It is for that reason that I think the empirical formula which I have already referred to, which has been given at the end of the Report, is of considerable value, and I hope that other empirical formulæ will be developed on other types of cable. The same point has been referred to by Mr. Selvey.

With regard to Mr. French's criticism of the Report, the cables were laid solid in bitumen and the ends were drawn up through drain pipes filled in also with bitumen, the method being that used by the Corporation in their street boxes. A comparatively small length of the cable was exposed to the air, and it was this that produced, probably, the greatest differences. I have already referred to the fact that the time-constant for a cable is not a definite number, and that is the reason why no time-constants have been calculated. The curves themselves must be used in determining the temperature-rise with intermittent loads. The calculations given by Mr. French show that a figure could be found for the heating time-constants which enables the temperature-rise to be calculated to a fairly high degree of accuracy, but, as I have already pointed out, the curves are not strictly logarithmic, and the final temperature-rise which has been assumed in making Mr. French's calculations is not the true final rise after the cables have been heated up for three or four days.

Mr. Morton has referred to the heating caused by dielectric losses on cables working at 20 000 volts or over. I hope this subject will soon receive more elaborate and detailed investigation. The question of expansion is a most interesting one; naturally we did not have much trouble with the comparatively small length of cable with which we had to deal in our tests. An explanation of the fact stated by Mr. Morton, that with a small wire a covering with a braiding of cotton soaked in ozokerite tends to cool the wire, is that with a small wire a comparatively thin covering will appreciably increase the surface from which the heat is radiated, and the extra thermal resistivity which is introduced by the covering is more than compensated by this additional cooling surface. When, on the other hand, the cable is of relatively large section, the increase in surface due to the covering is relatively small, and the extra thermal resistivity of the covering produces a lagging effect, so that the temperature-rise of the wire is increased. There will obviously be one section of wire for which the extra cooling caused by the increased surface would exactly balance the extra heating due to the lagging effect of the covering.

The calculation made by Mr. Fuller of the effect of specific heat on the temperature-rise of the cable is of interest. It is only during the period when the cable is heating up that this effect is important. When steady conditions have been reached, specific heat does not enter into the question. As he says, specific heat will affect the overload capacity of the cable, but it is, of course, taken into account in the curves which have been included in the Report. The calculation that he gives for the time taken to raise the temperature of a cable 10 degrees F. is of great interest. It is quite possible that for cables which have to withstand con-

siderable overloads for short periods, special material of a large thermal capacity might be desirable.

Mr. Holttum has referred to the variations observed in the thermal resistivity of the material. The thermal resistivity is a comparatively difficult quantity to measure with a high degree of accuracy, although it is much easier to get results with lead-sheathed cables than with unsheathed cables. I think his suggestion that variations might be due to variations in the intimacy of contact between the copper or lead and the insulating material is probably correct.

Professor Thornton refers to the differences that have been recorded in the temperature coefficient of copper. This quantity does vary within considerable limits in cables. In our experiments we found differences in cables of exactly similar type and construction of as much as 1 per cent, although the copper used in the cables was assumed to be pure. The figures given by Mr. Melsom with the larger number of samples that he had to test show still wider fluctuations, and any difference that there is between the average figure for the temperature coefficient given in our results and his is fully explained by these differences. With regard to the variations in resistivity, it is of course very difficult to base measurements of resistivity on these tests. The cable sections given are nominal, and no allowance has been made for the effect of laying up. I think there is no doubt that the cable manufacturers take very good care that the copper that they purchase has good conductivity. The determination of this quantity may be made with great accuracy on a solid wire, but it cannot, of course, be determined with any degree of precision on cables built up with, say, 90 strands or more.

ADDRESS TO THE STUDENTS' SECTION.

By Llewelyn B. Atkinson, President.

(*Address delivered at* Birmingham, 4 *January, and at* London, 18 *February*, 1921.)

In addressing the Students of the Institution I may point out that in addition to the Students' Section in London which has 406 members attached, there are 5 other Students' Sections, and the total number of Students on the roll of the Institution is now 1 756, of whom 1 138 are attached to Students' Sections.

The Institution Council attach the greatest importance to the Students' Sections; they are the training ground for our future electrical engineers and, in addition, the recruiting ground from which the future membership of the Institution will be obtained.

The late Silvanus Thompson, in his opening address to the students at the Finsbury Technical College, used to say that possibly among the students were future Presidents of the Institutions of Civil, Mechanical, and Electrical Engineers, and so among the Students of this Institution we may reasonably look for our future Presidents. My own recollections go back to the days when I was a Student of the Institution of Civil Engineers, which I joined because my father was a civil engineer, and I realize now the great advantages which accrued to me from listening to, discussing, and reading papers before the Students' Section among my own equals.

The regular attendance at meetings of the Students' Sections and the training it provides in taking part in public meetings enable the Student later to take part in the proceedings of the Institution and, provided the speaker in these meetings speaks to the point and on a subject on which he can give new or relevant information, he is not only helping his fellow members, but obtaining a useful and proper publicity for his own abilities. Such publicity is the means of making acquaintances and coming into contact with workers in the same line, and not infrequently of professional or business opportunities which might not otherwise have occurred.

In an address to students or young engineers it is natural that an older man should be called upon to draw upon his experience for the benefit of those who are at the threshold of their careers. In so doing it is also only natural that the individual reviewing the causes of his successes and failures should perhaps come to the conclusion that his experiences are more or less general, whereas it is equally probable that they are particular, or there would be no difference between the outlook and aim of individuals.

For one the greatest satisfaction will accrue from the possession of wealth, not merely as an expression of the possibility of realizing the amenities of life, but for its own sake and the power and prestige it gives.

For another the pleasure of designing and executing new and interesting works, or of achieving scientific discovery, will compensate for quite moderate means of livelihood.

For yet another the ambition to stand in the forefront of his fellow men will, if satisfied, compensate for a lifetime of arduous labour.

Whilst yet again the leading of a full life, the life of

a home and family, the devotion of time and study to other subjects than engineering, such as nature, literature, or travel, will justify a departure from total absorption in engineering work, making this, in fact, a means to these other amenities.

Now it may be at once granted that in nearly all cases the greatest success will be obtained for a given ability, if that ability is devoted solely to one subject. In any case it is well to specialize on some branch of a subject to the extent of being a recognized authority on it.

It is also well to recognize that, for a given level of ability, material success will be greater if that ability is devoted directly to the commercial rather than to the purely scientific side of engineering.

Let me make this point quite clear. Ability covers such qualities as clear appreciation of the elements of a problem, sound judgment as to the weight to be attached to these elements, the power to *decide* on the most suitable solution of the problem, and the power to *utilize* the solution decided upon.

It will be seen that these qualities are equally applicable to an engineering or a commercial problem. If an engineer is to design a machine he must clearly apprehend the purpose of the machine, the space available, the weight permissible, the speed at which it may be worked, and the price it will fetch. He must first decide on the conflicting claims of these different elements, and be able to make up his mind as to the degree to which he will finally admit these claims. Having done so he will complete the design, and will finally be able to convince others that it is correct and superior to any other.

The commercial engineer will have to deal with the same problem, but probably at a later stage. That is, knowing the elements, and having more than one solution put before him, in the shape of completed designs at stated prices, and knowing the possible market, he has to choose between them and finally to convince the customer that his selection is the best possible one.

But mark the difference in the material reward. The designing engineer will only slowly feel the benefit arising from a correct solution; he will eventually do so by the desire of his employer to retain, or other employers to obtain, his services, and this will affect his salary. The commercial engineer will almost immediately realize the result of his decision, and this will much more quickly be reflected on his earning power, either as employee or as a manufacturer, than in the case of the designing engineer. Probably it is from this cause that the best-paid posts in engineering work are on the selling or commercial side rather than on the strictly engineering side. On the other hand, the weeding out of ability not equal to its tasks is more ruthless.

A friend of mine once remarked of the members of a learned Society that they were nearly all disappointed men, and that the majority of them nursed a grievance. These men are each eminent in some scientific domain, and yet their material rewards are not much greater than very moderately successful tradesmen would receive.

A union of scientific workers has recently been formed and has issued a manifesto on the exploitation of scientific research workers by commercial firms. What I think they intended to convey is this: that a scientific discovery made by a person who is definitely employed on research work is in a different category from a design by a draughtsman paid to design, or a cook paid to cook dinners. This, to my mind, is entirely fallacious.

Research workers have only to show by results that their employment is sufficiently profitable, when those of them producing results will be able to demand equivalent payments. It is true that some forms of research do not lend themselves to private ownership. Such researches should be undertaken by the State. If research workers consider that they are exploited they should work in co-operation, devoting themselves to research and employing commercial people to dispose of their discoveries, and the fact that they do not do so indicates that the exploitation of discovery is more difficult than the making of it. This brings me back to my starting point, viz. that for equal abilities a commercial engineer will obtain a higher reward than a purely scientific engineer. I do not, however, think that the scientific worker has any cause for complaint. He has an occupation giving admittedly great mental and intellectual pleasure, greater *réclame* if success is won, a more regular life, less anxiety and less risk.

So to you on the threshold of your careers I suggest that, whatever your engineering attainments, you should study the principles of business, of organization, of finance, and of economics. These subjects can be considered from a point of view quite as scientific as the problems of engineering. Those who would like to dip into such a subject as economics might study two small books. The first of these (published by Messrs. Frowde and Co.) is: "A Geometrical Political Economy; an Elementary Treatise on the Method of Explaining Some of the Theories of Pure Economic Science by means of Diagrams," by H. Cunynghame. One of the earliest writers to use diagrams or graphs to illustrate political economy was Fleming Jenkin, at one time Professor of Engineering at the University of Edinburgh, who was associated with Lord Kelvin in the early Atlantic cables and author of a book on "Electricity and Magnetism," which in 1880 was largely read, but which is now possibly forgotten. This book of Cunynghame's gives a fascinating study in a short compass by means of graphs of nearly all the principles of economics applicable to business.

The other book is a reprint from the *Electrician* of articles which appeared in 1918 and 1919 under the title: "The Economics of Works Costs," by J. R. Dick, B.Sc., who is the head of an engineering works producing electrical specialities. This book shows the application of simple analytical methods to the solution of the problems arising in works' costs, and illustrates the method of applying analytical methods to the various problems arising in productive businesses.

I mention these two books—one graphical, one analytical—because some students prefer one method

and some the other, but either illustrates my point that business problems may be studied by very powerful methods capable of use by those who, like engineers, are trained in these methods.

Consider again the questions of organizing production in a works or the carrying out of contracts external to works. Some 30 years ago methods of organization, both inside works and as regards the carrying out of engineering work outside, were in a very crude and chaotic state. Very little general information existed on costs of doing work or on the output of tools and plant. When I was apprenticed in Kitson's locomotive works, I did a good deal of timing of drilling operations, cutting speeds and so on, but it was left very much to individual taste to decide what to use. The keeping of costs was very perfunctory; each man entered up on a chalked board how long he was at work and the order number he was on, but there was no check and much manipulation. All these methods have now disappeared, and costs are accurately ascertained, and checked by staffs specially employed for the purpose. Engineering students should study these questions; there is plenty of literature on the subject, both permanent and in magazine form.

Outside contracts for erecting were in those days left very much to foreman erectors, and on the whole the system worked well, machinery being fairly simple and of few types. These men stayed all their lives with one firm and took a great pride in their work. To-day with much heavier and more complicated plant, or cable work with jointing which requires the very greatest care, and with working staffs of a much more migratory character, the planning of the work is done at the head office and the control of the execution entrusted to a young trained engineer.

I strongly urge you to seize every opportunity of getting on to such outside work; there is no better training in initiative and in overcoming unforeseen troubles, and there is no way in which a young engineer of ability can more readily and forcibly come to the notice of his superiors than by a successfully completed erecting contract, where foresight and energy and the ability to keep the working staff going without loss of time have told their tale by completion before the expected date.

Do not be afraid to study the markets and prices of the raw materials engineers have to deal with. Changes in methods of production, in demands by allied industries, and discoveries of new sources of raw materials, produce very great changes in the relative values and importance of different raw materials, and only by following or predicting these changes can advantageous changes in design be brought about.

Study the elementary principles of contract law, the transfer of property or rights to property and the consideration given, and how contractual conditions may sometimes arise even without intention. Make yourself acquainted with the legal obligations and rights of employers and employed, including employers' liability for accident, and for workmen's compensation. Study the principles of the capitalization and control of limited companies, and prepare for yourself a chart showing how the control and responsibility in your own works is delegated by the owners, that is the shareholders, to the directors, and thence by steps to the office boy or the floor sweeper.

You will often have men in your charge much less capable than you are of judging risks of accidents and injury. Never allow such men to be where—if any accident to plant or lifting tackle takes place—they will be injured. Special care in this respect is necessary with temporary structures and lifting appliances that have not frequently performed the same operation. "Safety first" is a very good watchword; it may seem a light matter to let men run risks, but it would be a lifelong sorrow to any engineer to think that a fellow-being had been killed or maimed for life by any lack of rigid care in this respect.

Engineers receive a scientific training which should lead to scientific accuracy in thought and expression, but there are little lazy habits of thought and expressions which mar otherwise useful work. The purpose of language is to express ideas, and although the English language is not so full of fine shades of meaning as the French language, it contains a great choice of words suited to express kindred ideas. The taking of trouble in thinking out and framing expressions is rather out of fashion, but this makes it the more easy to mark one's way in the crowd. When writing letters or reports read them over once or twice, for it is possible that some phrase you have used may have two meanings, one of them not intended. Suppose yourself to be the recipient who knows nothing of the matter reported upon; have you stated all that is necessary for a complete statement of the case, are all the dates correct, and have you checked all the figures, or do you only "think" that something stated is right? These are things about which young engineers are not careful enough; care comes with age and experience. I read in my father's diary in which he wrote of his engineering work: "It is a pity X is so careless, we have had to re-level a whole section of the railway again, owing to his inaccuracy." X afterwards became a very celebrated engineer and a President of one of our Institutions, but such carelessness might have lost him advancement at some critical moment.

Make the acquaintance as far as you can of men of ability and position in your own and allied professions, as such men are usually anxious to help the younger men in the profession, especially if they know that they are working seriously at the problems of science and industry. I speak gratefully now, as I have done on other occasions, of the help and guidance given to me as a young engineer by such men as Sir William Siemens, Sir William Preece, Professor Ayrton, Dr. Silvanus Thompson, and many others.

Realize the immense importance of personal character and regularity of work and life. A young engineer who used to be employed by my company applied for an important post, giving my name as a reference. The man who was to choose the candidate asked me to see him, and I did so. Replying to his question, I said: "I do not know whether his qualifications are what you want, you can satisfy yourself on that, but I will say this, he will always be at his post when wanted, and will never let you down." He thanked

me for that information and said: "We are always in this difficulty, that several men seem equally qualified, but lack just those qualities that tell in the end." The candidate I recommended obtained the post and has amply justified his selection.

A man of experience once said to me: "I do not know a single case of a man of reasonable ability, who works reasonably hard, and who has a good character and good health and is absolutely honest, who has not got on." Another said to me one day, speaking of the members of his staff: "There is a very small apparent difference between the man worth £100 a year and the man worth £1 000 a year; the first does his work well, but comes a minute or two late, and at the closing hour he has put everything away, has washed his hands and has his hat on ready to go. The man with £1 000 a year finishes when the job is finished, and has prepared his mind for the day's work by the time he arrives in the morning." It is the attitude of mind that leads to the great difference.

Another, the head of a great concern, once said to me: "There is plenty of room at the top of every business, but the lower parts are always full." The difficulty is to find jobs for people of only moderate attainments, and equally difficult is it to find men to fill jobs requiring high attainments.

Among the greatest of qualities to be cultivated is that of unfailing good temper in times of difficulty, of annoyance, and of strain. It can be cultivated, and it not only saves nerve strain and exhaustion to yourself, but it is infectious and helps others along at the same time. Few men realize the power of a smile. Some of the most difficult situations may be surmounted by a good-humoured word at the appropriate moment; it ionizes the atmosphere, and discharges the accumulated and opposite electricities.

Take part in the social activities connected with your works, or societies connected with your profession and industry. They provide a means of coming into touch with associates that you do not meet in your daily life and work, and it is another means of opening new channels of thought and new avenues of work and employment. It has the additional advantage that good friends are made who are a great pleasure in later life. You will not realize until you get older that men do not make friends in later life of the kind and depth that they do when young, and our real friends when we get older are those we have cultivated as young men. This is one reason why it is very important to be sure that the friends made in youth have the proper qualities to be worth a life-long acquaintance.

I must say a word or two on the deep and far-reaching social changes which are taking place around us. We are always meeting conditions which we cannot alter, and we must adapt ourselves to them. It might, for instance, be argued that the earth would be improved if the poles had an average temperature equal to that of our latitudes and the same increase of temperature on approaching the equator. But the discussion is useless. We must accept and adapt ourselves to the existing conditions.

It might also be argued that a social State with a leisured and cultured and learned or scientific class founded on a slave class or a wholly uneducated working class, and the first class ruling the whole State, is the social arrangement producing the greatest total comfort. It is certainly vigorously argued to-day that the ideal State is one in which everyone takes an equal share in the manual and laborious and tedious tasks and where education is as far as possible equally divided. But the real facts are these. To a large extent we are going through a process leading from the first of these towards the second. The process has advanced an immense way in my lifetime. Assuming, as I do, that the process will proceed in England by evolutionary and not revolutionary methods, it may take two or three more lifetimes to complete. But it must be realized that it is taking place. To the classes from which young engineers are drawn it must on the whole mean a diminution of *relative* wealth and position, but it need not necessarily mean a diminution of happiness and contentment. If education, refinement, and regard for others were equally distributed, much that we dislike in these changes would disappear This being so, if in our own cases education has had a refining influence, it may act on other classes in the same way. The great experiment in continued education which is just beginning will tell us a good deal, but not perhaps for 20 years. It is a long time to wait. In the meantime all that you can do is not to take any step that accentuates or emphasizes difference of class. Help those who are on a lower grade than yourself, show them that, as they rise, more is to be expected of them, realize that they have not had the training or environment that have been your advantages. On the whole, engineers have a wider and more tolerant outlook on these matters than other professional classes, because in youth they are in immediate working contact with manual workers, as part of their training.

In this address I may appear to have rambled in a somewhat disconnected manner over a good deal of ground, and to have thrust more advice than information upon you. But let me say this, that if I had been told and had understood many of the points I have mentioned to you, I should have been saved some trouble; and I may add that I have seen many failures to achieve success by young men of good abilities, because they have not understood the necessity of some of the things I have mentioned.

DISCUSSION ON

"AUTOMATIC PROTECTIVE DEVICES FOR ALTERNATING-CURRENT SYSTEMS."*

LIVERPOOL SUB-CENTRE, AT LIVERPOOL, 15 NOVEMBER, 1920.

Mr. A. E. Malpas : The paper brings home to those of us who are more particularly interested in the subject of automatic protective devices from the user's point of view, the great advances that have been made in late years with this class of gear. In the old days one had to be contented with the simple form of overload device sometimes, but not always, associated with a time-limit mechanism. In the gas power station erected in Madrid in 1903, distributing by means of overhead lines at 15 000 volts and by underground cables at 3 000 volts, the protective instruments were of the Ferrari disc type on large power circuits, and simple long-break fuses for smaller powers. In the new power station on the banks of the River Mersey at Widnes, the split-conductor system of feeders has been adopted, and there is no doubt that the more extensive and costly the network, the more justification there is for elaboration in the arrangement of the switchgear and protective devices. The paper deals exhaustively with certain types of protective devices, but I should like to mention the question of excess voltage due to surges or lightning which are important on long overhead transmission lines and may become important on underground networks. For instance, on the 10 000-kW transmission line operating at 60 000 volts and used for transmitting water power from Bolargne to Madrid, very elaborate precautions were taken to protect against lightning and surges, one of them being the discharge to earth through a water resistance. On one of the Norwegian water-power plants the overhead lines are protected by the electrolytic form of discharger. I mention this question of surges now because recently there has been trouble due to breakdown of transformers at one of our electrolytic plants in the North of England, and a careful consideration of the facts has led me to believe that these breakdowns are due to surges on the network. I shall be glad, therefore, if the author will tell us in his reply whether he has any record of the maximum voltage which can be attained in the course of everyday working on the network of a large industrial city.

Professor E. W. Marchant : The author mentions the protection of alternators. The real reason why alternator protection is so necessary is that, with the very large turbo-alternators which are now in service, the dangers that arise from momentary short-circuits are so enormously greater than they used to be that some protective device becomes almost a necessity. In the old days the amount of power available was comparatively small, but in the turbo-alternator the current with a short-circuit is more than 20 or 25 times the full-load current. The magnetism of the field core of the machine takes some time to die away, with the result that there is relatively a much larger rush of current than there is under steady short-circuit conditions. The protection of alternators, therefore, has become much more important than it used to be. The most interesting thing described by the author is the biased relay, but I think there is one objection to the device. It appears to necessitate special design of the transformers and other apparatus used in connection with it. I refer to the fact that it is necessary in certain cases to have at one end of the transformer a different ratio from that required at the other side of the circuit that is being protected. I think that is disadvantageous, as it prevents effective standardization of the gear. The author also refers to the difficulty that has been found in obtaining balance, particularly on light loads. I should like to ask him whether he has found any difficulties due to the momentary rush of current that is obtained when switching-on transformers. During the switching-on of three-phase transformers there might be a bigger rush in one leg than in the other two, because the rush of current depends upon the state of magnetization of the core, and it seems to me that with all devices of that kind there might be considerable difficulty due to this at the moment of switching-on.

Mr. E. M. Hollingsworth : Many of us are acquainted with the core balance, Merz-Prince and other well-known devices, and the author now offers alternatives to these, but without expressing any opinion as to their relative merits. I understand there is to be a greater "linking up" in the future, of manufacturers and electricity supply undertakings as regards operation, so that manufacturers after installing these devices will periodically obtain reports from the supply undertakings, giving particulars of their experience with them. Referring to the field-suppressing device, I know of an 8 000-kW machine which broke down some 18 months ago, when it took six weeks to carry out the repairs. On this account the engineer responsible decided to adopt balanced protective gear with a field-suppressing switch. After the gear had been installed the machine again failed, and the damage on this occasion was so slight that the machine was in commission again in less than 48 hours. From this example one can appreciate the reference to insurance in the paper, for in this instance the saving effected by having the machine in commission again so quickly was very considerable. Mr. Malpas referred to the split-conductor protection of the E.H.T. feeders, in connection with the United Alkali Company's West Bank power station. During the past two weeks we have carried out experiments on the feeders in question and also on the alternators, and the operation of the protective gear was most satisfactory. An artificial fault can readily and without risk, provided proper care is taken, be put on each feeder by opening one of

* Paper by Mr. A. E. McColl (see vol. 58, p. 525).

the splits, or connecting one phase to earth, and then running up one of the machines; the operation of the protective gear, both in connection with the feeders and machines, can thus be assured. In regard to the periodical testing of the protective gear, many station engineers in the past have been contented with trying the relays only, assuming the devices would operate in the event of a fault occurring. The necessity of carrying out frequent complete tests is being more and more appreciated by engineers, and several of the large undertakings have appointed an engineer for the purpose of supervising the testing of all devices in use on the system. I am surprised to find that a considerable number of station engineers still regard with some suspicion the reliability of protective devices. They admit the necessity for protection, but are concerned as to the possibility of the devices operating, from time to time, without cause. Provided the protective gear is properly installed and adjusted, the risk of trouble in this direction is very remote. I know of several cases where such devices have been condemned, the trouble being really due to carelessness. For example, I have in mind a length of 11 000-volt, three-phase overhead line, protected with balanced-current devices, which frequently came into action without any apparent reason. The length in question had a peculiar attraction for birds, and the cross-arms supporting the wires were so arranged that a bird standing on them could earth any of the phases. In the first instance the protective gear was held responsible, but a slight re-arrangement of the cross-arms eliminated the trouble. The author recommends the earthing of the neutral; this, of course, is necessary if the best results are to be obtained with protective devices, but apart from this I do not think any experienced engineer in charge of a system of considerable size would care to operate with the neutral insulated. The question of earthed versus insulated neutral has long been a matter of discussion, but I understand the supply undertakings in the United States have now decided the question. Whereas a few years ago 90 per cent of these undertakings were operating with an insulated neutral, 90 per cent have adopted the earthed neutral.

Mr. L. Breach: I suggest that all parallel feeders should be dealt with separately, and should be self-contained. I do not agree with the interlocking system, because what are feeders to-day may be distributors to-morrow, in which case, if arranged with interlocked protection, all the protection gear on those cables would have to be re-organized. I have had a great deal of trouble due to instruments being put out of action owing to excessive secondary current on short-circuit, the plungers pulling the cores of operating coils completely out of place. I should like the author to say in his reply whether he agrees with the system being earthed, or not. The question of protection of large generator windings has just arisen in my works, and we have decided to use a three-pole instrument—two poles to protect against phase faults, and the third to protect against leakage, thus allowing a much finer setting on the leakage coil. I should like to know whether, if the leakage protection of feeders were carried out beyond trunk feeders to, say, four substations in series, any discrimination would be possible, otherwise it seems to me that if there were a heavy fault at the far end the switches in the main station would operate.

Mr. A. E. McColl (*in reply*): I quite agree with Mr. Malpas that the larger and more elaborate the system of distribution, the greater the necessity for considering the question of protection. On some of the more involved networks practically the only choice lies between Merz-Price and split-conductor protection. As mentioned in the paper, protective gear is essentially a form of insurance, as individual circuits and apparatus are protected against destruction and the network generally against extensive dislocation of supply. Dislocation of supply represents a loss in revenue and a loss of confidence in the supply authority, the monetary value of which is difficult to assess.

Regarding the question of surges, within the last few months I have had experience of a number of cases where flash-over between phases has taken place over air distances varying from 3 to 6½ inches. This refers to a system which has a normal operating pressure of 11 000 volts. In some cases the flash-over was across transformer terminal studs above oil-level, and in other cases at the terminals of switchgear or across busbars. Certain of these occurrences are distinctly attributable to surges, as they have occurred simultaneously with severe breakdown on other parts of the system. Surges occur on large systems due to normal switching, synchronizing, etc., quite apart from surges which may be caused by faults. As the result of some hundreds of observations taken on spark-gaps over a considerable period, it was quite apparent that surges occurred under normal operating conditions. These rises of potential are quite distinct from the increases obtained above ground due to displacement of the neutral point. To give a figure to the potential value of the surges, I may say that it was a 6 600-volt system with free neutral and the spark-gap settings between phases were equivalent to 4 times the above voltage.

I quite concur with Professor Marchant's views regarding the inadvisability of installing devices which increase the liability to breakdown. Speaking generally, however, I do not think they do. Protective gear sometimes does fail, usually in the direction of operating when it should not operate, although I have known several cases of busbar faults where the incorrect operation of the protecting devices controlling incoming cables was a distinct blessing. On the whole, however, protective gear very rarely fails where it is called upon to operate. The failure to remove faults quickly, lies usually in the switchgear. Switchgear placed in lock-up substations and not operated at frequent intervals has a tendency to settle down hard, and when called upon to open will usually do so sluggishly or sometimes not at all. In any scheme of protection where we introduce current transformers we are adding a link which is liable to failure. The real reason for providing protective gear for alternators is to keep down the cost of the repair consequent on a fault and, in addition, it prevents the faulty alternator involving the rest of the system in difficulties.

Professor Marchant takes exception to the biased system on the score of the special apparatus required

for it. I do not think any real difficulties attach to this. Take, for instance, the arrangement shown in Fig. 16; the beam relay is a standard piece of apparatus and is employed in several other schemes described in the paper. In the scheme shown in Fig. 20, transformers of the same ratio and capacity may be placed at each end of the winding and the necessary bias obtained by inserting one coil of the relay so that it creates the desired unbalance itself. Figs. 22 and 24, however, require that the transformers have different ratios and, as special results are desired, there should be no real objection to this.

Regarding the rush of magnetizing current when a transformer is switched into circuit there are really no difficulties, as the relay may be momentarily short-circuited through light fuses by auxiliary switches operated by the main switches. This admits of the charging current being diverted from the relay for an instant and, if the transformer should actually be faulty, at the moment of switching-in the fuse is blown and the relay operates.

In reply to Mr. Hollingsworth, I can fully bear out what he says with regard to the desirability of fitting protective gear to alternators. One case which came within my experience relates to a 6 000-kW unprotected alternator. Not only were the windings completely destroyed, but the core was raised to such a temperature that the complete body of laminations had to be re-insulated again. This repair was a matter of months and I am not far wrong when I say that with a proper protective equipment the disablement period would have been a question of only a week or two. I know of several cases of protected alternators which have broken down and have been in service well within a fortnight. The points which I desire to emphasize are that a proper protective equipment will reduce the cost of the subsequent repair, and that the loss of a valuable unit to the system may be reduced in many cases to a few days.

Mr. Breach does not agree with the interlocking system for parallel feeders. The ideal arrangement undoubtedly is to have each feeder self-contained as far as the protective gear is concerned. However, in a great many cases we have to accept the lay-out as it stands. At a later date it is decided to fit protective gear, and for a case like this there is little choice left. It may be impracticable and the cost possibly prohibitive to lay pilot wires, and the alternative lies either in coupling each pair of parallel feeders together as one unit or in fitting interlocked balanced relays.

Relays being put out of commission by excessive secondary current can be overcome by causing the transformers to saturate at earlier values of overload, and this can be accomplished by simple and well-known means. By placing leakage protection on feeders which supply a number of substations in series, a measure of protection can be given. To discriminate between near and remote faults requires that properly graded settings be employed, and I would certainly prefer definite-time in preference to inverse-time relays.

Scottish Centre, at Glasgow, 8 February, 1921.

Mr. C. W. Marshall: I am disappointed that the author did not take one definite piece of apparatus and discuss the protection of that type, first as regards sensitiveness and then as regards stability, as this might have enabled us to decide what progress had really been made. In the case of the alternator, the question of sensitiveness, as the author very properly points out, is of vast importance, and I believe that he has gone a long way towards relieving us of some of the worries which are only too apparent at present. The alternator, however, does not give the most extreme case as regards stability, because the maximum short-circuit current that has to be dealt with is very small compared with the short-circuit current in feeders, with the usual system of parallel operation. I think that the author's new induction-type relay should receive careful consideration. The paper by Major Edgcumbe * brought out two sides to the controversy on the use of fuses to get the necessary amount of bias, and the general opinion at that time was that while the fuse was a very far from perfect means of obtaining a time limit, it was the best that could then be obtained. I have now obtained one of the author's relays, and although I have not yet wholly satisfied myself that it will operate satisfactorily, I believe that they will provide a decidedly better solution of the problem than has been given up till now. If that proves to be the case, I have no doubt that they will become very much better known in the future.

* *Journal I.E.E.*, 1920, vol. 58, p. 391.

Mr. G. G. Braid: All protective relays should be simple and certain in their action. The trouble with balanced relays seems to be that the transformers operating them do not maintain their balance when excessive loads and heavy surges occur. It seems to me that the introduction of coreless transformers would solve this difficulty of want of balance at the extreme loads. Of course, the relays might then have to be made much more sensitive. The author's proposals in regard to biased relays go far towards solving the question of certainty of action. The question of instantaneous action as against delayed action for relays has caused considerable discussion. I know the author favours the former. On the other hand, delayed action means breaking the circuit under much less severe conditions, and if properly arranged, say with a delay of 4 to 5 seconds, there should not be any great drop in the supply voltage. Protection, however, is not only a matter of relays. The switches must be capable of interrupting the rush of current to the fault and, as there is a limit to the size of the switches, reactances or choke coils should be installed at various points. These will choke back the short-circuit current due to the fault, and thus prevent a collapse of the voltage on the rest of the system. Not only should these reactances be inserted in the main busbar, between sections fed by different generators, but they should be inserted on all the main outgoing feeders, and especially in interconnecting cables between two power stations. As regards generator

protection, the author's devices seem to be a distinct advance. Modern alternators are usually built with a considerable amount of reactance, 15 per cent or more. It is a question, however, whether this is an advantage. The idea no doubt is to safeguard the alternator and to prevent very heavy rushes of current to other alternators or to faults outside. The voltage at the terminals of the alternator, however, is apt to collapse too readily, and on the other hand, as voltage regulators are now installed in most stations, this to a great extent counteracts the effect of the increased reactance. It seems to me that the insertion of reactances between the different generators, combined with a moderate amount of reactance, say 10 per cent, would form sufficient protection.

Mr. A. P. Robertson : The question of instantaneous relays is very debatable. While it is true that the fault should be cleared at the earliest opportunity, this throws a great deal of strain on the switch, and with the increased power behind switchgear nowadays, and the fact that the switches in use are not all of the most modern type, something must be done to limit the current to an amount which the switch will break. To retard the action of the relay will effect this to some extent. The insertion of fuses in parallel with the relay has been effective, but they are not quite reliable. As Mr. Marshall says, the time is indefinite, although a time-lag is certainly afforded. The makers of switchgear have certainly done a great deal to give us better switches, especially in the direction of fast operation. I think it is almost imperative that we should have spring-loaded switches. The initial action in the case of a switch which is not spring-loaded is not fast enough, and it may fuse the contacts together just at the moment when they should move fastest, so that the switch will not open and the fault will not be cleared. The protection of generators is certainly a point which must be looked into very carefully. With the large generators which are used nowadays, and the consequent smaller number of units available, the loss of one machine is very serious, and there is a great strain on the remaining plant to keep up the supply. At the same time, continuity of supply must be taken into consideration. The protection of feeders and of the apparatus at the end of feeders has also to be taken into account, and this is where discriminative protective gear is useful.

Mr. E. Seddon : Now that the author has carefully investigated all the known forms of protection, I hope that he will tell us in his reply what form of protection he would recommend for (1) generator protection, (2) parallel trunk feeders, and in case of one of a pair breaking down how the remaining one will be protected, (3) interconnectors where power will flow both ways, and (4) independent feeders. I think that for general use the split-conductor system is the best form of protection yet devised, but I admit that the expense of such special cables and more complicated switchgear warrants our further consideration of other methods. The electrical supply industry always appears to be in a transition stage. When the Merz-Price gear was first introduced it was thought that we had reached a point with protective gear at which further improvement was unlikely, but extended experience showed that there were difficulties to be overcome which could not have been foreseen until the apparatus had been thoroughly tried. I should therefore like to ask the author whether his very ingenious biased relays have been thoroughly tried out in practice on fault conditions, and with what result. In regard to trunk feeders, it occurs to me that we shall have to consider the advisability of putting two switches in series at each end of a feeder. One switch, which takes the final break, will deal principally with leakage to earth, in which case the current would be limited to an amount determined by the star-point resistance. The second switch should operate on short-circuits between phases, and would be fitted with a reactance placed permanently across its terminals to limit the flow of current into the fault when the switch opens, the final break being taken by the first switch. With such an arrangement I think there would be a tendency to standardize switchgear. In the case of faults on generators, whatever system of protection is adopted, I think we are all agreed that whenever a fault occurs the sooner the field flux disappears the better. The modern practice is to interrupt the field circuit of the exciter, but I would go further than this and cause a heavy copper connection to be placed across the exciter terminals so that the flux will collapse at a greater speed.

Mr. J. McDonald : What the power engineer really requires is a reasonable kind of protection for his plant and apparatus which will be at the same time simple and not too complicated, and which will clear a fault quickly, because protection can be carried too far, in which case it becomes a menace to the system. The author has, by means of his biased relay system, added considerably to the stability of protective arrangements, as we at present know them. Some of them are too sensitive, while others are not sensitive enough on certain conditions of fault. The author's system gives a definite bias, so that stability is improved.

Mr. O. C. Thompson : The author says that balanced systems become unstable with excessive loads, and Mr. Braid suggested the use of coreless series transformers to overcome the difficulty. I find that transformers which are almost coreless give far better balance on excessive loads than those in which the magnetic circuit is almost complete. I should also like to point out that in the circulating-current system a short-circuit in the pilot wires would prevent the relays from operating, and a fault on the relay cable is not unknown.

Mr. J. Henderson : The author has certainly paid great attention to stability under severe conditions. If continuity of supply is to be maintained with sound plant, it is of much greater importance than light setting. It is easy to get protective gear to operate with almost any fault conditions, but it is anything but easy to make it remain inoperative under severe stress. In the case of parallel feeder protection, space will often enter into the problem. In a large supply system where the demand has grown rapidly, space for extra switchgear becomes an urgent necessity,

and the obvious solution of the difficulty is found in the split-conductor method of paired feeders. There is no reason why good balance should not be obtained with paired cables, provided reasonable care is taken. Perhaps the author could suggest some method of correcting out-of-balance on the split-conductor system without introducing an extra length of cable. I certainly agree with him that a 30 per cent setting is too high for a large generator, but, in laying out the connections for a new machine, the Merz-Beard system would offer the greatest advantages, and it is certainly better and simpler than anything the author has suggested. For large transformers, however, the circulating-current system could be much improved upon, and some of the schemes suggested in the paper will no doubt find a wider application in the future. More attention has been paid in the past to protective schemes than to switchgear, yet it is on the switch that the whole operation of clearing a fault depends. Many switches are incapable of breaking the power flowing into a serious fault, and very little is known of what even modern switchgear will do. It is useless to adopt elaborate schemes of protection if one has not absolute confidence in the switchgear. I am in favour of clearing a fault as quickly as possible. Even with a short time-lag, the switch must be capable of breaking the maximum instantaneous short-circuit current, since it may have to do this any time in the case of a recurring short-circuit. Such a case occurred not long ago, with the result that the switch failed badly.

Mr. A. E. McColl (*in reply*): The short-circuit current of an alternator does not, as Mr. Marshall states, represent the most extreme condition as regards stability. In a large modern station the individual feeder has a rating which may be, perhaps, from 20 to 30 per cent of the output of individual generating units. Under short-circuit conditions the feeder may require to carry the short-circuit current of the station, and, naturally, the stability of the cable protective gear is severely taxed. However, even with the limited short-circuit current from individual alternators, generating and transforming arrangements are being adopted now which make for unbalancing in the protective-gear circuits. I refer particularly to the large alternator and its associated step-up transformer bank which are usually protected as one unit. If this combination involve delta-star transformers and the protective equipment comprises circulating-current protection, a corrective connection must be made in the protective-gear circuits. Under conditions of three-phase load this corrective connection provides equipotential points for the relays. With large through single-phase forward currents to the neutral of the transformer bank, or through short-circuit between two phases, a shift in the equipotential points of the protective-gear circuits takes place. The extent of this shift depends on the resistance of the interconnecting pilot wires between the protective gear transformers and, in addition, the normal induction at which the transformers are worked. In a modern equipment under the most severe conditions the unbalancing may be quite considerable, and this form of unbalance is quite distinct from unbalancing due to faulty choice of equipotential points.

Mr. Braid suggests coreless transformers as a possible solution of the unbalancing problem. I am afraid this would not be possible with the present size of circuits and reasonable dimensions for the transformers. The relays certainly would require to have a sensitiveness to which we have not hitherto been accustomed. Mr. Braid advocates delayed action of the switchgear on the occurrence of a fault, so that the short-circuit current may have time to fall to reasonable dimensions. I think that this is really a retrograde step. It is assumed that all faults start off from the disastrous stage. This, however, is not the case. For every fault which has disastrous effects on the system there are probably about six which are disconnected by any modern form of protective gear, with little shock to the system. We know from experience that the great mass of cable breakdowns develop originally as faults to earth. If the neutral point is earthed and instantaneous settings are given to the protective gear, roughly 80 per cent of the breakdowns will be disconnected as leakage to earth before the disastrous stage, i.e. short-circuit between phases, has been reached. Within the time-settings Mr. Braid proposes, and depending on the value of the current passed by the neutral point resistance, the leakage may develop to the short-circuit stage, with the result that we are actually converting into severe short-circuits faults which would have been disconnected as simple leaks to earth. Another question in connection with the proposal to delay the operation of switches is the welding together of switch contacts by the heavy current which flows for this appreciable time. I have experienced this on two occasions recently, and it is quite apparent that on a large system we may have welding together of the contacts on a number of switches through which the fault current has to pass. Further, we may unsweat a considerable proportion of the joints in the cables which have to carry the short-circuit current. Within the time which most modern switchgear takes to operate, the short-circuit current under the most severe conditions has considerably fallen in value. From a number of tests which the author has recently taken, the initial lost time on several different types of switchgear averages about 0·15 second before the contacts actually move apart. This time includes movement of instantaneous relay, growth of current in trip coil, movement of tripping plunger, and preliminary movement of switch mechanism previous to parting of switch contacts. Undoubtedly, the protection is not merely a matter of relays. The limitation of the maximum current-flow to within the capacity of switchgear has also to be considered. All large power systems are confronted with the problem of adapting existing smaller switchgear to the needs of a growing system. The installation of reactance appears to be a solution, although there is singularly little experience to enable one to say definitely that it actually is so. It is an open question whether a switch will break a large current more satisfactorily at a power factor of 0·3 than three times the current at or near unity power factor. Undoubtedly, the reactance will permit of the voltage being maintained on the rest of the system.

I agree with Mr. Robertson that spring loading may

give a further lease of life to older switchgear on large systems.

In reply to Mr. Seddon, I should recommend for alternator protection any of the arrangements shown in the paper, particularly those shown in Figs. 15, 16, 22 and 24. For large alternators I should recommend the combination of differential protection and reverse current relays or, alternatively, the combined systems shown in Figs. 22 and 24. For the protection of two parallel feeders, we can provide interlocked balanced relays which automatically transfer to overload protection for the remaining feeder in the event of one of the pair being disconnected. Alternatively, interlocked balanced leakage relays can be provided with incorporated overload fuse protection for dealing with faults between phases, the remaining feeder of a pair automatically transferring to combined leakage and fuse overload protection. For interconnectors, the most suitable protective systems are the split-conductor and Merz-Price gear in its various forms. For single feeders, the combination of leakage and overload relays is ordinarily sufficient. In a large station I think it will prove rather difficult to find the additional space required for the duplicate switching arrangements which Mr. Seddon suggests. The modern practice of interrupting the exciter field circuit does not give the best results, and the tendency now is to break direct on the generator field circuit. To further reduce this time interval, I would recommend that the operation of the field circuit-breaker be not dependent on the oil-break switch. In this connection I would point out that an alternator may break down while excited but not actually on the bars, and, in this case, protection is obtained only if the field interruption is independent of the oil-break switch.

In reply to Mr. Thompson, I may say that the circulating-current system would be rendered inoperative with a short-circuit in the pilot cores. This, however, holds good only if the short-circuit is not more than 25 per cent of the cable length from either end. If it is beyond this it does not affect the relay at all and, moreover, only the relay towards which the fault point is located is rendered ineffective.

Mr. Henderson remarks that there is no reason why good balance should not be obtained with paired cables, provided reasonable care is taken. Unfortunately, however, despite the care the unbalance is often evident and I am sure Mr. Henderson could give us instances from his own experience. It seems to me that to introduce an additional length of cable in order to correct for balance is an expensive method and better results could be obtained from biased relays, particularly the arrangement shown in Fig. 14, which is intended to deal with a case such as this.

DISCUSSION ON "SOME ECONOMIC ASPECTS OF EXTRA-HIGH-TENSION DISTRIBUTION BY UNDERGROUND CABLE." *

North-Western Centre, at Manchester, 11 January, 1921.

Mr. S. J. Watson: The more the problems of distribution and transmission are discussed, the better it will be for the industry as a whole. Any economy which may be effected in the cost of constructing a system does, in the end, have a very important bearing on the charges which have to be made for the supply. In his opening remarks the author suggests that in the past the cost of distribution systems has been comparatively small. I must differ from him on that point because in the majority of undertakings the cost of low-tension networks already in use is very nearly half the total capital employed. If, in the past, high-pressure transmission has been used to a larger extent, generally speaking the cost of distribution and transmission would have been appreciably lower than it stands to-day. The author rather confuses transmission with distribution. There is really a very clear line of demarcation between the two. The author appears to take transmission to mean a straight-through cable from point to point with a distribution centre at the end of it or situated at a certain point on its length. I cannot agree with the author's figure of £4 per kilovolt-ampere of peak load. It seems to me too low, especially in view of the fact that he includes in that cost the switchgear and transformers used for distribution purposes at the different points. At the present prices of cables and labour, the cost must approximate to a somewhat higher figure. Then, again, he deals with the distribution of 40 000 kW over only 5 square miles, which represents an exceedingly brisk demand in a very small area. In the majority of cases to-day, in order to take up 40 000 kW it would be necessary to transmit or distribute, as the case may be, over an area a good deal larger than 5 square miles, which only represents a maximum distance from the generating station of about 1¼ miles. There, again, there is a difference, I think, between distribution and transmission. It is in that case really distribution; but when a load of 40 000 kW has to be supplied it requires something more than what is usually called a distribution system. Some three or four years ago I had to get out figures for the transmission of some 750 000 kW over an area of 400 square miles, the maximum distance from the power station being about 10 miles. I found that, using a pressure of 11 000 volts, the capital cost with underground cables worked out at about £9 per kilowatt transmitted. With a load factor of about 30 per cent, the cost per unit varied from 0·012d. when supplying near the power station to 0·108d. at a distance of 10 miles, compared with the figures of 0·05d. to 0·22d. per unit given by the author. Incidentally I may mention that if a charge were made to cover these costs, in the form of so much per annum, it would vary from about 2s. 6d. to 22s. 6d. per kilowatt demanded, according to the distance from the power station. On page 95 the author says: "On the other hand, overhead line work is not as cheap as cables when small quantities of power have to be distributed to isolated points of a complicated network." I do not agree, because in an E.H.T. line, whether it is for 6 600 or 11 000 volts, there is a certain minimum size of conductor, and it seems to me the overhead line will be considerably less costly than an underground cable. There may be some misunderstanding on that point which perhaps the author will clear up in his reply. On page 98 there is a diagram giving the cost of transmission in bulk at 6 600, 11 000, and 30 000 volts. I was particularly interested in a paper by Mr. S. L. Pearce some three or four years ago dealing with the transmission of definite blocks of power over a certain fixed distance, which stated that at a distance of about four miles the 30 000-volt system was as cheap as the 6 600-volt system. There is another remark on the same page with which I am afraid many supply engineers will not altogether agree: "In the author's opinion it is very doubtful indeed if engineers will decide in future schemes that the advantages of cable protective gear are great enough to justify its cost." I do not know what the author would have us infer from that remark. Does he really mean that in large cable systems no protective gear of any kind should be provided, or does he suggest (which I expect is the case) that many people are inclined to install too much protective gear? If the latter idea is what he intends to convey I am in agreement with him, but if he suggests that cable protective gear should not be provided on these large schemes I do not think he will receive a very large measure of support. On page 99 the herring-bone type of lay-out is referred to. Much can be said for this, particularly in the early development of an undertaking; in fact I do not think any other system can be developed. The cost of point-to-point transmission with no tappings on the way would be prohibitive. It seems to me that in the future the herring-bone system will become a secondary network and will be connected up to the larger and more important main transmission lines. The ring-main system, I frankly admit, is ideal, but it is not every district which is so constructed geographically as to justify such a system. In some cases I think it is better to construct a duplicate main system, because, although the cables are duplicated, there is only one cost for excavation and one for reinstatement, which under the present-day conditions amounts to a considerable percentage of the total cost. On page 100 (sub-section 6) the author says: "On a ring main the cross-section of copper should be reduced on the feeders furthest from the power house." It seems to me that

* Paper by Mr. R. O. Kapp (see page 94

on a ring main it is very desirable that the cross-section of the copper should be the same between the various interconnecting points, because under certain conditions they may be called upon to transmit the same power as one of the main feeders going out to the ring main from the generating station. Again, in sub-section (8) on the same page, the author suggests that the largest cable used on a 6 600-volt system should be 0·075 sq. in. I fail to understand why he recommends so small a cable in view of the fact that he has considered throughout his paper the cost of transmitting 40 000 kW. A 0·075-sq. in. cable is far too small.

Mr. H. A. Ratcliff : I was surprised at the author's statements with reference to the centre of gravity of the load ; I think the distinction made is of little or no importance. Perhaps, however, it may apply in some of his hypothetical towns which have length without breadth, but in an ordinary town with a reasonably symmetrical lay-out of feeders I do not think one need worry very much about the academic distinction between the centre of gravity of the load and the centre which gives the cheapest lay-out on a heating basis. Such cases as I have considered do not appear to show a very marked difference. The author admits that he has not succeeded in finding an analytical method of determining the most economical site for a power house. I think he has made the mistake of trying to work out his schemes with mathematical precision when so many of his factors are more or less indefinite, and consequently such precision is not justified ; moreover, he has omitted a very important factor. Most distribution systems of any importance to-day are the result of more or less gradual growth, and I think it must be admitted that in very many, if not all, cases the dominating factor governing their development has been expediency. Apparently that factor has not entered into the author's calculations. The alternative to the centre of gravity, which is based upon heating, apparently ignores losses entirely. In view of the fact that the author has only considered heating, I should like to know whether he has allowed for different permissible limits of heating on different pressure cables. For example, would the temperature limit be the same for 33 000-volt as for 6 600-volt cables ? The author's arrangement of transformer taps is very ingenious, and possesses the great merit that it gives a large range of pressures with a comparatively small number of taps. Generally I am in agreement with the author on the subject of economic voltages. When making somewhat similar calculations I have been surprised to find at what a short distance it pays to change over to a higher pressure. There is also another aspect of the matter which I think will become important in the future. In large cities, as the load becomes more dense the cost of the necessary additional cables is very heavy, due not so much to the cost of the cables themselves as to the cost of laying them. It may therefore be desirable to consider the question of increasing the pressure on existing feeders. Well-made cables designed for 6 600 volts would undoubtedly be quite safe for a very much higher pressure. The question will, therefore, depend upon the economic aspects of the necessary transformation. With a primary transmission at, say, 33 000 volts there would necessarily be transformation steps and, therefore, it is quite possible that another step would be less expensive than additional feeders. In many cases an additional transformation step would not be necessary. I am pleased to see that the author has so much confidence in the reliability of cables. His remarks on protective gear are also distinctly reassuring. Some of the recent papers on the subject have been rather disquieting. With reference to the various types of feeder lay-out, the word " cobweb " used in the paper provides a very expressive description of an effective, but otherwise rather complicated, system. I consider that the simple herring-bone system is a very bad one, but a system which is practically the same, in which the main cable is looped into and out of substations, is quite legitimate and comparatively easy to control. Ring mains have served a very useful purpose indeed in scattered districts, but I consider that they are rather overrated for cities or dense areas. The distribution systems of the future will probably consist, as the author suggests, of some combinations of the star and link systems for the primary transmission, with a secondary star and link system, and then perhaps some sort of simple radiating system for the final distribution to consumers. It is not to be expected that every consumer on a high-pressure system can have a duplicate supply, any more than he can on an ordinary low-pressure system, but all the important portions of the primary transmission will certainly have to be provided with alternative connections for use in the event of breakdown. In cases where feeders are paralleled through link mains as shown in Fig. 9, there is another alternative to the use of boosters and induction regulators, as it is frequently possible to control the distribution of the load between several feeders, either by sectionalizing the busbars or by the use of duplicate bars. With such an arrangement almost any desired distribution of load may be obtained, provided that there is a sufficient range of transforming or converting plant available. In the event of breakdowns the busbar sections would, of course, have to be linked together. I agree with the author that main feeders should not be tapped, but I am sure that most engineers will question the desirability of reducing the cross-section of any portion of a ring main. I should like to have further information in regard to Fig. 8, particularly as to whether the costs are based on capital charges only. Presumably the question of load factor has not been taken into consideration, for apparently the author has ignored losses and has based his limiting conditions on temperature-rise only, but I cannot see how he arrives at the values given. One result of this curve is that it leads to the selection of 0·075 sq. in. as the maximum size for a 6 600-volt cable. In Manchester there are miles of 0·25-sq. in. and 0·3-sq. in. 6 600-volt cables in use, and 0·3-sq. in. cables are being laid for the 33 000-volt transmission system. The heat curves are very interesting, and appear to be quite sound, but nothing is stated as to the data on which they are based as regards permissible temperature or current density, or whether they are equally applicable to both 6 600- and 33 000-volt cables. The maximum permissible temperature is not necessarily the same in the case of both cables, nor are the limiting values governed by the same considerations. I can hardly

agree that the manipulation of these curves is a suitable occupation for a switchboard attendant, as he has far more important duties to perform, but some form of graphical estimation may ultimately become one of the duties of the operator upon a large system who, for want of a better term, I will call the "load controller."

Mr. J. B. Palmer: On page 97 the author says: "Voltage drop on the E.H.T. system is always so small that it need not govern the lay-out as it does in the case of L.T. distribution systems. In the case of E.H.T. cables, heating and not voltage-drop is always the determining factor." This is probably true if one is considering distribution in a dense area, but I think the statement would be misleading if it were applied to all conditions of distribution and transmission. I can imagine the case of a power station supplying not only a town, but a group of towns; if one of these towns happens to be situated at a distance from the power station approaching the economical limit for the particular transmission voltage, the voltage drop might possibly be considerable. If the power factor were, say, 0·7, and step-up and step-down transformers with 6 per cent reactance were installed, the voltage drop might be very embarrassing indeed. It is true that in a case of this sort a considerable proportion of the drop will occur in the transformers and not in the cable, but the effect is the same, as for all practical purposes the transformers are part of the cable. I do not think the author's statement would apply at all to long-distance transmission with correspondingly high voltages; in fact, the voltage drop would limit the amount of power the line would carry economically long before the conductors were carrying their full load considered from the point of view of heating. I am not clear what is included in the figure for cost per kilovolt-ampere in Fig 6. The author has assumed the cables to be laid under average circumstances for heat dissipation, and I think it would be interesting if he would tell us what he considers to be average conditions. For instance, one hears widely differing opinions as to how far apart 33 000-volt cables should be laid. If it is permissible to lay them at such a distance apart that two cables can be put into one trench, the cost of laying is obviously much less than if it were necessary to excavate for each individual cable. The nature of the ground also affects the cost of laying; I believe it is about three times as expensive to lay cables under granite setts as it is under macadam roads. I notice that at 30 000 volts the cost per kilovolt-ampere is about £7 for zero distance, but this figure will vary very much with the size of transformers used and, while the curve may be correct for a certain size of feeder, I do not think it is correct for any wide range of sizes. To take a specific example, I should like to know how the author arrives at the value of £12·5 per kilovolt-ampere for 30 000-volt transmission over 8 miles. I join with other speakers in querying the author's statement that it is doubtful if engineers will decide in future schemes that the advantages of cable protective gear are great enough to justify its cost. I do not see what alternative there is; it seems to me that it is essential that a cable fault should be cut out, and in such a manner that only the faulty section is isolated, otherwise it would be necessary to lay a greater number of cables to ensure an uninterrupted supply. Protective apparatus and pilot cables are costly enough, it is true, but they are less costly than a multiplicity of stand-by cables. The whole subject is one which cannot be studied too much; an engineer in designing a distribution or transmission system (and they are really the same thing) should be sure that he is on the right lines. Cable once in the ground represents capital in its most inaccessible form, and alterations to route or capacity are not to be contemplated, and although mistakes are buried they are by no means finished with.

Mr. L. Romero (*communicated*): The author considers only capital costs, and disregards losses altogether. He attempts to defend this attitude on page 97 by stating that, for the purpose of the rough generalizations dealt with, feeder losses are too small to need considering. This is a most misleading statement, and I have worked out the I^2R losses in a 0·2-sq. in., 3-core armoured cable laid direct and loaded at 250 amperes per core, which is well below the temperature limit of the cable. At this load the losses per mile at the working temperature amount to about 47 units per hour. With a load curve giving losses equal to 25 per cent of the losses at 100 per cent load factor the annual value of these losses at 0·9d. per unit would be £386. The capital charges per mile per annum on a 6 600-volt cable of this size laid and jointed are about £270, or actually much less than the value of the losses. I have chosen these figures as fair average figures, but they could obviously be modified widely without making the value of the losses in the least negligible as compared with the capital charges on the cable. I recently worked out the total annual cost of transmission at 6 600 volts to a very large cotton mill. In this case one 0·2-sq. in. cable was of sufficient capacity to transmit the load, the estimated load being about 240 amperes, and I found that the total annual cost of transmission for two 0·2-sq. in. feeders in parallel (which would give a complete stand-by) was only very little more than for the scheme with one feeder, the saving in I^2R losses almost balancing the capital charges on the second feeder. Reduction of losses should never be lost sight of when stand-by feeders are under consideration. In a properly designed system stand-by feeders will generally go a considerable way towards paying their own capital charges by reducing I^2R losses, and in many cases (at least at pressures up to 6 600 volts) they will effect a net overall economy as well as providing a stand-by. The points I wish to emphasize are: (1) That at 6 600 volts the most economical loading is often well below the temperature limit; and (2) That it is unwise and unsafe to run very near the temperature limit, and that the extra cost of running well below it is not very great. In comparing 33 000-volt and 6 600-volt transmission by capital cost alone, the author is doing an injustice to the 33 000-volt transmission, as for the same current density the losses at 33 000 volts are only one-fifth of the losses at 6 600 volts, whereas the capital cost is one-third. The author's temperature-rise curves are very ingenious, but I think that most engineers will prefer to have sufficient copper to make it unnecessary for them to watch their cables so very closely. Any results obtained from these curves on normal days in winter might be completely upset by a period of black

fog when the peak load would be nearly the all-day load, and the engineer who had designed his feeder system so that the maximum peak load on any cable was no higher than the safe continuous load would be justified. On page 94 the author states that the capital charges on the E.H.T. distribution system of a super-power scheme may exceed the coal bill. This seems to me to be almost impossible with a well-designed scheme loaded to nearly its full capacity. In the next paragraph he states that if a power house with a peak load of 40 000 kVA is at the centre of a supply area extending over 5 square miles, the capital cost of distribution at 6 600 volts will be about £4 per kilovolt-ampere of peak load. Should not this be "5 miles square," or 25 square miles, as the capital cost of distribution given appears to be extremely high for such a concentrated area as 5 square miles?

[The author's reply to this discussion will be found on page 256.]

South Midland Centre, at Birmingham, 12 January, 1921.

Major A. M. Taylor: Fig. 6 does not clearly state what is included in the prices given. Presumably the prices given for the 30 000-volt cables include the costs of step-up and step-down transformers, and the necessary switchgear on the primary side of the step-up and the secondary side of the step-down transformer. The curves for 6 600-volt and 33 000-volt cables intersect one another at a point approximately five miles from the generating station. Presumably, therefore, it is intended to convey that at this point the cost of the two methods of transmission is equal. The curves, however, do not account for the very common case where, on a 5 000-volt system, it would be very desirable to step-up the pressure to 10 000 volts by means of transformers and switchgear. It may therefore be of interest to give an actual case that has been worked out for Birmingham. Consider a substation whose present demand is 6 000 kVA and which is fed by three 0·15-sq. in. trunk feeders, working at 1 500 amperes per sq. in. and on which trunks it is desired to double the pressure on account of voltage drop. The question to be considered, therefore, is whether it is best to put in three additional cables or to introduce step-up and step-down transformers and transmit the whole load over the three existing cables. On the one side of the balance sheet we have interest and sinking fund on the whole six cables, plus I^2R losses in the cables, plus interest and sinking fund on new switchgear at both ends for the three new cables. On the other side we have interest and sinking fund on the transformers and on the existing three cables, plus iron and copper losses in transformers, plus copper losses in the cables. The cost of all lost energy is taken at 1d. per unit as, with the exception of the iron losses on the transformers, it is of the nature of a peak-load loss. It is also assumed that no additional switchgear is required with the transformer scheme, the switching being done on the primary of the step-up transformers and on the secondary of the step-down transformers. The results of this investigation were that on a two-mile transmission there was an annual loss of £0·16 per kilovolt-ampere. On a three-mile transmission there was a gain of £0·16 per kilovolt-ampere per annum, and on a five-mile transmission there was a gain of £0·80 per kilovolt-ampere per annum. Some of our existing trunks are as long as this, and the gain in such a case would be of the order of £30 000 per annum on the total quantity of power we are considering transmitting or, in the case of the three-mile transmission, £6 000 per annum. The gain in flexibility is of course very great, and the equivalent of induction regulators can be obtained by suitable tappings taken off the step-up and step-down transformers. In the above estimate it should be mentioned that an amount representing 33 per cent of the cable losses has been deducted in the case of the transformer transmission, as the introduction of the transformers immediately reduces the current density in the existing cables to one-half, and it will probably be five years before they are again fully loaded up, whereas, in the case of the cable scheme, the additional feeders would probably be laid one by one. An interesting feature develops when a transformer transmission is run in parallel with a direct cable transmission. It is anticipated that the transformer will cater for the unity power-factor component of the load, leaving the cable to deal with the lagging component. This will have the advantage that the transformers will be operating under the best conditions of regulation, even though the load itself may have a poor power factor. This should prove quite a valuable feature, as at the higher voltage the drop in the feeder itself is less serious than the drop in the transformer. I should be glad to know if the author has had occasion to consider such a case as the above, and whether Fig. 6 could not be so modified as to show the advantage of stepping-up from 5 000 volts to 10 000 volts in the manner just described. There are probably many large towns where, owing to the generating station being situated some distance away from the centre of the supply area, the feeder drops are quite considerable on the longer feeders, and where a step-up to 10 000 volts would be very much appreciated.

Mr. W. Brew: Useful guiding principles have been clearly demonstrated by the author, who admits that the indefinite conditions arising in each case in practice must invariably be specially treated. Figs. 1 and 2 assume that the total cost of cable per unit of length is proportional to the sectional area of the copper. This assumption will only be approximately correct in the case of the largest cables, and takes no account of the cost of trenching and reinstatement. When we remember that, for medium and small E.H.T. cables laid singly, the cost of trenching and reinstatement may be nearly double that of the cable itself, it will be evident that the form of the author's diagrams from the centre outwards must be modified to approximate to practical conditions under the loading assumed. The author's treatment of the problem indicated by Figs. 3 and 4

is interesting as an ideal arrangement; the position of the transformer houses required under such super-schemes as he discusses will generally be the sites of small power stations shut down under the larger scheme. This will enable existing cable networks, buildings, etc., to be utilized to a considerable extent. The curves relating to the heating of buried cables are of special interest; I do not think that we can hope to get in practice the logarithmic curves the author assumes. The necessary condition for a logarithmic heating curve is that the rate at which heat is being generated in the cable (which must be constant for each portion of the curve drawn) is equal to the rate at which heat is being stored, plus the rate at which heat is being dissipated during the same interval. The rate of the heating in the present case is proportional to the I^2R loss in the cable, where I represents load current, and R the copper resistance. The value of R may vary by 20 per cent or more, depending upon the temperature of the cable, and the rate of heating corresponding to any value of I will vary accordingly. A further discrepancy may arise due to the charging current on long cables causing the outgoing end of a feeder to attain a somewhat higher temperature than that at the far end. Other disturbances to be expected are temperature-rise due to dielectric loss, and the loss due to eddy currents in the lead sheath at times of maximum load. In spite of these disturbances it is conceivable that a well-arranged "load dispatch" system between a number of interconnected power stations might effect considerable economies in the direction suggested by the author.

Mr. H. W. Blades: Two of the most important factors in settling the site of a power house are a plentiful supply of water and adequate railway facilities, but the mechanical method suggested by Mr. Carrothers might be useful in solving the most economical position of the transformer house. On a ring-main system the cross-sectional area of copper should not be varied as suggested by the author since, although theoretically this is correct, in practice it is highly undesirable. Has the cost of excavation been included in the cost per kilovolt-ampere per mile? I do not agree with the results shown in Fig. 8, and I consider the figure of 0·075 sq. in. to be too small as a standard size for a ring main on a 6 000-volt network. I am of the opinion that a research should be carried out to investigate the heating effects of cables drawn into conduits. It is well known that E.H.T. cables are much better laid direct in the ground, and are capable of carrying anything up to 25 per cent more load than a similar cable drawn into a duct, but it is not always possible in large cities to lay all cables direct, due to the high cost of excavation, and the necessity of frequently opening the same ground. Consequently, a conduit system frequently has to be adopted in congested areas. A considerable amount of trouble has been experienced in Birmingham due to the expansion and contraction of the lead-covered cables, caused by heating and cooling effects. The repeated expansion and contraction resulted in ripples forming in the lead near the joints, these ripples later developing into fractures. In other cases the fracture developed at the plumb of the lead sleeve. In order to overcome these difficulties the lead sleeve-joints were mounted on special cradles or carriers, the cradles themselves being provided with steel balls as feet, and allowed to move on runner plates. The longitudinal movement of the cable was changed into lateral movement of the joint by suitable bends or sets in the cable, with the exception of some pits where, owing to the special construction of the pit, it was not possible to form the correct sets or bends in the cable. The trouble due to cracks in the lead has now practically disappeared. The 3-core, 0·15-sq. in. cables are loaded up to a current density of 1 500 amperes per sq. in., and the 6-core, 0·185-sq. in. cables are loaded up to a current density of 1 000 amperes per sq. in. The normal maximum daily movement or travel of a 6-core, 0·185-sq. in. joint on its cradle is approximately 4 inches, and this takes place on a conduit system where the joint pits are 120 yards apart, so that one of these joints moves through a distance of 30 yards in the course of 12 months. The ordinary expansion joint was not found to be suitable, as it allows for movement of the lead at one end only, but by mounting the expansion joint-box itself on a special cradle the difficulty was overcome in one case where the cable passed in a direct line from bellmouth to bellmouth.

Mr. W. Wilson: The economy of using 0·075-sq. in. cable for 6 600-volt distribution is well shown in Fig. 8, and this appealed to me especially. The author states that a pressure of 30 000 volts, or the maximum the cable insulation will reliably withstand, should be used for all long-distance distribution. This means that, as soon as it is decided to use transformers—I presume the distance of the 30 000-volt curve in Fig. 6 from the origin at the supply point represents the cost of the transforming apparatus—then the maximum voltage is to be employed, whatever the distance. I should like to ask the author whether he really intends this to be done. My own experience has been entirely with overhead transmission, but with this there is a well-defined relation existing between the length of transmission and the voltage, roughly about 1 000 volts per mile. I should be very glad if the author would comment on this apparent anomaly. His condemnation of protective schemes seems rather strange. On the one hand we hear of great activity in a number of directions in developing and improving the art of protection; on the other hand, one would have thought such a safeguard of especial value in the days to come, when electric supply will be on a larger scale, more widely distributed, and used for a greater variety of purposes. Will the author explain why this form of insurance has fallen so low in his estimation? I was particularly interested in the method of estimating the heating effect upon a cable, by the use of logarithmic curves, as shown in Figs. 10 to 16, and I appreciate the soundness of substituting this methodical treatment of the subject for the guesswork that at one time prevailed. I have used a somewhat similar graphical construction to arrive at the ultimate capacity of liquid rheostats, and I found it very effective. It was easy, in this case, to obtain the cooling curve for each apparatus, at first by direct experiment, and then by a simple calculation for the coefficient of the curve, deduced from the dimensions of the rheostats.

Mr. W. J. Line : The logarithmic curve and time-constants were used by myself and others in investigating the heating of machines and resistances 16 or 18 years ago, and the method was originally published in the *Elektrotechnische Zeitschrift* in 1900. It has, however, only been applied to a very limited extent, if at all, to the heating of cables, and the author's method is capable of giving valuable results in the near future. This will be the case notwithstanding the fact pointed out by previous speakers, that actual heating curves are not exact logarithmic curves, and that the increase in resistance of the conductor with rising temperature, and other considerations, tend to cause some departure from the true logarithmic curve. The paper brings out in a very interesting manner the difference between the peak capacity in relation to the continuous capacity on apparatus with a short time-constant and that on apparatus with a long time-constant. Apparatus with a long time-constant has a large thermal or heat-storage capacity, as compared with its continuous heat-dissipating capacity. The latter depends on the surface and surroundings, whereas the former depends on the specific heat and mass of the materials of which the apparatus is composed. The various diagrams and particulars in the paper thus show how much greater may be the peak capacity of a large transformer than its continuous capacity, whereas in the case of a small cable this may not be the case. The paper also shows that a large transformer having a time-constant of 10 hours may take practically three days to reach its final steady-temperature cycle. The time-constants shown in the paper for cables and transformers vary from $\frac{1}{2}$ hour to 10 hours. In my investigations of machines I found that the time-constant varied from, say, 15 minutes for a 1-h.p. motor running at, say, 1 500 r.p.m., to several hours for large machines. Small articles such as wire resistances in air may have a time-constant as low as fractions of a minute.

(*Communicated*) : In connection with the discussion I propose to describe two methods of obtaining the time-constant. One method is entirely by experiment, a heating curve being taken at constant load. The other method is partly by experiment and partly by calculation, in which case certain factors of the problem require to be known fairly correctly. To understand these methods one must consider that the time-constant can be defined in various ways. First, it is equal to the time which would be necessary to raise the temperature of the body to its final state, if none of the heat were dissipated from the body to its surroundings. Second, it is equal to the joule capacity of the body divided by the amount of heat dissipated per second.

Method 1 (*Experimentally*).—A steady load (direct current in order to eliminate dielectric losses) of suitable value is applied, and temperatures are taken at intervals of time. A curve is plotted having time as ordinates, and temperature as abscissæ. The temperature finally reaches a maximum steady value, corresponding to the load applied. If a cable is inaccessible for the purpose of taking temperatures by thermometer, the temperature may be obtained by the method of increase of resistance. This requires a return pilot cable or wire to measure the resistance by means of voltmeter and ammeter ; or out-and-return lengths of similar cable may be used, and the current and voltage drop between the ends may be measured at the home end. A well-known property of the logarithmic curve is then applied to the experimental heating curve thus obtained, namely, that if a vertical and a tangent are drawn at any point on the curve to intersect the line of final temperature, the length on this line subtending the angle thus formed is equal to the time-constant. The value of the time-constant may thus be tried at various points on the curve and an average taken. If the vertical and the tangent are drawn at the origin, the tangent of the angle of slope of the curve at the origin will represent the rate of increase of temperature with time ($d\theta/dT$, if θ is the temperature, and T the time), at the origin. If there were no cooling, the course followed by the temperature would be given by a straight line along the tangent, and the tangent will intersect the line of final temperature at a time corresponding to the time-constant, thus following out the definition given above.

Method 2 (*By combined experiment and calculation*).—To apply this method it is necessary to know the mass and specific heat of the conductor being heated, and also to have the same information for any bodies in intimate thermal contact with the conductor, to which heat is readily and quickly transferred. It is also necessary to know the final temperature-rise corresponding to a given steady load. If θ_{max} is such a final temperature-rise in degrees C., and M_1, M_2, M_3, . . ., are the masses in grammes, and S_1, S_2, S_3, . . ., the specific heats, respectively, of the conductor and other bodies in intimate contact with it, then the units of heat stored in the body for a temperature-rise of θ_{max} degrees C. will be θ_{max} $(M_1S_1 + M_2S_2 + M_3S_3 + \ldots)$ calories, and this quantity may be converted to joules (watt-secs.) on multiplying by 4·2. The quantity thus obtained represents the joule capacity for a final temperature-rise of θ_{max} in degrees C. Now at the final temperature-rise the watts input per second are equal to the watts dissipated per second. These quantities are known for the load which gives the final temperature-rise θ_{max}. Then

$$\text{Time-constant in seconds} = \frac{\text{(joule capacity at final temperature-rise } \theta_{max})}{\text{(watts dissipated per sec. at final temp.-rise } \theta_{max})}$$

Mr. W. E. Groves (*communicated*) : It is not possible to foresee the conditions that will arise during the development of a large supply scheme and, unfortunately, networks have to be planned a long time before the anticipated demand is realized. In the interval, loads may turn up in unexpected directions, therefore a step-by-step policy must be pursued. At the same time one must base design with some ultimate lay-outs in view and with definite assumptions ; the present paper is therefore of great interest if its application is limited. The time factor and its attendant uncertainties seem to be left out of the calculation ; and this is particularly apparent in the reference to the effect that capital charges on the distribution system equal or exceed the coal bill. This could surely only apply to a certain period of development. The most

important thing to know is the load capacity of the cables, concerning which useful information has come to hand in the preliminary Report on the Heating of Buried Cables and also in the present paper. On page 99 the author mentions the scope for ingenuity to meet various conditions, and that paragraph forms a fitting commentary to the earlier portions of the paper. This view will be heartily endorsed by those engaged in designing for posterity. The reference to ring mains being used only for a small number of isolated loads needs some qualification. It is an attractive scheme for pioneering work and for dealing with E.H.T. consumers even in dense districts. In Birmingham there are upwards of 200 such consumers supplied on this system, the individual loads varying from, say, 100 to 1 000 h.p. It is possible to make tappings as safe as straight joints and, though for convenience of handling they should not be used for general supplies, where economy demands it they can be employed with great advantage for stand-by purposes. Stand-by feeders are never economical as regards capital outlay, but are essential in varying degrees. In a supply network the normal grading of loads to the load capacity of the conductors can be satisfactorily dealt with by the manipulation of section switches. It is, of course, essential that this should be in the hands of a control engineer. The "despatching" of loads will be governed rather by the economic running conditions of the stations for the time being than by the size of the cables. I agree with the author that cables are very reliable, and also that heavy outlay on pilot protection systems for distribution networks on a large scale is difficult to justify. The reliability of cables makes it practicable to avoid the complications of extensive interlinking, provided alternative means of supply can be brought into service at short notice The author does not mention the system of protection he would advocate in the various interlinking schemes dealt with in the early portion of the paper.

[The author's reply to this discussion will be found on page 256.]

North-Eastern Centre, at Newcastle, 24 January, 1921.

Mr. G. L. Porter: The whole question of cable loading has come rapidly to the front in recent years, mainly owing to the following causes: (1) The laying of surplus copper to meet future requirements practically stopped soon after the outbreak of war; and (2) the present financial stringency renders it necessary to make the fullest possible use of existing copper before spending further capital. I am glad the author has pointed out that the use of boosters is a great assistance towards the full utilization of existing copper. This is not sufficiently realized, but at the same time I consider that a word of warning is also necessary. It must always be remembered that to lay additional copper reduces the distribution losses, whereas the use of boosters always increases them. Additional copper directly increases the gross capacity of the distribution system; boosters at fixed points merely increase its available capacity under certain conditions of loading which may altogether change in a few years. The author states on page 97 that, with E.H.T. distribution, heating and not voltage drop is always the determining factor and that with L.T. distribution the converse always holds. It is perhaps unnecessary to point out that these statements are much too strong, seeing that on page 95 the author qualifies them by using the terms "almost always" and "usually." In reference to Fig. 5, the author has apparently overlooked the fact that, owing to the reactance of the step-up and step-down transformers, the ratio of resistance to reactance on the 6 600-volt and 30 000-volt routes will be so different that, even if X is the point of lowest potential, a considerable wattless current will flow across it, and in practice it will probably be advisable to leave the switches open there. I am unable to follow the author's remarks under the heading "Most Economical Voltage." Although the cost of cable per kilovolt-ampere of cable capacity is less the higher the voltage, it does not follow that the cost of cable per kilovolt-ampere transmitted is also less. What does he mean by the expressions "as high as convenient" and "the highest safe voltage"? Again, he states that the highest voltage at which power can be sold is 6 600, although I cannot agree with the figure. Then if 11 000-volt distribution be chosen it will be necessary to step-down to 6 600 volts or less before the power can be sold, and, in order to get a fair comparison with 6 600-volt distribution where such transformers are not essential, their cost should be included and the 11 000-volt curve in the figure would then cross the 6 600-volt curve. It seems to me that this section of the paper should have been headed "Most Convenient Voltage." I cannot agree with the conclusions which the author draws from Fig. 8. Had he carried the curve a little further he would have found it rising again, and it is obvious that a curve which is merely passing through a minimum value after a steep fall cannot possibly be flat through such a long range. I have plotted a similar curve from the schedule of prices of a large firm of cable makers and I find that the actual minimum is about 0·2 square inch, and that the cost of 0·075-sq. in. cable per kVA is about 20 per cent above the minimum. There are also other considerations which militate against the choice of the smaller section. If the author proposes to run numbers of small cables in parallel it is necessary to have a reliable form of protective gear, and if he takes the cost of pilot cables into account the 20 per cent found above will be increased to 30 per cent. Again, he mentions the reduced cost of the spare cable if a small cable section is chosen. The use of a single spare is only possible if separate switchgear is allowed for each cable. In the modern large power station the switchgear has to be of large breaking capacity, and its cost bears little relation to the size of the feeder. It is not economical to allocate one of these expensive super-switches to a single 0·075-sq. in. cable.

Mr. J. W. J. Townley: In the preliminary part

of the paper the author gives some figures relating to the capital cost of distribution systems and the coal costs for large stations, and a consideration of these figures suggests that they are an argument against the super station, with its usually heavier transmission and distribution capital costs. The argument, however, has weight only if the higher figures of 0·22d. per unit for capital charges on distribution, and of 0·28d. for coal given by the author are accepted. I think that, for the heavily loaded industrial areas in this country, the capital charges for distribution will be very much less than those given, and the coal cost, which with coal at, say, 30s. per ton (a low price at present) would mean 1·75 lb. per unit, is a figure which I believe has yet to be attained over long periods. It would be interesting to have the values of power factor, load factor, and interest and sinking fund charges upon which the distribution costs are based. I find that a power factor of 0·75, a load factor of 30 per cent, and interest and sinking fund of 10 per cent give approximately the same figures per unit as those in the paper. The author's methods of finding the most suitable position for a power station or substation are interesting but, particularly in the case of the former, are of academic interest only, as it is, of course, condensing water that is with our present system the paramount consideration in the choice of a power-station site. Even in the case of a distribution substation the direction of the development of the load in the vicinity is somewhat uncertain, and in industrial districts the choice of sites is limited, so that very approximate methods applied with intelligence will usually indicate the most suitable of the available positions. Recent researches have shown that paper cables can be loaded above the point at which, owing to the additional I^2R losses, it pays to run them, but this overload capacity could be counted on for use in emergency, and the system would therefore not require so many stand-by cables as the author suggests on pages 98 and 99. On the former page the author condemns the "herring-bone" system, but it is very often necessary to tap E.H.T. feeders to supply, economically, relatively small consumers, and I know an E.H.T. system where this has been done for the last 15 years and no trouble has been experienced with the tees. In one or two cases oil-immersed isolating switches have been inserted, but the necessity for these on a 6 600-volt system is doubtful. An interruption in supply once in, say, 5 years costs less than the additional capital cost required to provide stand-by against its happening. The author's suggested use of boosters is interesting, and has already been successfully applied. Much has been written on the question of encouraging the consumer to improve his power factor, and most undertakings nowadays arrange their tariffs to take this into account, but, assuming that a consumer is paying standing charges upon his kVA demand, it usually happens that it pays the undertaking best not to encourage him to improve his power factor, but to do so themselves where and when necessary. This is because a distribution system can rarely be run for any length of time on its maximum capacity. Extensions must be made for several years ahead, and not by degrees as the load increases. The undertakers, by power-factor correction in any area, can postpone the time when extensions have to take place, without reducing the amount of the standing charges received from the consumer, and this is a very important point when one is considering the balance sheet.

Mr. A. W. Crompton : The author appears rather to deprecate the use of cable protective gear, but it is difficult to see how, without the use of such gear, it is possible to ensure that in the case of the breakdown of a cable forming part of an interconnected system no switches shall trip out other than those controlling the faulty feeder. The consequence is that, if such gear is omitted, the lay-out of a network must be such that, in order to avoid the disconnection of sound feeders in case of faults, practically all interconnection or arrangement of substations in series must be strictly limited, and hence the capital saved by the omission of this gear is more than offset by the reduced flexibility and increased cost of the cable system as a whole. I would therefore suggest that in item (1) under the heading of "Cable Lay-out" the words "or disconnected" should be inserted at the end of the first sentence. The statement in item (4) is, of course, only true provided the consumer to be supplied is situated at a less distance from the power house than two-thirds of that between the power house and the particular substation. If account is taken of the fact that less expensive switchgear may possibly be installed if supply is given via the substation, due to the reduced short-circuit current which it will have to break, it may be found that the equivalent distance from the power house is something less than two-thirds. The deciding factor in cases of this kind is the magnitude of the load required by the new consumer. With regard to the most economical voltage, I cannot quite see why the author suggests three voltages. In the majority of cases transformation will be required even with 6 600 volts, and it would appear a much more economical arrangement to have 11 000-volt generation and distribution, with the maximum voltage allowable for cables, say 30 000 volts, for transmission. It will be noted that in Fig. 6 it is shown that the cost per kilovolt-ampere is at all distances least in the case of 11 000 volts. The author suggests a very good method of obtaining a record indicative of the temperature variations of cables under normal working conditions, and it is regrettable that greater use cannot be made of this for the reason stated. The curves are drawn without making allowance for the fact that the time-constants of all the cables will not be the same. It is to be hoped that, as a result of the work now being carried out by the British Electrical and Allied Industries Research Association on the Heating of Buried Cables, the information required in regard to time-constants will be obtained.

Mr. C. Turnbull : The paper marks a further step in the development of electricity supply. Up to now the sizes of mains have been fixed largely by rule of thumb; indeed, this is necessary owing to the fact that one has not been able to foretell the development which may take place in any district. In actual

experience, demands for current follow no law, and it is not uncommon to get a very large demand in a district where little had been expected. As time goes on, however, and as electricity becomes used to a greater extent, there will be a growing need for the methods outlined in the paper. I think that in erecting large stations one should first of all select a site that is the most suitable for generating purposes, even if it is not at the centre of gravity. Sites should be chosen, if possible, in the neighbourhood of large vacant areas, on which an option should be taken. Encouragement should be given for new factories to be erected on these sites. I think the time is coming when, in fixing the price for current, we must take into account the distance of the consumer from the generating station. It is evident that a factory which is within half a mile of the station can be supplied at a cheaper rate than one which is 20 miles away. With the higher pressures in use, we are coming to a point where the carrying capacities of cables will be limited by their heating, in the same way as the carrying capacity of conductors in motors and generators. This being the case, one is surprised to see that shaped conductors are still much used for high-pressure cables. It has been stated on good authority that a cable with shaped conductors carries 10 per cent less current than one with round conductors. If this be correct, and seeing that the price of shaped-conductor cables is very little less than that of cables with round conductors, it would appear desirable to give up entirely the use of shaped conductors.

Mr. W. Tolmé Maccall (*communicated*): In Figs. 10, 11 and 12, I think the vertical scale should be marked in $(\text{kVA})^2$ instead of $(\text{kW})^2$, as the rate of heating is proportional to the former. With regard to the application to transformers of this interesting graphical method of obtaining temperature-rise, it should not be necessary to alter the procedure from that applied to cables. The equation of the heating curve of a transformer may be written (cf. p. 103):

$$y = (T_1 + k^2 T_2)\ (1 - e^{-x/c})$$

where T_1 = final temperature-rise due to iron losses only, k = load in kVA, and $T_1 + T_2$ = final temperature-rise due to a load of 1 kVA. It follows from this that the point of zero load is above that of zero temperature-rise (as is obvious from other considerations). It also follows that above zero load the temperature scale may be marked $(\text{kVA})^2$ just as in the case of cables. The only further point to settle is the distance at which the zero of load must be placed above the zero of temperature. One simple method of finding this is to find the load (say K kVA) which makes the copper losses of the transformer equal to its iron losses. Then zero load must be placed a distance K^2 [measured on the $(\text{kVA})^2$ scale] above zero temperature-rise. When the transformer is on load so long before the peak occurs that the rise of temperature due to iron losses has reached practically its maximum value, the procedure can be simplified a little. The additional temperature-rise above T_1 is then given by applying without modification the method for cables.

Mr. R. O. Kapp (*in reply to the discussions before the North-Western, South Midland, and North-Eastern Centres*): Various speakers have criticized some of the figures in my paper, and it must be borne in mind that, at the present time, figures as to cost vary so much that such quantitative statements as are contained in the paper can be regarded as a guide only, but not as accurate figures.

Mr. Watson considered my estimate of £4 per kilovolt-ampere for the cost of a given distribution scheme too low, and Mr. Romero considered it too high, which suggests that the figure may be somewhere near the truth. Personally I think the figure is low, and I chose it as the lowest limit one could conceivably obtain.

Mr. Watson and Mr. Porter and other speakers expressed the opinion that 0·075 square inch is probably too low a cross-section for economy, and I admit that Fig. 6 does not include cable losses or switchgear. If these are included, as suggested by Mr. Porter, the most economical cross-section would possibly be above 0·075 square inch, though it appears to be a matter that must be considered for each scheme and on which generalization is not easy.

With regard to protective gear, I think that the inclination in the past has been greatly to exaggerate its advantages, as I said in London. This is largely due to the fact that such gear has been called protective gear, which suggests to one's mind that it has a more specifically protective function than is actually the case. When we remember that such gear does not protect the cable from damage, but only comes into operation when the cable has already been damaged, some of its apparent virtue seems to evaporate. I do not deny that protective gear has distinct advantages, and that it is desirable in many cases, but in many other cases it does not justify its cost.

I would refer Mr. Watson, Mr. Ratcliff, and others who expressed the opinion that the cross-section on a ring main should not be reduced, to my reply in Liverpool, but I agree with Mr. Blades that the actual cases in which one would be likely to reduce the cross-section in practice will be rare.

With regard to economical voltage, I am inclined to agree with Mr. Porter and Mr. Crompton that there is little to be said for 6 600 volts. For the purpose of the curve in Fig. 6, I assumed this voltage because it is one that is so much in use, but it seems quite likely that it will go out of fashion in the future. In reply to Mr. Ratcliff, my curves assume the same temperature limits on all cable, that is to say, different current densities.

Mr. Romero points out that the losses on a 33 000-volt cable are a fifth of those on a 6 600-volt cable, but this does not apply to the whole transmission if the iron and copper losses in the transformers are also taken into account. Fig. 6 takes account of these, assuming a load factor of 30 per cent.

I agree with Major Taylor as to the advantages of stepping up the voltage on existing cables in order to get more service out of them wherever this can be safely done.

Mr. Brew and others have pointed out that a logarithmic

curve is only an approximation to a true heating curve, but, as Mr. Line said, in spite of this the approximate figures given by constructing temperature charts may be of value, particularly where the time-constant is great. Mr. Line's two suggestions for finding the time-constant are interesting. A difficulty may be that it is not easy to draw a tangent to a curve accurately, and in my reply to the Liverpool discussion (see page 130) I gave a reference to a further method for determining the time-constant.

I fear I have too little experience of overhead line work to be able to answer Mr. Wilson's question concerning the economical voltage of transmission for this.

In answer to Mr. Townley concerning the economical size of a super-power station, it must be remembered that, even if the actual coal bill is greater, and the actual capital charges on transmission lines smaller than I assumed, a size is reached where increase in the size of the station brings no appreciable saving in coal, administrative charges, or capital charges on the power house with it. Increase of size beyond this point, however, brings with it an increase in cost of the distribution system, so that the cost per unit to the consumer is bound to rise.

Mr. Blade's experience with extension due to temperature on the Birmingham cables is most interesting.

I quite agree with Mr. Porter with reference to Fig. 5 that it would be better for working conditions to disconnect at point X so that the area supplied through the 30 000-volt system is not in electrical connection with that supplied through the 6 600-volt system, and on a large scheme the two areas would probably have to be disconnected anyhow, because they would be fed from different sections of the busbars, which would be interconnected by reactances. However, one is still likely to retain one or more feeders between these two areas as stand-by feeders, which though normally disconnected might, on emergency, be used. For this emergency condition a variety of transformer tappings would be useful.

The point raised by Mr. Turnbull, that it may be necessary to take account of the distance of the consumer from the generating station in fixing the price for current, is worth serious consideration. So much can be said on both sides that it is impossible to deal with it at the present moment.

I was most interested in Mr. Maccall's suggestion for drawing a temperature chart for transformers, which is certainly a much simpler proceeding than that given in the paper.

DISCUSSION ON "THE DISTRIBUTION OF ELECTRICITY."*

NORTH-EASTERN CENTRE, AT NEWCASTLE, 22 NOVEMBER, 1920.

Mr. J. W. J. Townley: I do not agree with the author's statement that a step-by-step development is consistent with economy. My opinion is that a broad view must be taken in laying out a distribution system. This is particularly the case where mains have to be laid through urban areas (and it is not always possible to avoid this, even with a transmission system) where the cost of laying and reinstatement is a relatively large proportion of the total cost. In such cases consideration should be given to the question of laying cables for the highest voltage considered likely to be used, and run them if desired at a lower voltage until circumstances make the step-up to the higher pressure desirable. Mr. Beard dealt with this matter in his recent Address † and suggested that the step-by-step development might be taken too far. I believe greater expense would be involved and considerable confusion caused when changes were made later, than if the system were laid out at the start with a broad view of what would be ultimately required. I agree with the author that a number of different pressures are desirable, but I think that with the development of switchgear and transformer design, particularly the latter, there will be a tendency to give supplies to consumers' transformers at much higher pressures than hitherto. Incidentally we find it commercially possible to supply consumers with demands down to 50 kW at 5 000 volts, three-phase. The author states that "it does not appear that there is a superior economical limit to the size of a power station"; but if it be accepted that little improvement in consumption or reduction in capital cost can be effected with sets above about 30 000 kW,‡ then I am of the opinion that very little saving could be effected by putting down more than four or five such sets on any one site. At any rate, the advantages would not be sufficient to counterbalance the additional cost of transmission lines, and this point the author appears to have in mind when he says that "transmission of electricity is, from an economic point of view, not a virtue but an expensive necessity." The author's remarks regarding load factor are very interesting, and are on the lines of an article which he published some time ago. I do not think sufficient consideration has been given to diversity of incidence of demand when considering tariffs for consumers whose maximum demands do not occur within the usual factory hours, and there is no doubt that this should have very close attention when considering the effect upon the costs of any undertaking. It is interesting to note that the author recommends underground cables for main transmission, and, in view of the fact that upon the system for which he is responsible the main transmission lines are largely overhead, it would be interesting to hear from him whether his experience of the working of these lines has influenced his opinions, or whether financial considerations were the determining factors in the choice of transmission for his own system.

Mr. C. Vernier: I generally concur with the author's conclusions on page 93, except No. 7, and on this point I do not agree with his advocacy of a step-by-step development. In my opinion it is essential to take the longest possible view of the ultimate development of the area to be supplied, and now that the whole country is being mapped out by the Electricity Commissioners into districts, this factor will be a good deal more definite when finally settled in the near future than it has ever been in the past. It is perhaps difficult at a time of financial stringency such as this to take the long view, but we should at any rate take the longest view that our finances will allow and, if necessary, go to some financial sacrifice to establish the correct transmission pressure for the area in the first instance. In 1906 we on the North-East Coast were pioneers of the adoption of 20 000 volts as a system pressure, but, were we considering the problem to-day, I think we should adopt 11 000-volt distribution with a very much higher pressure for transmission, as 20 000 volts is rather too high for distribution, and too low for transmission. Nevertheless, the adoption of 20 000 volts was a very long view to take in 1906, whereas to-day we can with greater assurance consider 55 000 to 66 000 volts as the possible maximum voltage for underground cables, which voltage is very suitable for use on overhead lines in our climate, and ample for the distances to be covered. A wrong decision on this question, or the adoption of a policy of expediency, would in many cases be greatly regretted in perhaps as short a time as 10 years, and, for this reason, I support Mr. Beard's suggestion in his recent Address * that the Electricity Commissioners might well loan part of the capital required at a low rate or even free of interest, to electricity authorities to enable them to take the long view and tide over the initial unremunerative period. I cannot reconcile the statement (on page 88), "For practical purposes it may be assumed that the cost [of insulation, sheathing and armouring] is proportional to the square of the pressure" with my own figures. The increase is not nearly so rapid. I must also disagree with the similar statement on the same page that the cost of an overhead line may be assumed to be proportional to the pressure, between 6 000 and 66 000 volts. This implies that a 30 000-volt overhead line would cost five times as much as a 6 000-volt overhead line, whereas, between 6 000 volts and 30 000 volts, the increase in cost with an increase of pressure for the same size of conductors is practically only that of larger insulators. Between these limits the problem is more a mechanical one than an electrical one, and resolves itself into the

* Paper by Mr. W. B. Woodhouse (see pages 85 and 108).

† *Journal I.E.E.*, 1921, vol. 59, p. 30.

‡ See paper on "Power Station Design in Relation to Thermal Efficiency" by I. V. Robinson, Wh.Sc., *Beama Journal*, 1920 vol. 7, p. 117.

* *Journal I.E.E.*, 1921, vol. 59, p. 30.

supporting of a given number and size of conductors with economy which, with wood poles, determines the span length, and the latter the spacing. The spacing does not, within those limits of pressure, depend, except in the very smallest degree, on electrical considerations, but on the sag. For higher pressures, wood poles are not usually suitable, and therefore longer spans and larger spacings become obligatory. The author makes the statement on page 92 that the minimum section of high-pressure cable is determined by two considerations, that of maximum permissible stress, and that of minimum mass to withstand overheating, and that, given an ungraded cable, a minimum section of 0·05 sq. in. appears necessary for pressures above 10 000 volts. This appears to suggest that the maximum stress is adversely affected by the diminution of conductor diameter according to the law of potential gradient, whereas the contrary would appear to be the case. Mr. G. H. Nisbett many years ago * first called attention to a number of tests which he had carried out, showing that a number of small cables gave higher average breakdown pressures than a similar number of larger ones. For a long time I was sceptical, but recent tests have convinced me that small cables for a given thickness of insulation will stand as high, if not higher, breakdown pressures than large ones. At the present time I am engaged in laying a 0·0225 sq. in., 20 000-volt, 3-core cable, approximately 10 miles in length, which is of course very much smaller and for a much higher pressure than the author appears to consider permissible. As to minimum mass, this depends entirely on the possible short-circuit current, and can always be safeguarded by external reactance or by taking care that such cables are only laid in the more remote parts of the system, where the short-circuit currents are reduced to a safe figure. As to dielectric losses, a certain amount of work has been carried out privately, but unfortunately is not available for publication. It may be of interest to mention that the question, so far as it affects the current-carrying capacity of E.H.T. cables, is included in the programme of the Committee of the British Electrical and Allied Industries Research Association, which is at present engaged in the Research on the Heating of Buried Cables, and it is hoped that the work may be carried out during the coming year. As to high-pressure cables for main transmission, I have already expressed the opinion that the correct pressure for the area should be adopted in the first place, if at all financially possible, and thus avoid the addition of one or more transmission pressures at a later date with all the disadvantages involved in a multiplicity of standards, and the greater amount of spare apparatus required for different pressures. Regarding underground cables I can say that 55 000-volt, 3-core cables are now actually being made and laid, and I am satisfied that the problem of making and jointing 66 000-volt, 3-core cables is now practically solved. A certain amount of very satisfactory experience is also available from special tests carried out during the past 15 months as to their probable behaviour under service conditions.

The progress achieved by some of the leading cable-makers in the manufacture of E.H.T. cables during the past two years is quite remarkable, and has very largely contributed to diminish the increased costs of underground electrical transmission and distribution arising from the war, with the result that this increased cost is probably lower with E.H.T. cables than with any other class of electrical apparatus. In the early part of the paper the author suggests that there does not appear to be a superior economical limit to the size of a power station. I think that there is a very definite superior economical limit not only to size of unit but to the size of a power station on any one site, and that this limit will be much below such figures as 300 000 to 500 000 kW which are often mentioned. The limitations are coal-handling and ash-handling facilities, cooling water, and cable routes.

Mr. E. H. E. Woodward : The best answer to the question raised as to the advisability of step-by-step development, as opposed to development on the broad and final scheme from the commencement, is given earlier in the paper where reference is made to "the time element." In nearly all electricity supply undertakings capital has to be expended a considerable time before any adequate return can be expected, and it is essential, both from the point of view of the suppliers and the consumers, that the amount of this dead capital should be kept as small as possible. The best course seems to be to carry out developments gradually, as required, but always to keep in mind the idea of the final lay-out of the system, and to arrange each successive development so that it will fit in with the final scheme. At the end of the paper, the question of supplies to consumers of from 100 to 200 kW is dealt with. It is often a difficult matter to decide the best method of giving a supply of this size, as they are generally too large to be connected to a low-tension network, but on the other hand the capital expenditure involved in installing a 10 000-volt or 6 000-volt supply is very heavy in proportion to the amount of current that will be used. This is particularly the case in an extensive interconnected system, where it is generally impossible to adopt any less expensive methods of supplying smaller consumers without affecting the general security of the system.

Mr. J. M. Heslop : The author refers to Kelvin's law, but there is one point which he does not mention and which should probably be emphasized, i.e. the effect of the general increase in costs since 1914 on the most economical size of conductor. The effect of the rise in cost of cables is to increase the slope of the line representing annual charges on conductors. This increase in slope has the effect of moving back the point of minimum cost towards the origin. In other words, the higher cost of cables to-day means that, for a given duty, the most economical size of conductor is somewhat smaller than in pre-war days, or, in other words, having due regard to safety, it pays now to run conductors at a higher current density than formerly.

Mr. S. H. Fowles : For many years a tremendous amount of copper has been laid in the ground, and it has not earned its correct share of revenue to repay the capital expended, and so has become a burden to small undertakings. In a similar manner many small

* *Journal I.E.E.*, 1907, vol. 40, p. 36.

sub-stations have been erected, and the annual losses have been enormous; this applies especially to many small undertakings. I should like to ask the author what he considers should be done in connection with the linking up of our waste-heat stations. We have now a good number, some of which are isolated for two reasons. One is that the power companies will not give a reasonable price for the surplus energy that can be supplied from this source, while a second cause is that they may be situated some distance from the supply mains, but I think the former is the more common cause of delay. These waste-heat stations are running very efficiently, and are very helpful to the greater networks for several reasons. First, they form a boosting centre in the long feeders, in many cases; secondly, they at times give support to the large stations by supplying a heavy load as surplus to the large net-

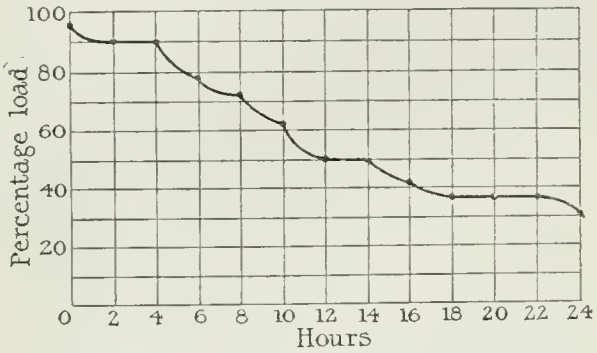

FIG. L.—Graded load curve of a gas-driven, waste-heat station (1920).

works in case of a large unit failing, and so give very useful help until such time as spare plant can be got under way; lastly, they form a splendid asset for load diversity and often allow the network to be split up for fault location without any cessation of supply. The author showed some very interesting graded curves of daily loads. Fig. L shows a graded curve of the daily load on a gas-driven waste-heat station, which agrees very well with some of those the author produced. This station has run on a load factor of 70 per cent for five weeks continuously, and has worked 85 per cent of the possible engine-hours, only one hour in 5 040 being lost owing to engine trouble.

Mr. N. W. Prangnell: The paper brings home to us how necessary it is to use cables and plant to their full capacity, by utilizing the load curve and taking every possible advantage of the diversity factor. Possibly drastic methods should be introduced, even if it leads to stipulating the hours at which a supply may be given to users whose load curves may be extremely bad from the supply authority's point of view. Co-operation between the supply authorities and consumers, with a view to levelling out the load curves by means of regulating dinner hours, starting and leaving-off hours, is well worth a careful study. When one realizes that, in many cases, the load factor of an individual consumer may be only 20 per cent of his maximum load, the necessity for providing mains of sufficient capacity to deal with his maximum demand indicates the spending of perhaps unnecessary capital. Under the heading "High-Pressure Cables" the author mentions the considerations necessary when dealing with large-capacity cables, and I should like to amplify this by stating that a 0·25 sq. in., 3-core cable suitable for working at a pressure up to 50 000 volts would require 0·5 inch of insulation between conductors, and between conductors and the lead sheath. This would give a total diameter of finished cable of over 3 inches. The laying of such a cable has to receive very special attention, and, as I have proved myself, it may even be necessary to stipulate that such a cable shall only be laid in warm weather, or that it shall be warmed before being rolled off its drums and handled for the laying operations. It may be that these considerations will outweigh the possibilities of using such large cables, and it would be interesting to know what is the largest 3-core cable for above 20 000 volts that has so far been built and put into use. I should like also to mention the jointing of such cables. Under no circumstances should the insulation be coned to the joint in the conductor by the usual method of shaving it down with a knife. By the use of such a method a series of flats is left, as is the case when sharpening a lead pencil, and, on the application of insulation, pockets are left on the coned surface. The coning should be done by means of carefully unwinding the insulation. Then again, in all E.H.T. cable jointing, the jointer's hands should not come in contact with any of the insulation of the joint. Oil-impregnated paper absorbs moisture from the fingers, and this is inclined to collect in quite large globules which, in time, may cause trouble. In the jointing of the 100 000-volt, single-core cable that was installed by the Metropolitan Electric Supply Company for the constant-current Thury system, the presence of moisture from jointers' hands was found to restrict the breaking-down voltage to below 50 000 volts, but immediately the moisture on the jointers' hands was eliminated it was possible to increase the pressure to over 100 000 volts. These are points worth remembering when dealing with high-pressure cables.

[The author's reply to this discussion will be found on page 273.]

NORTH-WESTERN CENTRE, AT MANCHESTER, 23 NOVEMBER, 1920.

Mr. H. A. Ratcliff: Mr. Woodhouse's paper invites discussion in every paragraph, for, as the author remarks, the technical and financial considerations involved are numerous. The reference on page 86 to the time element is extremely desirable, in view of the fantastic schemes which are put forward from time to time in the lay Press. To judge from some of these schemes it would almost appear that a 200-mile transmission line was regarded as a desirable adjunct to a generating station, whereas (as the author points out) transmission is not a virtue but an expensive necessity. I am pleased to see the reference to pro-

tective gear, and I feel that we appear to be approaching a stage of development where we shall require super-protective gear to protect the protective gear. With regard to the interconnection of networks, I am of the opinion that this should be effected through the medium of the E.H.T. transmission mains, and that, so far as possible, the intermediate-pressure and low-pressure networks should remain isolated. There is one point with regard to overhead lines which I would like the author to amplify. I cannot quite understand his statement to the effect that the cost of overhead lines is proportional to the pressure, for, if so, it hardly agrees with figures which I have studied, and it would then appear that the only advantage of raising the line pressure lies in the reduced transmission losses. The author deals very thoroughly with the question of load factor and losses. I do not think the effect of the load factor on the copper losses is always appreciated as it should be. Not only are the copper losses peak-load losses, but they also have a load factor which may be very appreciably lower than that of the load. A 16 per cent difference has been cited, and it may even exceed that figure. That means to say that the cost of generating energy which is dissipated as copper loss is relatively high, and consequently such loss should be charged at a figure quite as high as, or probably higher than, the maximum price charged for electricity. Furthermore, since the copper loss is a peak-load loss and has a load factor much lower than that of the load, it has the effect of reducing the load factor of the system, and therefore raising the average cost of generation. In contrast to the copper losses are the iron losses in transformers, and the dielectric losses in cables, both of which are, within certain limits, practically constant, irrespective of the load, and they can therefore be charged at the lowest possible rate. I had occasion some time ago to make calculations of load-factor losses, and as I found the work rather tedious, and with the object of simplifying it, I arrived at a formula, which for simplicity will compare favourably with the author's. If F = load factor of load, and f = load factor of losses, then for average daily load curves $f = F^{3/2}$. The advantage of this formula is that the calculation involved can be made with one setting of the slide rule. I was satisfied as to the accuracy of the results derived from it when I found that they agreed very closely with the loss load-factor curve given in Mr. Beard's paper on "Design of High-Pressure Distribution Systems." * With regard to the carrying capacity of cables, the author suggests that, as the pressure is raised, the capacity of cables is reduced owing to the increased thickness of the dielectric. That is no doubt true to a certain extent, but I consider the main reason for the reduced current density at the higher pressures to be the very rapid increase in the dielectric losses, which vary approximately as the square of the pressure. The explanation given by the author depends upon the relative thermal conductivities of the dielectric and the medium external to the cable. The total temperature-rise of the conductor depends upon the sum of the internal and external temperature gradients, as shown in Fig. M.

* *Journal I.E.E.*, 1916, vol. 54, p. 125.

The gradients follow the well-known logarithmic law, and it is obvious from the figure that, under certain conditions of relative thermal conductivity, an increase in the thickness of dielectric will have very little effect on the total temperature-rise of the conductor, because what is added to one curve is subtracted from the other. In fact, it would be possible to have conditions such that an increase in the thickness of dielectric would actually reduce the temperature of the conductor. The author gives a formula for the permissible current density in 11 000-volt cables, and therefore the following formula for 33 000-volt, 3-core cables may be of interest. During the last few years I have been associated to some extent with the preparation of specifications for such cables, and, from a consideration of the principles involved, and the observed results of tests at manufacturers' works, I arrived at the formula $I = 300\sqrt{(aD)}$,

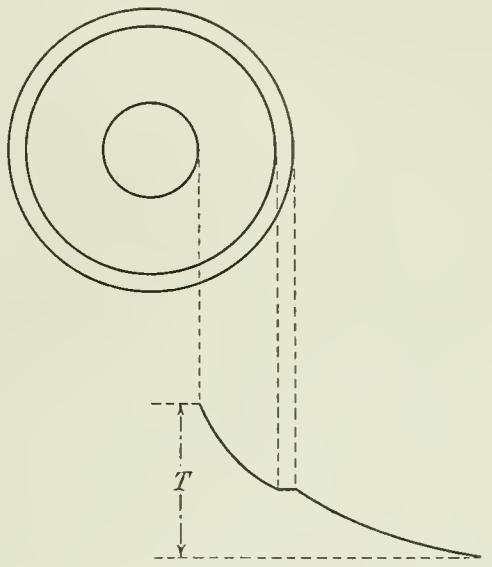

FIG. M.—Showing how the total temperature-rise, T, of a conductor depends upon the sum of the internal and external temperature gradients.

where a = the area of one conductor, and D = the diameter over the lead covering. Whether it is scientifically sound or not may be open to question, but the current loadings derived from the use of it have not so far been questioned by the cable-makers. Referring to the dielectric losses, they are of course, as the author suggests, proportional to the capacity of the cable and to the pressure, but unfortunately they are also proportional to something else, i.e. to the temperature of the cable and to the power factor of the dielectric regarded as a capacity. That power factor should, of course, be as low as possible, but unfortunately it may at times be very high, depending upon the characteristics of the cable. The power factor may rise very rapidly with temperature, or it may rise comparatively slowly. What is required for E.H.T. work is a cable possessing a dielectric power factor which only varies slowly with increase of temperature. There are, however, always two sides to a question, and even dielectric losses have their advantages, for one of the best ways of dissipating a surge is by means

of a capacity with a non-inductive resistance in series with it, and that is practically the equivalent of a high-capacity cable having a relatively large dielectric loss. With regard to the dielectric loss increasing as the square of the pressure, I looked into that some time ago and came to the same conclusion as the author, i.e. that there is no very obvious reason why it should increase with the square of the pressure, and it would appear to be possible by suitable construction to reduce the rate of increase, but the cable-makers are not yet prepared to accept that view of the matter.

Mr. S. J. Watson: I am of opinion that the title of Mr. Woodhouse's paper should be "Some Considerations in the Design of Distributing Networks." The author puts forward some very important considerations, and, if one had to start afresh and lay out an entirely new distributing system, every point which he makes in his paper would be of great importance, and to those of us who are engaged upon distribution systems of any size it gives a good deal of information which will be useful in carrying out extensions. In the past, the laying out of distribution systems has been of slow growth, and one had to compromise here and there as the system developed. Those of us who have been engaged in electricity supply for industrial purposes know quite well that where one least expects load, and where the conductors are of a minimum size, there will be the largest demand. The author very rightly points out early in the paper the difference in the value of the losses between generating by means of water power and by means of steam plant. Once a water-power plant is established, the value of the losses, either in generating or in transmission, are almost, if not quite, negligible, whereas in the case of steam plant one has to purchase coal in order to supply the losses. This difference between the two methods is sometimes not fully appreciated. The author mentions the question of supplying power at a distance to large communities in dense areas. So far as this particular area is concerned, and also in many districts in Yorkshire and the North-East Coast, the load is extremely dense. In some particulars which I obtained a short while ago, I found that the ultimate requirements for industrial purposes amount to something of the order of 0·5 horse-power per inhabitant, and in greater Manchester, for instance, with a million inhabitants, the ultimate demand is bound to reach, and will possibly exceed, half-a-million kilowatts. That is a somewhat rough and ready rule, but it gives results which are approximately correct. In some districts it goes up to 0·625 h.p. per inhabitant, and in certain districts it may be higher still. I only give these figures as some indication of the amount of power which will ultimately be demanded in busy industrial districts with an intense demand. In such cases it will not be economical to generate at a great distance and incur appreciable losses in transmission. The power station must be near the centre of the district. At the end of the paper Mr. Woodhouse remarks that the failure of a 50 000-kVA cable would be a serious matter. For that reason I cannot see the slightest possibility of so large a cable being brought into general use; I should say that 20 000 kW, or even half that amount, is sufficiently large, by reason of the tremendous inconvenience and loss incurred if the cable goes out of commission. Mr. Woodhouse suggests that three pressures will probably be adopted. To some extent I agree with him, but on the other hand we cannot overlook the fact that if there are three pressures, say 10 000, 2 000, and 400 volts, then in certain cases a double transformation may be necessary between the main transmission pressure and the low-tension distribution pressure, and this would appreciably increase the losses. Where a second pressure is adopted it will have to be used, I think, entirely in the form of the intermediate pressure, say 2 000 for large motors in industrial works. We must pay the greatest possible attention to this question of losses. Our object is to give the cheapest possible supply; any additional transformation must mean some loss, and therefore the prices must be higher to reimburse us. The question of interconnection between power stations is one of importance at the present time, and I entirely agree with Mr. Ratcliff that it is only the main transmission line which should be interconnected. If interconnection be made at every possible point, and a serious derangement of control gear should occur, there would be a great deal of trouble and much time would be lost in restoring the supply. On pages 90 and 91 Mr. Woodhouse sets out only three items making up the cost of the supply. I am not quite sure where he includes the main transmission losses and distribution losses. Possibly it may be in the production or working costs. If that is so, I am inclined to differ from him. The production costs should be divided into three at least. First there are the costs at the power station with a certain load factor, then the main transmission lines which may have quite a different load factor and diversity factor, and lastly the distributing network which again may have quite a different load factor and diversity factor from that of the main transmission line and the power station. In order properly to calculate the charge per unit delivered to the consumers, I think it essential that one should separate the power station cost, the main transmission cost, and the distributing cost. Mr. Woodhouse mentions in one or two places the tapping of main interconnector lines. Personally, I feel very strongly that main interconnector lines must have no tappings; they must run straight through from station to station without any intermediate tappings which provide weak spots likely to cause trouble in operation. I have had some experience of overhead lines, and I should have been pleased if Mr. Woodhouse had been able to elaborate somewhat that portion of the paper which deals with them. As a result of careful consideration of their use I am inclined to think that they will continue to be extremely useful as secondary lines, but I do not think they are sufficiently free from troubles to be used for main transmission lines or interconnectors of large carrying capacity. In saying this, I do not infer that they should not be used at all, because they have given splendid service in the past, but they are subject to interruptions which do not occur to a well-made and well-laid underground cable. On the other hand, I think that, for secondary purposes, overhead lines

will be used to a far greater extent in the future than has been the case in the past.

Mr. H. R. Burnett : I should like to know whether the author includes pilot wires in his cost. I do not see any reference which indicates whether they are included in the cost of construction, either underground or overhead.

[The author's reply to this discussion will be found on page 273.]

Western Centre, at Cardiff, 6 December, 1920.

Mr. A. J. Newman : I notice that the author mentions on page 86 that the revenue rarely exceeds a quarter of the capital involved. I can confirm this figure at the present day, but in Bristol we have had to survive for many years past on a very much lower ratio. The author's system of analysing load factors and load losses is most illuminating. I had no idea there was so much in the subject, being quite content to use a planimeter and proceed on these lines.

Mr. A. N. Moore : Is it desirable to have two different high-pressure (2 000- and 11 000-volt) mains to consumers ? On page 88 a curve is given showing the cost of cables laid in ground. There the cost is roughly proportional to the square of the working pressure. I think that another curve for one particular section of cable would have given some very illuminating information. On the same page also, on the question of overhead lines, the author says it may be assumed that the cost of overhead lines is proportional to the pressure, but on page 93 he says : " The cost of overhead lines is not likely to increase as rapidly as the working pressure." That is rather misleading, because if in one case the cost is proportional to the working pressure it would increase as rapidly as the working pressure. Apart from that, the cost of overhead lines will vary in proportion to the working pressure. The author might with advantage have enlarged on the cost of energy and given a little more detail. He points out that the working costs were proportional to the load factor, but surely something should be taken into account for diversity factor and stand-by charges, as distinct from standing charges. In the formulæ set down he might have made it a little clearer how the different functions have been arrived at.

Mr. J. W. Burr : I agree that future manufacturing prosperity must depend largely on the provision of a cheap and reliable supply of electricity. If we are to run transmission lines north, south, east and west without considering the question of the annual cost, I do not see how a cheap supply of electricity is possible. The cry of cheap electricity for all appears to me to involve the expenditure of vast sums of money on transmission lines, and it must be borne in mind that, in addition to this expenditure, heavy annual capital charges would also have to be met. How are these charges to be met ? It seems that for many years these transmission lines will not be remunerative, and the only way to meet these charges is either by a financial assistance from the Government or by increased charges to consumers. The author's remarks on the concentration of generating plant lay the old super-station bogey once and for all. I think we are all agreed that the most economical method of generating electricity is not by erecting huge super-stations and distributing over a wide area, but by the interlinking of well-equipped and well-placed central stations 20 or 30 miles apart. The author raises this point in connection with the interlinking of mains. I suggest that these mains should be used not only for supplying consumers *en route* but for taking a supply from waste heat. On my journey from Swansea I noticed many huge jets of flame escaping into the air. If that heat were used for the generation of electricity it would be the source of a cheap supply. I strongly support the author's suggestion that we should have at least three pressures.

Mr. Arthur Ellis : The question of transmission is very important. I am now dealing with three schemes where 33 000 volts has been decided upon, together with an intermediate high-pressure as well as a low-pressure system. I should like to know the author's experience. My own view is that the simpler the form the better are the results obtained. Some systems are so very sensitive as to make one feel almost inclined to superimpose them so that the first should not operate unnecessarily. With transmission the question of the cost of switchgear has to be considered, and this is very serious, because switchgear is the most serious item of capital expenditure in any comprehensive scheme in connection with a bulk supply.

Mr. W. Nairn : The author refers to step-by-step development, and I think everyone agrees that three steps are necessary. As matters stand at present we have taken two of these steps—we extended our low-tension network at 440 volts 4-wire until it could no longer support itself, and we then took the second step and superimposed a 10 000-volt network on it. This second pressure has proved suitable for distributions within a 10-mile radius from the source of supply, i.e. for an area of about 300 square miles, but the 10 000-volt network is now being so extended and loaded that we are faced with the necessity of taking the third step, which will be to superimpose on the 10 000-volt network a still higher-pressure network in exactly the same way as before, both high-pressure networks being completely interconnected and protected. The consumers on this third network would be areas of about 300 square miles. The voltage for this third network has not yet been defined. At the recent inquiry into the London distribution a voltage of 40 000 was mentioned, and a voltage of 120 000 has been mentioned in connection with the Severn scheme, but I think the information which the author has given us, viz. that a cable is now available for a pressure of 60 000 volts, gives a good indication of what this third voltage is likely to be. I should like to know the size over the lead of the 60 000-volt cable referred to in the paper. In limiting our third voltage to a

value which can be carried by a cable we do not take advantage of the ability of overhead lines to carry much higher pressures, and I do not think that in this country we can do so. The third network will be transmitting large amounts of power—the author mentions a main carrying 50 000 kVA—and for maximum economy the mains must be carried right into the heart of the load, which is not practicable with overhead mains. The author gives two expressions for the rating of the cables, one for the current density according to Kelvin's law and the other for the maximum permissible current density. As regards the first rating, I should be very interested to know what this current density is, taking to-day's conditions on a large undertaking. Mr. Beard investigated this subject in a paper which he read before the Institution a few years ago, and he found this rating to be approximately 1 000 amperes to the square inch. I should like to know if the author confirms this figure. The maximum permissible rating for which the author gives an expression seems to be the rating of the cable for a 50 degree C. rise in temperature; perhaps he will tell us in his reply if this is so.

Mr. W. A. Chamen: I agree with the general conclusions in the paper. The chief difficulty is the question of capital; in laying out a distribution scheme for future developments who will find the interest on the capital? Another serious question is that of the very high pressure suggested in order to get an economical supply for a large area. The cost of transformers, switchgear, etc., makes one question whether these high pressures will really be resorted to as a general rule.

Major E. I. David: I agree that for the general system of transmission the three voltages mentioned by the author are most serviceable. We have for some years used 3 000 volts for general colliery use for all machines above 50 h.p., 11 000 volts for ring mains connecting collieries in the same area, and 33 000 volts for linking areas at distances up to 14 miles, and we are at the present moment extending the latter linkage so as to complete a ring main at 33 000 volts. In my experience, the cost of transmission lines does not increase with the voltage at the rate given by the author. In fact, the difference in cost between 3 000- and 11 000-volt lines is only a matter of slightly longer cross-arms and more expensive insulators. As the total cost of insulators and pins amounts to only $1\frac{1}{2}$ to 2 per cent of the total line cost, while cross-arms vary from 5 per cent with wood-pole lines down to 2 per cent with lattice-pole lines, it will be seen that the variables are a very small proportion of the total cost. Until recently it was possible to obtain a very high load factor on colliery plant owing to the possibility of restricting the pumping load to the afternoon and evening shifts, but the effect of the 7-hour working shift has been to introduce a very heavy peak between the hours of 11 a.m. and 2 p.m. The load factor, expressed as a ratio of units generated to kilowatt-hour capacity of plant running, has been reduced from 85 to 65 or 70 per cent as a result. The load factor expressed as the ratio of kilowatt capacity of motors installed to units supplied varies from 20 to 40 per cent, dependent upon the ratio of haulages, winders, and other intermittently running machines, to fans, pumps, and other continuously running machines.

Mr. I. A. D. Pedler (*communicated*): There are many points that require consideration and it would therefore be unwise to comment upon them before doing so. With regard to the area of supply it would appear to me to be as difficult now to attempt to determine the total amount of power to be provided as it is to estimate for the heating and domestic load of a large city, but as one would be dealing with a specific area reasonably large copper sections should be allowed for (more particularly with underground trunk mains, having regard to the high excavation costs). Comparatively high pressures should be utilized to ensure the pressure on consumers' premises being maintained within the Board of Trade limits when the scheme is finally completed, bearing in mind the abnormal pressure-drop in transformers due to poor power factors and peak loads. I am quite in agreement with the author's remarks in regard to step-by-step transformation in raising the working pressure, etc., provided that the period of development is reasonably long. I assume that the original cables laid and insulators installed would be capable of giving good service on the highest pressure which would be ultimately applied. If this is so then it naturally follows that switchgear, transformers, etc., have to be exchanged, involving expense which, added to the high cost of the original installation of underground cables and overhead lines, would probably condemn these transformations for a short period of development. In the adoption of a system of interconnected stations boosting regulators will of necessity be installed and, provided the pressure at the distributing centres and the consumers' premises can be maintained within the Board of Trade limits, the proposition of feeding the distributing centres from the interconnecting lines is a good one, otherwise care should be taken to provide apparatus at the distributing centres to deal with this regulation problem. It is interesting to note that the author has passed through the stages of faulty apparatus, etc., but my experience is that the faulty apparatus period is not yet over, and as long as apparatus is passed out of the factories without undergoing a more strict mechanical test, particularly in regard to the protective portion of the gear, by specialized engineers, we are still likely to experience considerable inconvenience either by having to overhaul gear before it goes into commission or by regrettable experiences when it is in commission. I assume that the term "simplification of electrical devices" includes the use of overload protection with inverse-ratio time-limit fuses, Merz-Hunter, Merz-Beard and similar protective gears.

[The author's reply to this discussion will be found on page 273.]

SCOTTISH CENTRE, AT GLASGOW, 14 DECEMBER, 1920.

Professor F. G. Baily : The paper forms a useful corrective to much vague prophecy and unconsidered opinion, with which the untechnical Press has lately been flooded. Elementary deductions from a crude form of Kelvin's law, from the relation between pressure and weight of copper, and from the economy of mass production, have suggested schemes of such size that existing experience is almost unable to supply data of any validity. That every additional mile of radius renders a customer less desirable, from a distribution point of view, cannot be too strongly urged, and that the reduction of costs of production becomes less and less as the station is increased, until at a perfectly definite distance, depending only on the density of the load per square mile, the loss due to wider distribution absorbs the gain due to mass production. In many parts of this country the load density is, or may become, very great, and the radius of economic distribution is accordingly small. No doubt every increase in the size of the producing units at the station, so long as it reduces coal consumption, first cost and wages, increases this radius ; but the mere accumulation of additional units under one roof tends asymptotically towards a minimum cost, which is nearly reached when several units have been put into use. Furthermore, as the distance depends on a sum of gain and loss, and not on a ratio of costs, any reduction in the cost of transmission will increase the radius, by reducing the magnitude of the loss. The author shows how this cost of transmission may be definitely determined, and gives a much-needed caution concerning the rapid increase in the cost of controlling devices of all kinds, when economy is sought by increase of pressure. This dependence of the economical radius of transmission on the progress of future invention doubtless renders the determination of a scheme a difficult matter, and I am unable to follow the author's proposal that a system of higher pressure may in the future be superposed on existing systems of mains, though it may be applied to a new set of substations feeding into the old network. Like all central station engineers, he hankers after a higher network pressure, but I submit that the consumer's attitude of mind is strongly opposed to this. If electric energy is to be freely used for domestic purposes a moderate pressure is essential, and the present maximum of 250 volts causes sufficient difficulty in the production of small appliances at a reasonable price and with an adequate factor of safety. Excepting the case of electric fires and ovens, where the wiring becomes cumbrous at low pressure, every single electric appliance in a house is cheaper and more durable at 100 volts than at the maximum pressure.

Mr. N. C. Bridge : In the developments of the industry that have come about as a result of the war, and in all the talk of reconstruction, too little attention has been given to the distribution question as compared with the question of generation, and I think the public at any rate have to a large extent been misled into regarding the distribution question as a mere side issue. As a matter of fact, the capital accounts of a typical large power supply company show that for every £100 sunk in generating stations, practically another £100 has to be spent on mains and substations in order to dispose of the power generated. One of the principal economic difficulties is touched upon on page 86 under the heading " The Time Element." It is impossible to forecast, in developing a power system, what will happen a year or two hence, and in any growing system the question of over-run substation equipments may cause as much anxiety as the loading up of the generating stations, because of the difficulty of reinforcement. To secure sufficient flexibility it would appear to be desirable in the first place to equip the substation with switchgear units of a semi-portable type. These could be transferred radially from time to time as the distribution system grew, the distance of any particular rating of equipment from the fringe of the system being kept more or less constant. Switchgear of increased rupturing capacity would replace the original equipments, and additional cables would be laid to these points from the generating stations. Such a policy of development at constant pressure would, I think, be preferable in many areas in this country to the policy of a gradual raising of pressures as suggested by the author. Bulk transmission would of course be at higher pressures in any case. I would ask the author whether his suggestion is that the lower-pressure apparatus be gradually scrapped, or whether the number of working pressures in use at one time is to go on increasing ? There is another matter referred to in the paper which does not seem quite convincing. I refer to the interconnection question. It is suggested on page 86 that the distribution problem is much simplified because interconnectors can be used to feed the system at intermediate points. If a cable between stations has to function as a distributor, it can only have a restricted capacity for transfer purposes. On the other hand, if the cable is intended primarily for interconnection in times of emergency, it can scarcely be counted upon to help the distribution, and in fact should preferably, from the technical standpoint, be a direct line. From the economic standpoint, it would seem to be really difficult to justify any large expenditure on interconnection. The cable is merely idle copper for a large part of the time, and stand-by boilers, as well as generating plant, have to be maintained at both ends to enable it to function in emergency. There is a danger, too, of a tendency to starvation of generating plant where stations are directly interconnected, and there are difficulties in regard to regulation. Also there is the considerable waste of power in supplying losses additional to the necessary waste in distribution. The suggested benefit (page 91) of the diversity of incidence of demand on stations run in parallel is, I am afraid, gradually losing point now that the working hours in industry are becoming shorter and more or less uniform, and the larger the stations the less the diversity. The interlinking of distribution systems without regard to bulk transfer seems much more commendable in general and more efficient than the attempt to run a

group of stations with interconnectors on a minimum of generating plant. The author gives some very useful suggestions in regard to the assessment of cable sizes and cable losses, and in particular a representative figure for the difference between the load factor of the load and the load factor of the losses. This figure of 16 per cent does not seem quite to agree with what is shown in Fig. 2, where 10 per cent would seem to be about the average difference, but it is at any rate a useful guide to what may be expected.

Mr. W. J. Cooper : The author refers to the financial aspect, and I should like to say that, if it can be avoided at all, there should be no question of subsidy. The question of power factor is also referred to. So long as the user of electricity need not pay attention to this factor, the manufacturer has no real demand made upon him to put on the market apparatus with good power-factor characteristics. The result is that avoidable inefficiency has to be borne in the distribution of electricity. I should like to add that during the transitory period between the present and the nearer completion of the scheme, great consideration should be given to the use of existing and further overhead lines because of the facility with which they can be changed to increased pressures and at much less expense than the laying of new cables. This would be a great advantage until we have passed through the experimental stage in E.H.T. cables of, say, 60 000 volts.

Mr. R. B. Mitchell : It is interesting to note that the recommendations in the paper, so far as pressures are concerned, are identical with what has been done in Glasgow. We have always isolated our low-tension distribution networks, each feeder serving a separate network which is completely isolated from the rest, and the system has always worked very well. I recognize, of course, that it is rather wasteful of copper. The 6 500-volt cables are at present interconnected to some extent, but it is intended that in the future these shall be isolated in sections as advocated in the paper. The 20 000-volt cables will be interconnected. With regard to the 2 000-volt distribution system advocated by the author, and which he says is used on his own undertaking, I do not quite see that this is necessary, and I should like to hear some arguments in its favour from the author in his reply. It seems to me that it would increase the capital expenditure to some extent, and in addition if there were at the same time another higher pressure, say 6 500 volts, it might be in many cases more convenient and less expensive to connect even comparatively small consumers to the higher-pressure mains, depending entirely upon their situation. The experience in a large city brings one to the conclusion that it is always best to take a broad view and adopt the ultimate pressures and sizes of cable which are compatible with common sense. The supply engineer can hardly make any calculations whatever about the growth of the load, and he has to lay out his system on certain definite lines without taking calculations very much into account. With regard to 20 000-volt cables, or cables of a higher pressure, not only has the question of a pressure drop to be considered, but also that of the amount of power which can be transmitted along the cable. In large cities the growth of the load is so rapid that the engineer does not want to go back upon his work repeatedly, and the more power he is able to transmit over a minimum number of cables seems to suit the occasion best. With regard to the dielectric losses on E.H.T. cables, it is understood that those losses increase when the temperature rises, and the current-carrying capacity may not on that account be proportional to, say, 6 500-volt cables. The author states that the capacity depends upon the area of the conductor and the rapidity with which the heat might be dissipated ; an important factor in this connection is the system of cable-laying used. Where ducts are used the same data may not be taken. I think we require much more information regarding these dielectric losses before we can go forward with confidence in loading up cables. The author states that one can run these cables with economy at a lower current density than they were capable of. That is all very well, but when they have to be loaded up to their limit to meet the load which comes on at an unforeseen time, we require to know very definitely what is the actual safe current-carrying capacity of a cable. We have carried out some experiments in Glasgow on 0·2-sq. in. cables, and we find we can load these up to 290 amperes with apparently safe temperatures. I agree with the last speaker that the use of overhead cables should be seriously considered. I think the schemes now being laid out in the United States have a great deal to recommend them, and I have in mind one described in the electrical Press recently. I think the area included the State of New York, and in that area there was one overhead line at high pressure, and it was run right through the heart of the system, tappings being taken off to suit the supply of the various areas. So far as my experience goes, underground cables are very reliable, and even 20 000-volt cables have given us the greatest satisfaction ; in fact, cables are now the soundest and most dependable part of our system. The protective devices are also satisfactory ; at least, experience in Glasgow has shown that they can, if carefully attended to, give excellent service, and that may lead to a greater amount of interconnecting than is considered safe at the present time.

Mr. D. Berry : The author in his general expression under the heading " Capital Expenditure " for the cost of a cable per mile laid, divides it into three parts. In the first part he includes that part which is independent of the cross-section of the conductors and of the working pressure. Does he not think that the cost of jointing is in some degree a function of the pressure ? Then, the figures given for the higher-pressure cables in the graphs seem low. Possibly the cables were laid in soft ground. I should be glad if the author would give the constants for certain cases. I should like to know how the equation for the safe rating of 11 000-volt cables is derived. The author does not give the cost per mile in Fig. 7. Will he state what this is ? In conclusion (8) he states that the size of distributing substations is determined by the density of load and size of power-users' installations. I would suggest that he adds the words " and available sites " to the end of the paragraph. This will apply more especially in the

case of cities. The question of the cost of excavating and reinstatement is certainly a far more serious factor for consideration with large municipal undertakings than with undertakings having no hard-paved streets in their area, and if reinforced concrete is adopted street opening may become the most serious item in cable laying.

Mr. A. P. Robertson : With regard to the size of power stations, the author mentions that the higher the pressure the more expensive are the stations, and that sometimes it does not pay to adopt extra high pressures. I was pleased to see that he recommends that the lowest economical pressure should always be used. I think that nowadays a good many people are trying to use the highest pressure, although lower pressures would often be more suitable. Take switchgear, for instance ; in doubling the pressure one almost quadruples the cost of the switchgear for the same capacity. High-pressure substations ought to be made fairly large. I have a few figures in that connection. A 6 000-kVA substation can be put down at £6 per kilovolt-ampere and, if the figure including buildings be doubled, the figure is brought down to £4 per kilovolt-ampere, which is a considerable saving. I think that underground cables are no doubt the most reliable. I have only had experience of two overhead transmission lines, and with fairly disastrous results during thunderstorms. The protection from lightning seems a matter which will require much attention. With regard to the cost of overhead mains, once the cable is placed under the street it is usually left there and forgotten. It stands for a long time without trouble, but with overhead cables one has storms blowing down the wires, and insulators getting broken, so that while the first cost is very much lower the considerable cost of upkeep may render the line unprofitable.

Mr. A. E. McColl : It may be of interest to give the figures per kilovolt-ampere per mile for a 33 000-volt transmission scheme which relates to a tie line between two power stations. The scheme involves step-up transformers and boosting equipment. The total cost was about £0·53 per kilovolt-ampere per mile. It was possible to estimate these charges with reasonable accuracy, as the major portion of the work had been tendered for at definite prices, and a considerable portion of the construction work has already been accomplished. These costs are made up roughly of 62 per cent for cable and pilot wires, including cost of laying ; switchgear absorbs about 4·7 per cent of the total cost ; jointing $2\frac{1}{2}$ per cent ; and transforming and boosting apparatus 31 per cent. This, of course, does not include any buildings required for housing the transforming equipment. Regarding the table of transmission pressures, the author refers to 66 000 volts as being the upper limit. It seems to me that so far the upper limit is in the region of 33 000 volts, and I believe that most cable-makers feel rather doubtful about cables operating at this voltage. With 33 000-volt cable the stress at the surface of the conductor is roughly about 25 per cent of the available strength of the insulating material, and the margin of safety is not such as we are accustomed to on the lower-voltage cables. If we adopt the higher pressures, the safety margin comes down because, unfortunately, for well-known reasons we cannot increase the diameter of the conductor so as to keep the stresses at the surface of the conductor equal. Consequently we have a decreasing safety margin as the pressure increases. Some time ago, I saw some tests on a 33 000-volt cable. These samples were tested to destruction, the pressure being raised to the vicinity of 200 000 volts. One interesting feature was made manifest, and that was the rapid accumulation of oil on the outer surfaces of the paper insulation, this oil apparently exuding from the inner layers of the insulating material. One explanation that appeals to me in connection with this phenomena is that each particle of oil is acted upon by electrostatic stresses and forced away from the vicinity of the conductor, and it seems to me that in a short period we should have the inner layers of the insulating material much drier than they should be. We know quite well that the strength of the dielectric lies in the impregnation and not in the paper itself. It would appear therefore that what is possible to accomplish, say, in a few seconds at extreme pressures, may be produced in the course of two to three years in a cable operating at moderate pressures. That is to say, the inner layer of the insulating material would dry out and lose considerably in insulating properties, with the possibility of failure of the entire cable. Proposals have been made to flood the cable from end to end with oil by erecting pumping equipment and pumping oil through the entire cable length. This would fulfil a treble function ; it would act as a cooling medium ; it would serve to maintain the impregnation wherein the strength of the cable lies ; and, in the event of fractures of the outer sheathing, oil would exude instead of water entering.

Mr. H. Allcock : The author foreshadows that we shall have at least three ranges of pressure, and it appears likely at no distant future that we shall have even more. There seems to me no escape from evolution along those lines, although it must complicate the distribution problem. Reference has been made in the discussion to extra-high-pressure cables, by which term I mean cables for 33 000 volts and upwards, and I was particularly struck by Mr. McColl's remarks. He rightly referred to the importance of complete and permanent impregnation of the paper dielectric on such cables. I agree with him that impregnation warrants very careful research, but I must confess that I am not yet prepared to support his suggestion that the continued flooding of the paper—after the cable is laid—will be the best solution. It appears to me that the most promising line of approaching this problem is to aim at greater uniformity of impregnation than could be secured by the mere flooding of the mass of paper in insulating oils. Those who have given detailed attention to this problem will recognize from these few words that I favour the more perfect impregnation of the paper *sheet by sheet* rather than *mass* impregnation by flooding. Reference has also been made to pressures higher than 30 000 volts, and it may be recalled that some years ago a claim was put forward that 60 000-volt cables had been produced by a Continental maker. On inquiry it transpired that they were not actually 60 000-volt cables, but 30 000-volt single-core cables

used on a 60 000-volt system. I mention this because the author referred to the possibility of using single-core cables for 60 000 volts, if 3-core cables for the latter pressure were not obtainable. As he indicated, there are many interesting problems to be solved in this direction, but great progress is now being made. I think we may sum the matter up, so far as cable manufacture is concerned, by saying that 33 000-volt cables are already an established product which can be used without qualms, and that 60 000-volt 3-core cables are an early possibility.

Mr. D. K. Coldwell : The utilization of condensers to improve the power factor and incidentally the distribution, is not mentioned in the paper. In small distribution areas static condensers could be used, whereas the engineer usually accepts the load factor as produced. I think the use of static condensers would be a great improvement, but rotary condensers at the present time are too costly from the point of view of upkeep and first cost.

Mr. T. I. Craig : The author refers to rising pressure, but would he be equally willing to use reduced pressure ? For instance, in an area like Yorkshire, there are numerous town-planning schemes being carried out, and I think most engineers must be uncertain as to how they are going to make those schemes pay. Would the author agree to a reduction of pressure to those smaller town-planning schemes, whereby we could get a cheaper form of heating and domestic appliances to secure a profitable load to justify us in giving them an electric lighting supply ?

[The author's reply to this discussion will be found on page 273.]

South Midland Centre, at Birmingham, 15 December, 1920.

Mr. R. A. Chattock : I am of opinion that the formulæ which Mr. Woodhouse uses throughout his paper are not at all easy of application to the problems involved. He adopts constants in these formulæ, but does not give any idea of the probable value of the constants that would apply in practice. I am quite aware of the difficulty of stating the exact value of constants of this kind, but I do feel that it would have been possible to work out one or two examples as an indication of how the constants should be arrived at. Having made this one criticism, I can say that generally I am in accord with the principles laid down, and I should like to emphasize specially the point Mr. Woodhouse raises in connection with pressures of supply. It is very obvious that, in a large distribution system, certainly three and possibly four pressures have to be adopted. In the Birmingham district we, of course, have first the low pressure that is applicable to individual consumers, then the 5 000-volt E.H.T. three-phase supply, which is given to large consumers and to substations, and is our pressure of generation. We are now proceeding to install trunk connections to outlying districts at a pressure of 30 000 volts and, whilst this is desirable and suitable for large-capacity trunk mains, it is too high for the smaller-capacity transmission lines which may be required for supplying the villages and agricultural districts within a radius of 10 or 15 miles from Birmingham. Underground cables rated at 30 000 volts have, for mechanical reasons, to contain a certain size of copper conductor, and this has the effect of limiting their minimum capacity to about 7 000 kW. Such a capacity is too great to lay all over a country district, and on this account it will probably be found advisable to adopt an intermediate pressure of 10 000 volts for this class of supply, and to confine the 30 000-volt cables for transmitting large-capacity supplies to well-defined centres. I quite agree with the author that alternative means of supply to individual consumers, and also to large centres, should be provided so as to ensure continuity of supply as far as possible. I feel, however, that interconnection of networks can be carried too far, and that, whilst the possibility of interconnection should be provided, the actual connection should be broken so that the networks may be split up into easily handled units. Where several generating stations are used, it is essential that these should be connected together by large-capacity trunk cables. At the same time, it is undesirable to connect together the centres of distribution external to the stations in a permanent manner, as, in case of a disturbance on the system, it is quite possible that the generating stations may be disconnected on the trunk cables, and that the connection on the distribution system may remain, with the result that the latter would become heavily overloaded, and the switches controlling such connection would be called upon to handle more than they were probably designed to do, with the result that they would very likely be seriously damaged. I have had an actual case of this happening in connection with the Birmingham network, where a switch panel was actually blown to pieces under these conditions. I also think that it will be found very desirable to provide ample reactances in the distribution system between the generating station busbars and each section of the distribution network. The enormous forces that have to be controlled from the busbars of a large supply system at any point where trouble occurs on the network require switches of very large capacity for the purpose, and these switches are very expensive items. In order to keep the expenditure on switchgear throughout the network, both in consumers' premises and in substations, within a reasonable figure, smaller type switches have to be employed, and the power that can get to these has to be limited by reactances. This control is also an advantage in taking a certain amount of strain off the transmission and distribution cables. The author confines himself solely to distribution of alternating current. It is, of course, a fact that most large industrial centres are supplied with direct current, and the advantages of direct current over alternating current, both as regards safety, flexibility of manipulation, and the general steadiness of supply, are well recognized. I should therefore like to remind Mr. Woodhouse of the possibility that has recently been

developed, of supplying direct current on as economical a basis as alternating current to outlying districts, by means of the mercury-arc rectifier. This apparatus can be installed at certain points along the line of an E.H.T. feeder, and can supply direct current into low-tension distributing cables in the same way that a static transformer supplies alternating current into similar networks. The heavy cost of low-tension feeders from transformer substations is done away with, and also the heavy cost of operating such substations. The method has been developed commercially in Switzerland, and is, I understand, giving every satisfaction. In Birmingham we have a rectifier on order to experiment with, with a view to developing the supply in the outlying districts on these lines if possible. I also have every hope and confidence that this method of transformation will be ultimately applied to railway electrification. It would be possible to do away with the heavy cost of rotary converter substations and low-tension feeders in connection with railway supply in the same way, and the same thing would also apply to tramways. The apparatus is being used in Switzerland for tramway supply, with, I understand, very great success, and I am proposing to experiment with it in Birmingham on the same lines. I should be very glad to know if Mr. Woodhouse has considered this method of distribution, and if he can give us any information about it.

Mr. W. Wilson: There are two small suggestions which I should like to bring forward with regard to the paper. One of those is that the lower curves in Fig. 1 of the paper might be reproduced in connection with the upper set of curves, perhaps by dotted lines, in order that these may afford a complete comparison at present-day prices. The other suggestion is that already made by Mr. Chattock, which I should like to support, as I think that a few numerical instances would make the paper more interesting for those who have not the same close association with the subject that the author himself possesses. I cannot help feeling how great a handicap it is that present-day conditions in this country so greatly hinder the carrying out of reticulation in accordance with the excellent principles laid down by the author. To my mind the arguments in favour of bold planning when the provision of an electric supply is being considered are far greater to-day than ever before, for we are now in a position to realize the great disadvantages under which we are labouring through the host of small and conservative schemes that are doing duty over so much of the field, and that by their very existence are hampering the inauguration of a more extensive and economical system. Now, electric supply enterprises may be divided into two classes. The first of these is that of a district wherein a demand for electric power has grown up, which has been partially satisfied, after a considerable time has elapsed, by a purely local scheme that manages to keep a number of years behind the requirements throughout the whole of its career. This is an example of what I may call human nature in its cautious mood. The second class is exemplified by most hydroelectric enterprises. The average man with money to invest, who comes face to face with a waterfall, or even a rapidly flowing river, immediately asks himself why all this power is permitted to go to waste, and begins to make plans for putting capital into a development project without making any rigid inquiry as to the existence of a payable demand. This shows us human nature in its rash mood. When such a scheme is carried out, from its very nature it is undertaken on a generous scale, and it is hence designed with every chance of being economical. Now the point that I am anxious to bring out by this comparison is that the "rash" method always proves to be the correct one. Although there are critics of every hydroelectric project, who confidently declare that operations are being undertaken on too large a scale, yet I have never heard of a case where such a scheme failed on account of too small a demand; and, given the demand, there is no doubt as to the economic success of the undertaking. It seems to me that at last, in the provisions of the recent electricity legislation, there is about to come into being an organization which will counteract the unfortunate tendency to excessive caution that has been only too marked a feature of our power schemes of the past. If the intentions of the Government, as declared by them at the outset, are not prevented from being carried into practice, there can be little doubt that a great day is dawning for electrical distribution, to the great advantage of the profession as a whole. It is to be devoutly hoped that this state of things will occur, and that it will therefore be possible to supply the country with the electricity it needs by the unfettered application of the principles which the author has described.

Major A. M. Taylor: With regard to the heating of cables, I anticipate that I, in common with most of the other members, have until recently assumed that the rotating field due to a three-phase current is not of any serious consequence in inducing eddy currents in the lead sheathing. I now believe, however, as a result of some experiments which I may announce, that the losses in the lead sheathing due to the rotating field are very much greater than was generally realized and far outweigh those due to hysteresis. It seems to me that, by the methods of measurement adopted by electricians for measuring the losses in their cables, it is difficult to discriminate between those due to hysteresis and those due to eddy currents in the lead sheathing. The results obtained by wattmeter measurements could only have this effect. The experiments to which I allude were made at a frequency of 25 cycles, and the principle adopted was to take two pieces of identical cable, but in the one case armoured and in the other case unarmoured, to pass identically the same current through them, and to compare their relative rises of temperature over such a period of time that practically constant temperature was obtained. Direct currents were first passed through the cables in series and the temperature-rises noted. Under these conditions, it will be obvious that no eddy-current losses or hysteresis losses could possibly occur. This condition represents the minimum temperature-rise. The next experiment was to pass through the two pieces of cable, in series, a three-phase current of such a value that the I^2R losses in copper were identical with those

obtaining with the direct current. The third experiment consisted in passing a single-phase alternating current through the two cables, arranged to give the same I^2R losses as in the case of the direct current. This last experiment, compared with the previous experiments, gives a means of differentiating the eddy-current losses in the lead sheathing due to the rotating field from those due simply to the pulsation of the current. It will be understood that the magnetic axis of the rotating field lies on a diameter of the cable, and the rate of rotation of the field is controlled by the periodicity. The axis of the field being, as stated, diametral, it follows that the armouring of the cable forms a path for the magnetic lines which is normal to the direction of magnetic flux, and that, therefore, the armouring has practically very little effect upon the strength of this rotating flux. In the case of the single-phase current, however, the armouring lies absolutely upon the path of the magnetic flux, and consequently very greatly affects the strength of this flux. This explains the difference that will be observed between the behaviour of the single-phase current in the armoured cable and in the unarmoured cable (see the table of tests below). It may be added that there was no phase currents in the case of long-distance transmission, where the improvement in maximum voltage gradient is effected by the use of single-phase cables, on the lines indicated by Messrs. Clark and Shanklin (in their paper before the American Institute of Electrical Engineers, in June, 1919); also that the losses with direct currents are the ideal towards which we should aim, and which can be reached within $12\frac{1}{2}$ per cent by using single-phase cables. It is also to be expected that, with a current at 50 periods, the losses in the lead sheathing, with single-phase cables, will be distinctly higher than with 25 periods. This, if corroborated (and experiments are being made with this in view), should indicate the desirability of 25 periods for extremely long-distance transmission. It is obviously not in the best interests of the maintenance of insulation of E.H.T. cables that there should be an unnecessary loss in the lead envelope mounting to 46 per cent of the copper loss. It only remains to add that, in order to emphasize the results, the current densities were raised to about 1 500 amperes per square inch in the three-phase tests. This, however, is not in excess of that indicated by Kelvin's law at voltages between 20 000 and 30 000.

Experiment No.	Nature of Current	No. of Cores Utilized	Current per Core	Class of Cable	Rise of Temperature		Percentage Rise above Direct Current (taken as Ideal)		Periods per Second	Hysteresis Losses
					Armoured	Unarmoured	Armoured	Unarmoured		
			Amps.		deg. F.	deg. F.	%	%		
1	Direct	1	400	Three-phase 6 600 v.	23·0*	23·7	Nil	Nil	Nil	Negligible
2	Three-phase	3	230	Ditto	32·0	38·0	39	58·5	25	Ditto
3	Single-phase	3	230	Ditto	47·5	27·0	106†	12·5†	25	Ditto

* Later experiments show this figure to be too high.
† The last two results should probably be divided by 3, as the field produced was that due to 690 amperes (aggregate).

Remarks.—In experiments 1 and 3, the thermometer was tried at different points around the circle, to check any unequal heating.

opportunity of hysteresis occurring in these tests, which were carried out at a very low voltage. The three principal points of interest deducible from these tests are: (1) With three-phase current, the total losses in the armoured and unarmoured cables are not greatly dissimilar. (2) The losses in the unarmoured cable with three-phase currents are $58\frac{1}{2}$ per cent greater than with direct current. (3) The losses in the unarmoured cable with single-phase current are only $12\frac{1}{2}$ per cent greater than with direct current. Since the result obtained in the case last mentioned is included in the $58\frac{1}{2}$ per cent for the three-phase losses, it follows that, of the $58\frac{1}{2}$ per cent above given, no less than 46 per cent is due to the eddy-current losses as caused by the rotating field, while only $12\frac{1}{2}$ per cent is due simply to the pulsations of the field. From these results I venture to submit that it is readily deducible that a very great saving in the eddy-current losses in the lead sheathing can be effected by resorting to single-

Mr. W. E. Groves: The correct practice in the lay-out of a power-distribution system always presents great difficulties, and the present costly condition of things renders it still more difficult. Formulæ have therefore to be applied with caution, but this does not mean that rule-of-thumb methods are to be employed, and we are much indebted to the author for bringing forward the various points for consideration. The time factor is of the very greatest importance. With labour costs as they are at present, it is more than ever necessary to take a long view. With some classes of cable of small size laid underground, labour costs may nearly approach the cost of cable; ample provision must therefore be made when the ground is open. The difficulties of taking a long view and a bold course are not so great where developments take place from an established and remunerative nucleus, but where entirely new schemes have to be embarked upon there must be some trouble in getting under way without

outside assistance. In any case a step-by-step development is necessary. In opening up a new area, a ring main should be laid linking up the most attractive centres. This would suffice for pioneering purposes, and the next step would be the laying of further mains cutting up the original ring, again taking the most promising routes. When this arrangement no longer suffices, super-tension transmission mains should be taken to the centres of gravity which developments have revealed. Alternative means of supply are essential, and with ring mains it is necessary that the cable should be capable of taking the total load on the ring from either end, whether the ring be closed or not, in order to provide for any section being out of use. Where considerable distances have to be covered, the cost of pilot cables for Merz-Price or similar protection is prohibitive. The sectional switches on a ring main should be non-automatic, and protection limited to overload and leakage protection at either end of the ring, consumers' installations being equipped with overload and no-volt trip-gear. As regards the pressure of low-tension distribution, the local standard of 100 to 250 volts is mentioned. The British Engineering Standards Association recommend 415 volts between phases and 240 volts to the neutral. Is it not time that there should be agreement as to standards for new developments? Opinions have been expressed in various quarters that the pressures recommended are too high, and it has been proposed to reduce them for reasons of safety and to permit of cheaper house wiring. This would obviously increase distribution costs. If it were not so, the lower pressure would certainly be preferred. The author deals exclusively with alternating-current distribution. Its adaptability renders it very attractive, and its conversion or rectification where it is imperative must be regarded as an "expensive necessity." Further information respecting the maintenance of overhead systems would be very acceptable. The cost of upkeep of pole lines must greatly exceed that of underground mains, which with modern cables is small and can be easily estimated, and it would seem that a fourth term, representing the capitalized value of maintenance, should be added to the analysis of costs of overhead systems. It is of course realized that, in some districts and for some uses, overhead lines are essential and must hold the field. The importance of the shape of the load-factor chart is emphasized, and the ideal should be borne in mind during the stages of development, tariffs being arranged accordingly. The section of conductor for high-pressure cables is limited by mechanical as well as electrical considerations, a very heavy dielectric requiring a substantial core. In considering the conditions of minimum cost per kilovolt-ampere per mile, the limitation of the application of the rule relating to increase of cost with pressure is soon reached on account of this increment being so small in comparison with the other items of cost which include labour. It is probable that more information will shortly be available respecting dielectric losses in their relation to other cable characteristics, but is is not surprising that there should be some reticence when active research in this direction is in progress.

[The author's reply to this discussion will be found on page 273.]

North Midland Centre, at Leeds, 25 January, 1921.

Mr. J. E. Storr: The author refers on page 86 to the time limit for development. In normal areas of electricity supply, say of a few hundred square miles, as distinct from the interconnected areas of the whole country, there is a very definite limit beyond which it is inadvisable, for financial reasons, to plan too far ahead. The uncertainties as to the line of development are great in practice. If the ideal area to be supplied existed as outlined on page 91 of the paper, it would not be difficult to decide upon the best course to pursue. In considering the development of distribution in areas referred to above as normal, the time limit involves an important point in that the first lay-out need not cover the whole of the final cross-section of copper required between any two principal centres. First economies may be practised with due caution, and the final cross-section of copper be made by following an alternative route, tapping new load centres perhaps several years after the first section is laid. Referring to the cost of substations, these are not necessarily centres of transformation only. In a comprehensive system of distribution switching stations are required and should be planned to group together the loading of any circumscribed area to a main feeding point or switching centre. These centres are valuable adjuncts to a distributing system of mains, not only for load grouping but for simplified protection, and the economy in expenditure on mains which results in feeding branch lines from such centres more than pays for the cost of the switch station and its apparatus. In such arrangements a single cable is sometimes ample provision on a branch and can often be of small cross-section. It is suggested that these switching centres judiciously placed should have full consideration in schemes of any magnitude for the distribution of electricity.

Mr. T. B. Johnson: The author says: "The concentration of generating plant in a small number of stations involves greater expense in distribution, and is only justifiable so far as the advantages of centralization outweigh the extra costs of distribution which result." With the alteration of a few words that would exactly describe the problems telephone engineers have in large cities in deciding whether they shall put up one large exchange for the whole city or have a number of sub-exchanges on the outskirts. Although the telephone distribution is not the same as power distribution, inasmuch as the former is necessarily of a tree form whereas the latter may be described as being of web design, yet the considerations are very similar, as we have to enter into rather elaborate calculations, and a good deal of surveying has to be done in order to

decide whether a sub-exchange should be put down in a particular area or not. In addition we have our own questions of density for, whereas the bulk of the conversations are local and consequently wires of very small diameter will suffice, we have also to provide for the junction and trunk traffic which requires telephonic efficiency of a much higher order. I am particularly impressed by the fact that the author emphasizes the need for a considered lay-out for the distribution of power, and, indeed, I think he might have gone a little further and said first of all how desirable it is that the station itself should be selected after a consideration of the area. I agree that one cannot get just the ideal areas one would like and that there are important considerations which limit the choice of stations, but we are in contact with similar problems in a different area; for instance, the Hull telephone area extends for several miles north, east and west, whereas it only extends a little way to the south because it meets the Humber. Allowing for those physical difficulties, there is great necessity for the study of the location of the station. To begin with we find that if we erect one central station instead of a number of substations, or *vice versa*, the capital spent at the beginning with a large unexpired life makes it almost impossible to put affairs right afterwards. For instance, when we put up the automatic exchange in Basinghall-street we took Ordnance maps, on which were plotted the location of every subscriber in Leeds, and drew one horizontal and one perpendicular line not in the middle of the paper but so that we had as many stations on each side of the perpendicular and exactly as many above as below the horizontal line. Obviously where those lines crossed was the place where we could put the Central Exchange with the least expenditure of labour. We then found that it would not do to put in cables without a very thorough understanding of what we were going to do ultimately, and before we entered upon the main scheme we plotted out on Ordnance maps the probable requirements of telephones within periods of 5, 10, 15 and 20 years. So that, although we may only carry out a preliminary part of the work at the time, we are subject to capital cost; we know at all events what we are working for and it is a comparatively easy matter after that to make alterations due to the unexpected development of one part, or the under development of another part, and it goes without saying that we provide cables for a very much shorter period than we provide ducts. We see that we have cables enough at all events for 5 and usually for 8 to 10 years, whereas we provide ducts for at least 15 years. We reckon that beyond those periods the interest and other maintenance charges would counterbalance the cost of adding cables when the time comes. To a trained man it is comparatively easy to make a very reliable estimate of the development in any particular part within those periods, and I believe that power-supply undertakers also find that they can make a fairly accurate forecast of the requirements in a somewhat similar way. That also applies largely to the class of cable used between the central station and the substations. In that connection the paragraph on page 93 is of interest: "It would seem reasonable therefore to have regard to the probable demands at the end of a period of years determined by the rate of growth of the load," and to make arrangements accordingly. I think that the Institution might well devote a good deal of its power to the necessity of arranging areas on an intelligent basis. I know that there are immense difficulties in the way, but the advantages of arranging the area properly to begin with are so great that it seems worth while to do a great deal in order to obtain that result. If, for instance, we are going to make use of water power, and naturally we all look with very great interest on the scheme which has been proposed for the Severn, the area certainly ought to be grouped round that source of supply, even if local jealousies stand in the way. The scheme is so important that it ought to be possible to overcome those, and if we once get away from the economic centre it seems almost impossible ever to get the thing put right. I would emphasize the importance of the preliminary survey both as regards the area, the location of the station, and the question whether there shall be any, and if so how many, substations. There should be an intelligent knowledge of what the station is going to serve for a good many years ahead, and the work should be performed with that object in view. If that be done, then, with all the data which the author has given us, the path of the electrical engineer will be smoothed considerably.

Mr. F. S. G. Hinings: I think it is a pity the author has not brought the curves for 1907 in Fig. 1 up to date, because as they stand they are really of no value for comparative purposes. I should like to know what the up-to-date cost of a 22 000-volt cable will be, so that we can make a comparison with the higher voltages. I imagine the dielectric losses on a low-pressure system can be neglected, but that with very high pressures they have to be taken into account. The author says that the resistance losses vary with the load. Should it not be the square of the load? Even that might be slightly increased, on account of the temperature of the cables. I feel sure most mains engineers will agree with the author when he tells us that 0·05 sq. in. should be the minimum section of a cable for pressures above 10 000 volts, but I know of individual cases where cables of smaller cross-section than 0·05 sq. in. are in use. The ones I have in mind are of 0·035 sq. in. section on an 11 000-volt system. These have given years of good service without any apparent disastrous results. I should like to know what are the limits of regulation of pressure permissible at the stations. It seems to me that a fairly definite pressure must be maintained at each station for the sake of the local supply areas. This will necessitate the use of some form of regulating booster equipment on the tie lines.

Mr. C. I. Shuttleworth: I can answer Mr. Hinings's question as regards cost. I have recently been interested in a proposal for transmitting from 2 000 to 3 000 kW about 12 miles. The estimated approximate cost of 22 000-volt cable and laying, including pilot cable, etc., was about £80 000, and the cost of switchgear and transformers for stepping up and stepping down was approximately £20 000. The whole scheme in

round figures would cost about £100 000. The other day, as a matter of private interest, I reckoned up what the transmission cost in standing charges would be, allowing 10 per cent interest and depreciation. I estimated that at 100 per cent load factor the feeder would transmit, say, 40 000 000 units per annum. That would represent a cost of 0·06d. per unit. With an average figure of 10 000 000 units, equivalent to 25 per cent load factor, the cost per unit would be four times that, or 0·24d. The cost of transmission is becoming a very serious factor with the present cost of cables and laying. I agree with the author that to lay light cables is a fallacy, as the cost of labour is such a big proportion of the cost of the cable. The size of the proposed cable was 0·15 sq. in. at 22 000 volts. My experience of 6 000-volt transmission is that it does not carry far, on account of pressure regulation. I have in mind a 6-mile feeder. For two-thirds of the distance a 0·15-sq. in. cable is used; for the remainder 0·075 sq. in. It is found in practice that, with an approximate load of 300 kW at the consumer's premises at the end of the line, and with the cable fully loaded at the power station and tapped off at intervals on the way, there was as much as 15 per cent variation. At six miles the economic limit for 6 000 volts is being exceeded, and in the aforesaid scheme the choice was between 11 000, 22 000 and 33 000 volts. Finally, both as regards cost and reliability 22 000 volts was thought to be the most suitable pressure.

Mr. L. E. Wood: I should like to ask the author if he does not think that the overhead system of mains on the steel-clad aluminium principle would be much more economical if induction-type pressure regulators were used on the system, as is done in Canada and the United States. In a country like this I should imagine that they would be much more reliable than an untried extra-high-pressure underground cable. It seems to me that the overhead cable is not used enough in this country.

Mr. C. J. Jewell: I should like to offer one suggestion in connection with the economics of setting out the distribution system. I have found many cases where for the sake of a few pounds in the cost of site, those who have chosen the system have turned away from the main cable route and put their substations either in back streets or in places somewhat remote from the route which the cables have followed, and incurred an avoidable expenditure on cables in some cases amounting to a considerable sum. It must be obvious that in towns enough money can be wasted on cables to provide the cost of a frontage on to a fairly busy street. This is more especially the case when erecting substations for tramway systems, where usually there are many low-tension feeders besides the high-tension feeders. I am quite in agreement with the author's idea of building up a system in such a way that account is first taken at all times of the load in sight, so as to avoid capital lying idle before the output reaches the paying basis. In most distribution systems to-day it will be found advantageous to commence distributing to substations at the voltage of generation, namely 10 000 volts, and, when a load is increased to a sufficiently large figure, the new mains can be laid up to 30 000 volts or more as the manufacture of these high-tension cables progresses, and tappings at 10 000 volts can be led to the substation.

Mr. W. H. Wraith: The author was the first in this district to use a working pressure of 11 000 volts, and I think we can say that it has served him very well. In other parts of the country a working pressure of 33 000 volts has been in operation for some time, and I hope that in due course we shall be able to obtain cables for 60 000 volts, as in the scheme which he has laid out.

Mr. W. B. Woodhouse (*in reply*): For the sake of brevity I propose to summarize my replies to the various discussions, more particularly as many of the points raised were answered by others and as the principles enunciated in the paper have not been generally challenged. Many of the speakers who expressed an apparent disagreement will find, I think, that their points have been met in the paper.

The economic transmission pressure is not determined by a consideration of the cost of mains only; the criterion is the cost to the consumer, which includes considerations of generation, transformation and distribution. The author has no preference for a high or a low transmission pressure *per se*; the problem under consideration was that of the most economical pressure, which for underground cables is shown to be the lowest pressure satisfying the conditions enumerated.

Some confusion seems to exist as to the purpose of transmission mains interconnecting several stations of a common system. These mains serve a dual purpose, namely, the transfer of load from a heavily loaded station for the purpose of security, and the transfer of load from a lightly loaded station for the purpose of economy. The first transfer can most economically be made by the introduction of intermediate feeding points, so reducing the expenditure on distribution mains. The objection to intermediate feeding points seems rather a matter of prejudice, as, in the case of more than two interconnected stations, one at least is an intermediate feeding point at certain times.

The use of overhead lines is essential to economy; I think that those engineers with the greatest experience of them most fully appreciate their value, but if power stations are situated in large towns the use of overhead transmission lines becomes impossible.

The expression on page 92 for the maximum rating of 10 000-volt cables was based on a temperature-rise of 50 degrees C.; in view of changed cable design it may now be necessary to revise it.

The equation of cost of cable has been challenged; it agrees with the author's experience and has a definite and logical basis. It may be suggested that discrepancies of price from such a relationship will repay examination by those who meet with them.

Several speakers assumed that the author's reference to proportionality of cost implied a constant ratio of total cost to pressure or section as the case might be. This was not intended, as a reference to page 87 will show.

The load factor of copper losses, it has long been known, differs from the system load factor, and I imagine that neither Mr. Beard nor I would wish to claim novelty for the statement.

I do not find in Mr. Beard's paper which has been referred to in the discussion a general expression such as is set out in my own and which I think is novel. Mr. Ratcliff's expression for the same quantity is also new; it gives rather lower values.

The financial side of the problem has been emphasized by some speakers and dismissed lightly by others. It would be unfortunate for the taxpayer and for the industry if doles were depended on for development. The principles on which the electricity supply industry is founded are sound and the industry should be able to stand on its merits. The rate of progress of electricity supply is already very rapid and any artificial stimulus might easily prove disastrous.

PROCEEDINGS OF THE INSTITUTION.

12TH MEETING OF THE WIRELESS SECTION, 15 DECEMBER, 1920.

(Held at the Institution of Civil Engineers.)

Dr. W. H. Eccles, Chairman of the Wireless Section, took the chair at 6 p.m.

The minutes of the meeting of the Wireless Section of the 24th November, 1920, were taken as read, and were confirmed and signed.

A paper by Captain R. Chenevix Trench, entitled "Range of Wireless Stations" (see page 157) was read and discussed, and the meeting adjourned at 7.50 p.m.

656TH ORDINARY MEETING, 16 DECEMBER, 1920.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 9th December, 1920, were taken as read, and were confirmed and signed.

The President announced that the Council had unanimously elected Professor André Blondel an Honorary Member of the Institution.

The President also announced that Mr. E. M. Hughman (Associate) of Bombay had undertaken to give £250 to the Benevolent Fund if, before the 31st March, 1921, 19 donations of a similar amount were received.

A "Report on the Heating of Buried Cables" (see page 181) made to the British Electrical and Allied Industries Research Association by Mr. S. W. Melsom, Associate Member, and Miss V. Cockburn, Mr. E. Fawssett, Member, and Professor E. W. Marchant, Member, was discussed, and the meeting terminated at 7.50 p.m.

657TH ORDINARY MEETING, 13 JANUARY, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 16th December, 1920, were taken as read, and were confirmed and signed.

The President announced that the Council had considered what steps could be taken to encourage research workers and inventors to bring their discoveries and inventions to the Institution for publication, and had decided to invite, for publication in the *Journal*, papers describing discoveries and inventions. Such papers would be given priority with a view to early publication, and the Council were of opinion that papers of this kind would considerably enhance the value of the *Journal*.

A list of candidates for election and transfer, approved by the Council for ballot, was taken as read and was ordered to be suspended in the hall.

Sir William Bragg, K.B.E., F.R.S., then delivered the Twelfth Kelvin Lecture, entitled "Electrons" (see page 132). A vote of thanks to the lecturer, proposed by Dr. A. Russell and seconded by Mr. C. C. Paterson, O.B.E., was carried with acclamation, and the meeting adjourned at 7.15 p.m.

658TH ORDINARY MEETING, 27 JANUARY, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 13th January, 1921, were taken as read, and were confirmed and signed.

Messrs. A. T. Dover and P. J. S. Tiddeman were appointed scrutineers of the ballot for the election and transfer of members, and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Fulton, David.
Head, Howard Clark.
Huddleston, Johnson Snow.
Moore, George James.

As Associate Members.

Anson, Bernard Oglie.
Arundel, Daniel Sim.
Ashford, Walter.
Atkins, Francis.
Ballard, Thomas James.
Banks, Arthur Edward.
Bannister, George William.
Barker, Arthur James.
Bassett, Sidney William.
Baxter, Edwin Harry.
Baxter, Ernest James.
Beattie, William.
Bectlestone, Mark Arthur.
Beeton, Herbert Greenwood.
Bell, Frank Norton.
Bell, John Hope.
Best, Frederick William.
Blick, Frank.
Bolton, Douglas Joseph, B.Sc. (Eng.).
Bramwell, John Thomas.
Brown, James.
Buchanan, John.
Bullock, Alfred Harold.
Cameron, John.
Cardrey, Ashby George.
Carter, William Gilbert, M.C.
Cawood, Sidney Dalton.
Chandler, Albert Edward.
Cheetham, Walter Bryan.
Clack, Charles William.
Couch, John Mackinnon.
Couchman, Charles John.
Cowling, George.
Croker, Frederick Stanley.
Cunningham, Richard.
Dalston, John Frederick F.
Davey, Frank William.
Dipple, Herbert Wallis.
Dorey, Edgar Wall.
Drewitt, Charles John P.
Elston, John Sissons.
Escott, Hercules.
Evans, Lennard Melville.
Faulkner, Harry, B.Sc.
Fewtrell, John Wellington.
Field, Albert William.
Finlayson, William Johnstone.
Fleming, George.
Gardner, Alfred Welch.
Gear, William Joseph.
Gilbert, Dinely Prior.
Giles, Henry Walter.
Gillespie, James Taggart.
Glover, John Bell.
Gray, Hubert Washington, B.A., B.Sc.
Greenstreet, Robert.
Grocott, John Reginald.
Gundry, Walter Ernest.
Gwennap, William.
Hague, Bernard, M.Sc.
Hansford, Richard Vernon, B.Sc. (Eng.).
Hardie, James John.
Harris, Edward Clifford.
Haywood, Charles.
Hick, Edwin Arney.
Higson, William.
Hill, George Job W.
Hill, Sydney Frank.
Hines, John Gerald.
Hook, George Henry J.
I'Anson, Robert.
Innes, George.
Ireland, William.
Jeary, Leonard Gordon.
Jenkins, James Brinley.
Johnson, Alfred Samuel A.
Jones, Joseph.
Jones, Leonard Joseph.
Jupp, James.
Kemp, Harry.
Kennard, Ernest Gerald.
King, Robert Titlow.
Lakey, Ernest Alfred.
Lancaster, Thomas Stephenson, M.C.
Lawes, William Horace, B.Sc.
Lawson, Robert Johnston.
Leigh, James Henry.
Lemont, George Andrew.
Lewis, Ernest Albert.
Linsell, Frederick Albert.
Lock, Frank.
Lomas, Walter.
Luke, John Pearce.
McClarence, Francis.
McConnell, John Albert.
McCormack, William.
McGillivray, John.
McGregor, John Ernest M.
McKichan, John James.
MacKintosh, Alexander.
Maddock, Gerald Maxfield.
Mairs, James Baillie.
Malyon, Frederick Wilson.
Markwick, John Jabez.
Marshall, Charles William, B.Sc.
Milnes, John Robert.
Modrak, Peter, B.Sc.

As Associate Members—continued.

Moller, Oscar Percy.
Monaghan, Thomas Joseph, B.Sc. (Eng.).
Morice, Arthur Beaumont, B.Sc. (Eng.).
Morice, Lewis Fry.
Myles-Hook, Reginald.
Newell, Alfred Vincent.
Newton, Edwin Isaac T.
Nichols, Arthur Robert.
Norton, Henry Joseph.
O'dell, George Frederick, B.Sc. (Eng.).
Paddon, Albert Frederick.
Palmer, Arthur Herbert.
Phillips, Frederick John.
Pinching, William Benjamin.
Ponsford, Septimus.
Pooley, Reginald Emmerson.
Pratt, Arthur James.
Price, Leonard.
Radford, John.
Ratcliffe, Charles Plummer, M.B.E.
Redman, Christopher.
Reinold, Basil Edward.
Richardson, James.
Richardson, Thomas.
Roberts, Percy Alfred.
Robertson, John.
Rolfe, William John.
Rose, Hugh Edwin.
Savory, David Charles.
Schofield, George Harry.
Scott, Arthur.
Scott, William Robert.
Sharpley, Alfred John.
Shepherd, George Mervyn B.
Sirett, Albert William.
Sirett, Charles Joseph.
Speight, Alfred.
Stevenson, William.
Stewart, Joseph Henry.
Stone, Henry Charles.
Tabor, Henry John Harrison.
Taplin, Arthur Harold.
Taylor, John Lyon.
Thomas, Sydney, B.Sc.
Trew, Richard James F., Lieut.-Col., R.E.
Upton, Stephen.
Ward, William Lawrie.
Warren, Albert Edward.
Watson, David Walker.
West, George Edward.
Whitehead, James.
Wiggins, Frederick.
Wilby, Ernest John.
Wildgoose, George Henry A.
Wilson, Rowland.
Wood, William Killick.
Woods, Ernest James.
Wootton, George Arthur H.
Wynne-Jones, Arthur.

As Graduates.

Badhnivalla, Jehangershaw Nanabhai.
Barnacle, Alfred B.
Bloore, Thomas Addison.
Browne, Arthur William.
Brown-Stephens, Charles.
Bryan, Francis Charlton.
Buckley, Charles Wilson.
Bulow, Vincent Alfred M.
Cadman, Horace Bert.
Cameron, William Charles.
Chester, Clifford Ernest, (B.Eng.).
Cobbold, Geoffrey Wyndham N., B.A.
Cooper, William Robertson.
Dunbar, Leslie.
Easter, Herbert Arthur.
El-Sayed, Mohamed Amin.
Fawcett, Frank Nelson.
Ganguly, P. R.
Habgood, Edwin Valentine C.
Holroyd, Albert.
Hunter, Harley Kidd.
Jackson, John.
Jackson, Stanley Bowen.
Jarvis, Frank Ernest.
Johnstone, Alexander Bernhardt.
Jupe, Henry Arthur.
Leng, Charles William.
Lockwood, Henry.
MacEwan, Lewis Barclay, B.Sc.
Narayanan, Ramaswamyaggar Lakshmi, (B.Eng.).
Oswald, John Stevenson L., B.Sc.
Palmer, William George.
Rau, Karkul Madhava, (B.Eng.).
Robinson, Oswald Dennis.
Sampson, Henry.
Scott, Thomas Henry I.
Simmons, Thomas.
Thompson, Samuel Henry.
Timberlake, Eric Johnson.
Wadsworth, Thomas.
Williams, James Harold.
Wilson, Theophilus.

As Students.

Ablett, Leslie.
Adam, James Easton.
Addison, Joseph.
Allan, George.
Allen, Charles William.
Allen, Frederick Villetange.
Allen, Nora (Miss).
Allen, Robert James.
Ambrose, George Stanley.
Amis, Frederick Henry.
Anderson, Edwin.
Anson, Horatio St. George.
Armstrong, John Marshall.
Armstrong, William Henry.
Arnold, John Frank.
Arthur, Richard Henry.
Axford, Norman.
Badcock, Stanley.
Bailey, Henry Preston.
Banks, Percival.
Banks, William Quarmby.
Banner, Edgar Harold Walter.
Bannister, Harold.
Barker, John Webb.
Barnes, Herbert Bernard.
Barnett, Stanley.
Bartlett, Oswald Willoughby.
Base, George Cecil.
Bayley, Godfrey George.
Beale, Herbert Rene.
Bell, John, B.Sc.
Bell-MacIntosh, George Godfrey.
Bennett, Henry Percy.
Bennett, Stanley Richard.
Berkeley, Julius John C.
Best, Charles Harold.
Bird, Donald Egerton.
Birks, Kenneth William.
Birtwistle, Frank.
Bland, Stanley Ernest.
Boden, Wilfred.
Bolton, Leonard George.
Boucher, Frank Reginald.
Boucher, George Albert.
Bowden, Cyril Kenneth.
Brent, William Henry.
Bridgett, Harold.
Brown, William Herbert, Junr.
Bryan, John Henry D.
Burns, Desmond Gerald.
Burns, George Arthur.
Butcher, James Martin.
Byrne, Francis.
Cain, James Frederick.
Cape, Albert Bernard.
Carrott, Herbert Edward.
Chandler, Wilfred Hardinge.
Charles, Norman Henry.
Charman, Cyril Edward.
Chau, Iu Nin, B.Sc. (Eng.).
Chawner, John Clement.
Christy, Frank Fell.
Collings, Eric.
Cook, William John.
Cooper, Eric Raymond.
Cooper, Jack Albert.
Couzens, Reginald William H.
Coveney, Alexander Joseph.
Cox, Robert Stuart W.
Cramer, Derek Henry.
Crook, Edward Howard.
Crowder, Samuel Goddard.
Cruttwell, Colin Edward.
da Costa, Frederick Anthony V.
Dale, Arthur Clifford.
Dallow, Henry George.
Daniel, Thomas Ernest.
Dann, Thomas Walter.
Davey, Edward Leslie.
Davies, Arkles Joseph.
Davies, Edwin Charles.
Davies, Tracy R.
Davis, George Cedric C.
Dawson, John Armstrong.
Dawson, Roger Leedham.
Dhar, Ashok Kumar.
Dickie, William Henry.
Dixon, Dermot Henry J. P.
Dixon, Robert.
Dobson, Cecil George.
Dobson, Cecil Michael.
Dobson, Walter.
Downing, Herbert Edward.
Duncan, Norman McIntyre R.
Dupere, Thomas Rex.
Dutta, Sisir Coomar, M.Sc.
Duxbury, Henry Saxon.
Easton, William Ronald.
Eaton, Anthony Denzil M.
Elcock, Rowland Edward, V.C.
Ellis, Frank.
Ellison, Henry Havelock.
Emery, Arthur Joseph.
Fagan, James Arthur.
Farbridge, Walter Reginald E.
Fassnidge, Arthur Edward, Junr.
Fehr, Paul Edmund.
Ferguson, John Robert B.
Field, Norman Samuel.
Fielding, George Frederick.
Flax, Samson, B.Sc. (Tech.).

As Students—continued.

Fleming, David Crawford.
Flight, Theodore Nicholas.
Flitcroft, Frank.
Ford, William Henry.
Fowler, John.
Francis, Eric Mayland.
Francis, Morgan Rees.
Gammer, Louis Constable.
Gandhi, Khan Chand.
Gardiner, Robert.
Geis, John Edward.
Getty, Cyril Bernard.
Ginno, Sidney Charles.
Glen, Alexander Ernest.
Glover, Kenneth George.
Goldstein, Stanley Howard E.
Gooch, Leslie John.
Gould, Gerald H.
Gracie, James Johnstone.
Graham, John Anderson.
Grant, Douglas Frederick.
Gray, Charles Gordon.
Greaves, Reginald Douglas.
Green, Douglas Eustace.
Green, Thomas Edward.
Greenaway, Arthur Ronald.
Gregson, William Herbert.
Gresham, Samuel Roland.
Gupta, Bahadur Singh, B.Sc.
Haddow, Alexander.
Hallawell, Austin Morris.
Hamson, Paul.
Handley, George Joseph.
Hanks, Cecil Victor.
Hanney, Douglas Gordon.
Hanson, Ernest.
Harber, Frank Ollerenshaw.
Hardy, Frank.
Harvey-Barnes, Samuel Edward.
Hebditch, Edward Gladstone.
Hewett, Theodore Leonard.
Hillman, Reginald Victor.
Hodgson, Charles Henry.
Hodson, Donald Arthur P.
Holley, Edgar George.
Holmes, Sydney Jackson.
Holt, Thomas.
Horrell, James Walter.
Hoyle, Herbert.
Hudson, Fred James.
Hutchinson, Frederick John.
Hyams, Lewis Gerald.
Jackson, David.
Jackson, Horace.
Jaffrey, John Alan.
Jago, Ernest Harry.
James, Harold Frederick.
Johns, John Percy.
Johnson, John Hubert Noel G.
Jones, Harold Watson.
Jones, John Cyril.
Jones, William Pryce.
Jose, Victor William.
Josephs, Abraham.
Joyce, Esmond Lionel.
Kaempf, Emil.
Keet, Alan Livingstone.
Kellie, John Gordon.
Kelly, Maurice James.
Kingdon, Thomas Karl.
Knight, Arthur Digby.
Knox, William George.
Lackie, Donald Walker.
Lane, Francis Edward.
Lanes, Frank.
Langley, Harold James.
Lansdale, Edwin Dodsworth.
Leach, Herbert.
Lee, Chung Chee, B.Sc. (Eng.).
Lennie, George.
Lennox, Edwin Arthur.
Lett, Walter Ernest W.
Longstaff, George.
Lui, Sun Iu.
Lula-Crosse, Frederick Cecil.
Lumby, Frank.
Lyon, Edwin.
Mabbott, William Henry L.
McComb, Hugh.
Mackay, William John.
McKenzie, Douglas.
Mackie, Watson.
McLaren, John.
MacMillan, Archibald.
Macnab, Hector Archibald.
Macnaughton, Arnold Ingram.
Maddocks, William Arthur.
Mann, Adrian Bernard.
Marriage, William Frank.
Martin, Eric Harry B.
Mason, James Alfred.
Mawdsley, Walter Longton.
Mears, William Arthur.
Menesse, Norman Harold.
Minvalla, Gerald E.
Morduch, Oscar.
Morrison, James Douglas.
Mortimer, Robert Charles.
Munro, Robert.
Nicholson, Hugh John G. (B.Eng.).
Nixon, Ralph Henry.
Norris, Joseph Edward.

As Students—continued.

Oakley, George Edward J.
O'Dowd, Arthur Edward.
Orange, William John.
Orchard, George Henry.
Orkney, John Carnegie.
Orme, John Leonard.
Osborne, Ralph Sydney.
Owen, Eric Maxwell.
Owen, Thomas Sydney.
Palmer, Alfred James.
Palmer, William Thomas.
Parnaby, Trevor.
Parsons, Daniel Blundell.
Patton, William Charles.
Pearson, George Lincoln.
Pearson, Leonard Stanley.
Pearson, Norman Grey.
Pendleton, Edward.
Peters, William Herbert.
Phillips, Alexander Stevenson.
Pickles, John Sydney.
Pillans, James Pritchard S.
Pimble, Cyril Charles.
Poland, George Ernest S.
Powell, Edward Blennerhassette S.
Prangnell, Frank Norman.
Price, Arthur Conrad.
Price, Thomas Norman.
Pryce-Jones, Reginald.
Quick, William John.
Rampe, Percy Conrad.
Ramsay, Frank Raymond F.
Randall, Jocelyn Graeme.
Ransom, George Scott.
Ratcliffe, Thomas.
Rawlinson, John David S.
Read, Frederick William.
Read, Grosvenor Woodhouse.
Redford, Charles Frederick
Ribbons, Ernest William.
Robson, Andrew Eric.
Roddam, George.
Ross, William Bernard.
Russell, Frank Gerald.
Sanderson, John Marcellus.
Sankey, Bernard Robin.
Satchell, Harold Leslie.
Schofield, Wallace.
Scholefield, Leslie Cotterill.
Shannon, James Arthur.
Sharma, Jai Narain.
Sharman, Reginald.
Sharpe, Harry Derwent.
Shaw, Thomas.
Sheik, Abdur Razzaq.
Sherratt, Harold.
Short, William Thomas L.
Shute, Robert Oxenham.
Smiter, Herbert William J.
Smith, Frank.
Smith, Joseph.
Smith, Raymond.
Solomon, Julius.
Sowter, George Alfred V.
Springfield, Ernest.
Stahl, Henry Cecil.
Stanley, John Frank.
Steel, Arthur Frederick, B.Sc. (Eng.).
Steel, Carlyle Stewart.
Struthers, Alexander Marshall.
Suarez, Antony.
Sung, Lau Yoen, B.Sc. (Eng.).
Sutcliffe, Tom Halliwell.
Taylor, William Alexander.
Tees, James Errigal.
Thirtle, Arthur Charles.
Thomas, Demetrius.
Thompson, Edgar.
Thompson, Walter George.
Thomson, Alexander Stuart.
Train, Arthur.
Underwood, Gilbert Vivian.
Van der Byl, Voltelin St. John.
Vann, Leo John.
Vanneck, Richard Grant, M.C.
Villa, Joseph Edward.
Voelcker, John Westgarth.
Waddicor, Harold.
Walden, Ernest Merilion.
Walker, George Leslie.
Ward, John Wilfrid, B.Sc. (Eng.).
Wardle, Eric Bosward.
Warshawsky, Mark.
Wells, Francis Woodward.
West, Frank Edward.
Whitehurst, John Traill.
Wickes, Henry John.
Wilcoxson, Leslie Swales.
Williams, George Edward.
Wilmshurst, Arthur Percival.
Wilson, Henry Christopher.
Wilson, Joseph Reginald.
Wilson, Richard.
Wilson, Robert Albert.
Wilson, William Stanley.
Woodcock, Leslie James.
Woolliscroft, Harold Claude.
Wyborn, Sydney.
Yeung, Kwai Chiu, B.Sc. (Eng.).
Young, James Buchan.

As Associates.

Gardiner, Percy.
Lockhart, James.
Sillcox, Lewis Ketcham.

TRANSFERS.

Associate Member to Member.

Addey, Frederick, B.Sc.
Dutton, Hugh N.
Eaton, Richard Netterville.
Fitzpayne, Frederic Andrew.
Just, John Stephens.
Lawson, Percy Brailsford.
Leach, Richard Bertram.
Masters, Frederick Hill, O.B.E.
Overton, Thomas Richard.
Paton, Alfred Maurice, B.A., B.Sc.
Roberts, Leslie.
Scott, James.
Silver, Hugh Christopher.
Smith, Thomas Robert.
Tallent-Bateman, William T.
Tremain, William Ewart.
Ullmann, Ernest August.
Walker, William.
Watson, Edward Clifford, Commander, R.N.
Watts, Herbert W.
Wraith, William Heselton.

Associate to Member.

Johnson, Thomas Boyes.
Lewis, Francis Harman.

Graduate to Associate Member.

Blythe, William Herbert.
Caine, Allen Flinders.
Hamid, Khwaja Abdul.
Meecham, Jeffrey.
Norris, Albert George.
Smith, Gilbert.
Theaker, Herbert Michael.

Student to Associate Member.

Abbott, Geoffrey Joseph.
Bolton, Charles Richard.
Ford, Cyril Herbert.
McDougald, Leslie Alfred, Flight-Lieut., R.A.F.
Osborne, Ralph.
Rogers, William Langworthy A.
Smith-Rose, Reginald Leslie, B.Sc.
Woolley, Thomas Golding.

Student to Graduate.

Angus, Thomas Cochrane.
Brazel, Claude Hamilton, M.C.
Chalmers, John William P.
Felix-Smith, Leslie Eric.
Ford, William Arthur.
Habershon, Matthew Keith.
Hargreaves, Harold.
Hayward, Frank Howarth.
Henderson, John Alfred.
Ives, Charles Laurence.
Kilby, James Wheatley, O.B.E.
LeClezio, Maurice Jaques L.
Maclean, James Adam.
Nicolle, Dudley Eric.
O'Brien, Bernard Heath, M.C.
Pritt, Stephen Edward.
Pullan, Arthur Hubert.
Rhenius, Stanhope Theodore.
Shuter, Ernest John.
Sich, Walter Ebray E.
Strange, Frederick Philip O.
Tolley, Charles Edmund.

A paper by Mr. G. A. Juhlin, Member, entitled "Temperature Limits of Large Alternators" (see page 281), was read and discussed, and the meeting adjourned at 7.50 p.m.

ANNUAL DINNER, 1921.

The Annual Dinner of the Institution was held on Thursday, 3rd March, 1921, at the Hotel Cecil. The President, Mr. Llewellyn B. Atkinson, presided over a gathering numbering about 420 persons. Among those present were:—Sir Sydney Chapman, K.C.B., C.B.E. (*Permanent Secretary, Board of Trade*), Sir Frank Heath, K.C.B. (*Secretary, Department of Scientific and Industrial Research*), Brigadier-General Sir W. T. F. Horwood, K.C.B., D.S.O. (*Chief Commissioner of Police*), Sir Evelyn Murray, K.C.B. (*Secretary, G.P.O.*), Lieut.-Colonel Sir F. Younghusband, K.C.S.I. (*President, Royal Geographical Society*), Major-General Sir W. A. Liddell, K.C.M.G., Professor Sir W. H. Bragg, K.B.E., F.R.S. (*President, Physical Society*), Sir James Devonshire, K.B.E. (*Honorary Treasurer*), Sir J. E. Petavel, K.B.E., D.Sc., F.R.S. (*Director, National Physical Laboratory*), Sir William J. Pope, K.B.E., F.R.S. (*President, Society of Chemical Industry*), Sir A. I. Durrant, C.B.E., M.V.O. (*H.M. Office of Works*), Sir John Bland-Sutton, F.R.C.S. (*President, Royal Society of Medicine*), Sir Harry Haward (*Electricity Commissioner*), Sir W. Noble (*Member of Council, I.E.E., and Engineer-in-Chief, G.P.O.*), Sir Cooper Perry, M.D., F.R.C.P. (*Principal Officer, London University*), Sir John Snell (*Past President, I.E.E., and Chairman, Electricity Commission*), Sir Arthur Willert, Colonel R. E. Crompton, C.B. (*Past President*), Major-General G. D. Jeffreys, C.B., C.M.G. (*Officer Commanding London District*), Brigadier-General C. P. Martel, C.B. (*Chief Superintendent, Ordnance Factories, Woolwich Arsenal*), Mr. H. A. Payne, C.B. (*Permanent Secretary, Board of Trade*), Captain H. R. Sankey, C.B., C.B.E., R.E. (*President, The Institution of Mechanical Engineers*), Lt.-Colonel H. C. Sparks, C.M.G., D.S.O., M.C., Mr. G. W. Humphreys, C.B.E. (*Chief Engineer, London County Council*), Mr. P. V. Hunter, C.B.E. (*Member of Council*), Mr. W. W. Lackie, C.B.E. (*Electricity Commissioner*), Mr. C. D. le Maistre, C.B.E. (*Local Honorary Secretary, American I.E.E.*), Brigadier-General M. Mowatt, C.B.E. (*Secretary, The Institution of Mechanical Engineers*), Mr. C. P. Sparks, C.B.E. (*Past President*), Mr. C. H. Wordingham, C.B.E. (*Past President, I.E.E., and Chairman, The British Electrical and Allied Industries Research Association*), Mr. J. D. Bailie (*Hon. Secretary, North Midland Centre, I.E.E.*), Mr. G. Balfour, M.P., Professor E. Barker, M.A. (*Principal, King's College*), Mr. J. R. Beard (*Chairman, North-Eastern Centre, I.E.E.*), Mr. A. W. Berresford (*President, American I.E.E.*), Mr. H. Booth, O.B.E. (*Electricity Commissioner*), Mr. A. Carpmael (*Honorary Solicitor*), Mr. H. J. Cash (*Member of Council*), Mr. H. W. Clothier (*Member of Council*), Mr. J. R. Cowie (*Member of Council*), Mr. D. N. Dunlop (*Member of Council*), Mr. R. N. Eaton (*Hon. Secretary, Irish Centre, I.E.E.*), Mr. A. Ellis (*Past Chairman, Western Centre, I.E.E.*), Mr. A. G. Ellis (*Hon. Secretary, North-Western Centre, I.E.E.*), Dr. W. H. Eccles (*Vice-President*), Dr. S. Z. de Ferranti (*Past President*), Dr. C. C. Garrard (*Past Chairman, South Midland Centre, I.E.E.*), Mr. J. W. Gilbert, J.P. (*Chairman, London County Council*), Mr. F. Gill, O.B.E. (*Member of Council*), Mr. W. B. Hardy, F.R.S. (*Secretary, Royal Society*), Mr. J. S. Highfield (*Vice-President*), Mr. C. J. Jewell (*Chairman, North Midland Centre, I.E.E.*), Mr. J. E. Kingsbury, Dr. R. Knox (*President, Röntgen Society*), Mr. J. D. Morgan (*Hon. Secretary, South Midland Centre, I.E.E.*), Mr. A. J. Newman (*Chairman, Western Centre, I.E.E.*), Mr. A. Page (*Electricity Commissioner*), Professor A. W. Porter, (*President, Faraday Society*), Mr. A. H. Preece, Major H. Richardson, O.B.E., M.C. (*President, The Incorporated Municipal Electrical Association*), Mr. J. H. Rider, Dr. A. Russell (*Member of Council*), Mr. J. Sayers, O.B.E. (*Member of Council*), Professor C. S. Sherrington, F.R.S. (*President, Royal Society*), Mr. Roger T. Smith (*Past President*), Mr. W. O. Smith (*Chairman, British Electrical and Allied Manufacturers' Association*), Mr. W. J. Squires, J.P. (*Chairman, L.C.C. Highways Committee*), Mr. A. A. Campbell Swinton, F.R.S. (*Vice-President, I.E.E., and Chairman of Council, Royal Society of Arts*), Mr. J. Taylor (*Hon. Secretary, Scottish Centre, I.E.E.*), Mr. E. C. Wansbrough (*Chairman, Cable Makers' Association*), Mr. B. Welbourn (*Member of Council*), and Mr. P. F. Rowell (*Secretary, I.E.E.*).

After the usual loyal toasts the President read the following telegrams from the Associazione Elettrotecnica Italiana and the American Institute of Electrical Engineers:—

"MILAN.

"The Associazione Elettrotecnica Italiana sends warm greetings to the Institution of Electrical Engineers.—DELBUONO, *President.*"

"NEW YORK.

"Please convey to the President and members of the Institution of Electrical Engineers heartiest greetings of our Board, and assure them of the desire of all American electrical engineers to co-operate in furthering the peaceful aims and high principles of the English-speaking peoples."

Sir William H. Bragg, K.B.E., F.R.S., in proposing the toast of "The Institution of Electrical Engineers," said: It is my duty to-night to ask you to drink success to the Institution of Electrical Engineers. Everyone here wishes the greatest success and prosperity to this magnificent Institution, first of all, because in a patriotic way we like to think that the Society to which we and our friends belong is marching rapidly along the path of progress. You are all proud of the triumphs that electrical engineering has won in the past, and you are conscious that it only remains to see in the future still greater triumphs which it will be able to lay to its credit. All of us love electrical science. We know that electricity and electrical action lie at the root of all natural processes, and we feel proud to think

that it is our province to interpret them and to bring them to the use of man. I am, at the present time, the President of the Physical Society, and it is a great pleasure on its behalf to wish success to an Institution whose aims lie so closely to its own. Physics and electrical engineering have gone together these many years. In the days when electrical engineering was a very small thing, you were in close alliance with the physicists of the day, and one of the first tasks that electrical engineers set us was to measure accurately the standards that you were to employ; and for many years all the great physicists in England and abroad were in touch with you, developing the science of electro-magnetism. This continued until perhaps 20 or 30 years ago, and then some fairy waved her wand over the scene, everything changed, and you have an atomic theory and an electron theory, and physics is again in close touch with you, for you are developing work and apparatus and methods which depend intimately on these atomic and electron magnitudes. We in our field are grateful to you for the problems which they bring us, and we find in them some of our most interesting work. But beyond that there is one particular subject on which I should like to say a word. It is that the Institution represents a very high ideal. It represents the wish that everyone who belongs to it should learn how best to do the work they have to do, and how best to be able to give the greatest service to the public of which they form a part. You have encouraged men to learn their work; you have encouraged the reading of papers, studying, the development of the intellect of young men; and you have tried with all your heart to make yourselves efficient servants of this country. The acid test that you must apply to every association of men for one purpose or another is: Is it their high purpose to make themselves more fit to serve the people of whom they form a part? If they have not reached that point, then they are still in the lower stages of development. I pay the Institution of Electrical Engineers the homage that it has put that ideal before it, and it is for that more than perhaps anything else that those of us who watch your work honour it and wish you well. I ask you, therefore, to drink this toast with all honour, and I ask you to couple with it the name of your President. You have found a great President—a man who has done work of all sorts during his busy and useful life. I am delighted to couple the toast with his name and I ask you to drink it with all heartiness.

The President (Mr. Ll. B. Atkinson), in responding, said: Sir William Bragg has referred in kind words to the work and activities of the Institution. We are grateful to him for his kind words and, may I add, we are very proud of our activities. At the present time the total roll of the Institution is over 9 400. Of its activities let me say this: besides the work in public meetings we have some 32 committees of the Institution; many of these sit very regularly and often, carrying on the departmental work of our profession and industry. We are represented on, and take part in the proceedings of, 40 bodies external to the Institution, where we endeavour to uphold the ideas and interests of our members. Turning only to the months of January and February, I find that in London and the Provinces we have had 47 meetings for papers and discussions of one kind and another. This activity, all of it voluntary and co-operative, cannot be without an immense effect on the intellectual, scientific, technical and industrial work with which we are associated. In addition, the exchange of thought, ideas and information must have, and has, an immense effect in pushing forward the profession and the industry with which we are connected. The good fellowship which arises from these constant meetings among our members in all parts of the country is of the greatest assistance in these difficult times. May I add, that the frank confidence which exists between the body of members and the elected Council of our Institution—which has been well illustrated lately in the carrying out of certain domestic changes which we deemed necessary—that frank fellowship and good feeling are, I am sure, a source of the greatest gratification to everybody in the Institution. I would that I could speak equally happily of the actual state of industry in general. Last September, when I was preparing my Inaugural Address, our industry and all industries seemed to be in full blast and at the height of prosperity. But shortly after that, and very rapidly, there came a great change for the worse. Let me in a few words picture what took place. After the Armistice there was an immense release of purchasing power throughout the community. There were large profits in certain quarters which had been made during the war and which could not be spent during the war, because it was impossible for people to get what they wanted; there were Army gratuities; there were accumulated pays of all kinds; and the whole of this large purchasing power was augmented by credits advanced by the banks, which credits really relied on the deposits representing this purchasing power. An immense orgy of spending set in—public spending and, above all, private spending, principally on consumption and not on anything else. People were determined to spend and receive at whatever price and at whatever cost, with the result that the purchasing power before very long had left this country and gone elsewhere. What was the next picture: These bank deposits falling; bank loans called in and not realizable, because they were in stocks which were to be held for an advance in price, loans locked up in extensions of works for what appeared to be the coming and continuing boom. On the top of that we had unexampled taxation, and as the deposits disappeared and were not re-created, the banks rapidly began to put on pressure and call in their credits and loans. We all know what resulted. Some trades and many individuals were absolutely broken; nearly all trades were crippled, and immense unemployment has resulted. The banks have been wrongly blamed, for it is not their fault. The trouble, at the bottom of it all, is that instead of spending, as has been the custom in the past, about 85 per cent of our total income and earnings we were spending more than our total income and production, as Sir William Bragg has said. Of the immediate effect on our industry I can only say this: that as it does not depend on consumption, but on production for other

purposes, our industry has not yet felt the full effect of the collapse which took place; but the immediate future is, I fear, by no means so rosy. Notwithstanding the great impoverishment of the world and of our own country, the great majority of people in this country are living, and insisting that they ought to live, on a higher scale than in the period before the war. They are further putting forward the claim that, if they cannot live on that higher level, they should live at a lower level with money given them which has not been worked for at all. In these circumstances it is only natural that when we have satisfied the urgent buyers in our industry, the buyers who must have something at all costs, we reach the point—and we are reaching it, if we have not already reached it—where buyers who are not in such urgent necessity will hold off until a more normal state of production arises. The electrical industry lives by the outlay of capital on new constructions; it does not live by replacing past constructions. That means a constant flow of capital, and a constant flow of capital means savings. Before the war this country saved about £300 000 000 a year; it is doubtful to-day whether it is saving £100 000 000 a year. I estimate that at the present time, if the capital were forthcoming, this country wants for the next five or six years at least £20 000 000 a year put into the electrical industry for new construction; but if we are only saving £100 000 000 where is it to come from? The only place I know for the moment where it can come from is America. A good deal of American money is being placed over here now, and it is regarded as good speculation, because of the belief that the exchange will become normal. That may help us through part of the capital necessities of our own industry.

I turn now for a moment to that projected re-organization of our industry contemplated during and shortly after the war. May I remind you that in 1919 a great scheme was put forward in Parliament to re-organize the electrical industry on a basis of quasi-nationalization. Our industry did not accept that proposal well. There were many conferences in our own Institution of the various bodies who were interested. We made representations as an Institution to the Board of Trade, and our view then expressed was that it was not necessary to do more than appoint an understanding body of Commissioners, that those Commissioners should investigate the situation, but that for the rest the existing organizations should be left to work out a great scheme of electrical expansion. It was just about this time last year that the appointment of those Electrical Commissioners was completed. In the year that has elapsed they have mapped out provisional areas—areas which they considered should be grouped for the purpose of supply. But when we come to the proposals that have been put forward, and the results of the consideration of such proposals as have been considered, I confess to a feeling of disappointment. The schemes that have been put forward do not appear to me to have had that intensive study of technical or financial details that will carry conviction to engineers or financiers. We have, in addition, a further disappointment, that the old struggle between companies and local authorities has begun again—a struggle in which the question of technique and the question of economies are entirely submerged in questions of both financial and local policies. I feel that I must take this opportunity to utter what I consider to be a necessary note of warning. We have the opportunity under the Commissioners of doing the work ourselves in the way of which I have spoken, but I want everybody to realize that if we do not succeed, the public and the politicians will come in and again take a hand; and they will not again be satisfied with the reply that "If only we are left alone we can do the thing." Bear in mind, also, that if they come to that stage again, they will have the manufacturing interests and the employees' interests in our Institution backing them up, thinking that at least it is the best way of "getting a move on." I also want to say a word about a certain campaign that has been started in some of the financial and other papers, belittling the work that the Commissioners have so far carried out. I am sure that the members of our Institution do not accord their support to that campaign. It is suggested that the Commissioners are a costly and rather useless body, that they do little and are a grave expense. I am not in the inward counsels of the Commissioners, but I feel sure that, as regards the cost, if we took into account all the different authorities whose burdens they have taken over, we should find we had made a good bargain. Further, we have in those Commissioners men who understand the business and who are sympathetic to the industry in which many of them have been all their lives. Therefore I say for myself, and I believe for many others, that I do not fear trouble from the costliness or inability of the Commissioners. If I have any fear, it is that our own industry is not putting before those Commissioners schemes which they can recommend to go forward quickly.

If I have departed a little from the ordinary run of an after-dinner speech in thanking the proposer of the toast, it is because I feel that we have in front of us these one or two great difficulties to meet, and that everything is to be gained by looking them in the face and saying what we have to say about them. After all, with that immense number of units in our Institution that I have mentioned, spread all over this country, if the members will use their individual influences—and that is what it must always finally rest on—we can produce an enormous effect by concerted effort, and in that way we shall overcome these difficulties and obtain that success which the proposer of this toast has wished us, for which I thank him on your behalf.

Mr. A. A. Campbell Swinton, F.R.S., Vice-President, then proposed the toast of "Our Guests," to which **Sir Frank Heath, K.C.B.,** and **Sir John Bland-Sutton, F.R.C.S.,** responded.

TEMPERATURE LIMITS OF LARGE ALTERNATORS.

By G. A. Juhlin, Member.

(*Paper received* 29 *September*, 1920; *read before* The Institution 27 *January*, *before the* North-Western Centre 25 *January*, *before the* South Midland Centre 2 *February*, *before the* North Midland Centre 8 *February*, *before the* North-Eastern Centre 14 *February*, *and before the* Liverpool Sub-Centre 28 *February*, 1921.)

Summary.

The paper discusses existing practice of specifying temperature-rise of electrical machinery by thermometers, and points out that the B.E.S.A. Rules are more satisfactory but are not generally adopted, probably on account of the higher limits.

The relations between surface temperatures and internal or hot-spot temperatures are discussed, together with the influence of the load on the temperatures of different parts of a machine.

Location of hot spots in the axial direction is considered, as well as the heat flow through the insulation.

The influence of the thickness of insulation on the relation between internal and surface temperatures is dealt with, and a diagram showing the radial temperature distribution is given. The importance of considering the voltage of a machine in connection with temperature-rise on account of the greater temperature gradient through the insulation is pointed out.

Consideration is given to the question of operating large turbo plants on ultimate temperature rather than on temperature-rise, on account of the increased load obtainable at low air temperatures.

A plea is made to operating engineers to call for the fitting of embedded temperature detectors, although temperature guarantees based on this method may not be obtainable.

A series of tests on mica-insulated bars is described, including heating runs on bars insulated with mica wraps containing varying percentages of mica, and it is shown that the percentage of mica has a direct bearing on the breakdown voltage which the bars will stand after heating.

The results of bending tests on bars before and after heating are shown. The conclusion arrived at is that mica insulation, as used in modern turbo-alternators, retains its mechanical and electrical properties at temperatures up to 200° C. It is suggested that the limit of the ultimate temperature should be raised to 160° C. instead of the usual value of 125° C.

The general practice in this country is to a large extent to specify the temperature-rise of electrical machinery by thermometer measurement. The limiting temperature-rise generally adopted is 40 degrees C., independent of the type of machine under consideration and of the class of insulation employed in the construction of such machines. These temperatures are based on certain accepted limits as to the safe temperature to which the insulating materials can be subjected.

It has long been recognized that the 40 degrees C. limit is not altogether satisfactory, on account of improvements both in the insulating materials and in the methods employed in construction. Moreover, changes in the type of machines have made it clear that some revision of the methods of determining temperature-rises is required.

The difference between the internal and external temperature is comparatively small in the case of low-speed machines, on account of the facility with which the heat can be carried away from the internal parts. In the case of the modern large alternator, where large masses of material are concentrated in a comparatively small space, the problem is, however, quite different, and electrical manufacturers have long been aware of the fact that the internal temperatures have been far higher than those obtained by thermometer measurement on the external surfaces.

A great amount of work has been done by the electrical Standards Committees to bring temperature guarantees and the methods employed in measuring the temperatures to a common basis. In this connection reference may be made to Report No. 72 issued by the British Engineering Standards Association, which deals comprehensively with the whole question.

Comparatively few specifications issued for electrical plant are, however, based on these Rules. It is not uncommon to find that the methods of measurement laid down in the Rules are adopted, but that the limiting temperatures specified are generally very much lower. This may be due partly to the fact that the temperature-rises allowed in the Rules are in many cases higher than those that engineers are familiar with, and to a feeling of doubt whether insulations will safely withstand these temperatures. Experience has shown that such doubts are groundless and that mica insulation such as employed in turbo-alternators is capable of withstanding considerably higher temperatures than those laid down in the Rules. There is, so far as the author is aware, no evidence in support of a reduction in these temperature limits, but, on the other hand, good arguments can be put forward in support of higher limits.

The problem confronting the manufacturer has been to design machines which would meet specifications as regards temperature-rise measured by thermometer on external surfaces, and at the same time ensure that they would operate safely at the temperature known to exist in the interior. It is obviously desirable that the actual conditions under which a machine is operating should be known to the operating engineer, as well as the manufacturer, in order to obviate misunderstanding and difficulties which may arise if the operating conditions are not clearly appreciated.

Internal or Hot-spot Temperatures.

As has already been pointed out, manufacturers and operating engineers have long recognized that the

internal temperatures of large generators are far higher than indicated by thermometer measurements on the external surfaces of finished machines. To what extent the internal temperatures exceeded those obtained on the external surfaces has been largely a matter for conjecture. It is of the greatest importance that the true significance of the various temperatures should be understood as fully as possible, and for this reason it may not be out of place to consider in a general way the temperature distribution obtaining in large generators. In considering the temperature conditions of the stator, we shall for simplicity neglect the influence of the rotor upon the stator temperatures. The sources of heat in the stator are, as is well known :—

(1) Losses in the laminations due to hysteresis and eddy current, generally known as iron or core losses. These losses are as a rule considered constant and therefore independent of the load, although this assumption is not strictly correct.

(2) Losses caused by the load current. These losses may be divided into :—

(*a*) Losses in the winding (I^2R) and eddy-current losses in the copper, and

(*b*) Losses in the laminations and iron surrounding them, caused by the load current.

The whole of the losses under (2) may be said to vary as the square of the load current.

(3) Losses due to windage, and fan losses. These are of course independent of the load, except in special cases where external fans are used, and where the volume of air is altered with the load.

The change in temperature of the different parts of the stator with the load is shown in Fig. 1. This may be considered typical of large turbo-generators with long cores. In machines with short iron lengths, the temperature of the stator copper embedded in the iron may, at no load, be considerably below the temperature of the iron, due to the heat from the latter being conducted from the embedded portion to the end windings with a very small drop in temperature. Referring to Fig. 1 it will be noticed that the temperature of the iron increases with the load. This is due to the fact that in large turbo-generators the greater portion of the losses caused by the load current is generated in the laminations and the surrounding iron, the losses in the copper itself being a comparatively small portion of the total load loss. The temperature rise of the iron at any given load will then be a function of the total losses in the iron at this load and of the volume of cooling air. It is generally necessary to include a certain part of the losses in the embedded portion of the stator winding as a part of the losses causing the temperature-rise of the iron. In dealing with the temperature-rise of the embedded portion of the stator copper it is necessary to consider the temperature of the iron surrounding the winding, the amount of heat generated in the copper, and the thickness and quality of the insulation.

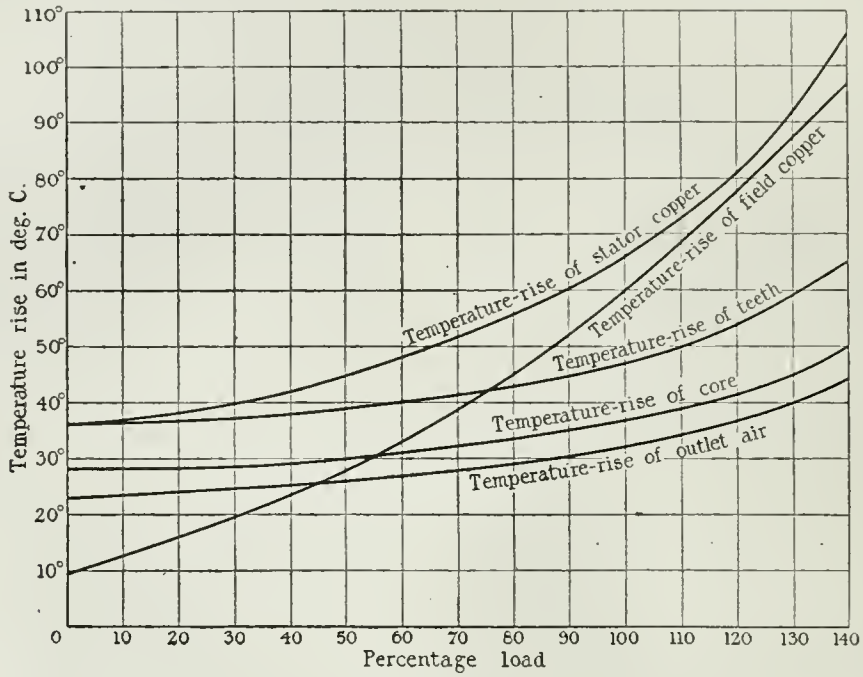

Fig. 1.—Curves showing variations in temperature-rise of different parts of a large alternator, due to change in load.

The problem of internal temperature is a very complicated one, for, although it is possible to determine the total losses caused by the load, it is practically impossible to ascertain by means of tests the amount of losses in different parts of the machine. Moreover, a part of the heat generated in the winding flows along

the conductors in an axial direction to the end windings, where it is taken up by the cooling air. Another part of the heat will flow through the insulation into the iron and will be conducted to the surfaces of the laminations exposed to the cooling air and thus dissipated. The predetermination of the internal temperature is therefore very complicated, and can only be done by a close study of each individual design, and even then it must, of necessity, be approximate. It may, however, be of interest to study in a general way the more important features governing the temperature distribution. As already pointed out, the heat generated in the stator winding may flow in an axial direction or through the insulation, and we shall therefore consider the conditions governing the flow of heat in these directions. A small part of the heat may be taken away by the air flowing over the outside surface of the coils exposed to the cooling air. In special cases cooling air is supplied

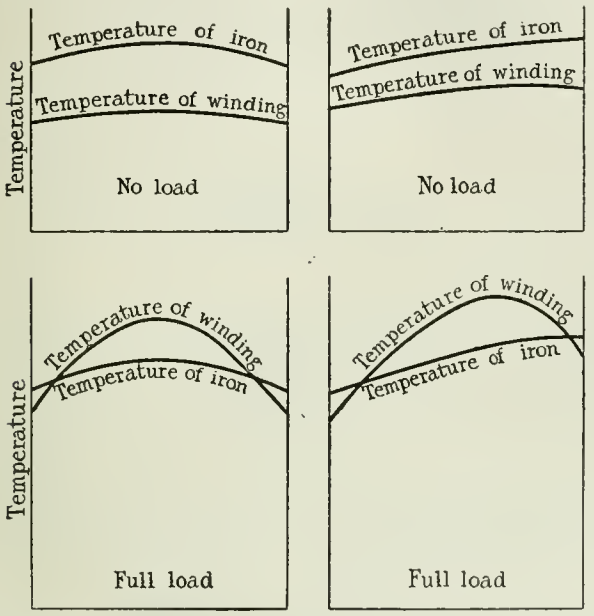

Fig. 2. Fig. 3.

Fig. 2.—Curves showing axial temperature distribution in stator, with radial ventilation.
Fig. 3.—Curves showing axial temperature distribution in stator, with axial ventilation.

through tubular conductors, but this will not materially affect the general principles.

Axial Temperature Distribution.

It will be clear that the axial temperature distribution must of necessity depend on the system of ventilation adopted. If cooling air of uniform temperature and in equal quantities could be supplied over the whole length of the lamination, the influence of the type of ventilation system used would be eliminated, but as this is not possible we will consider the purely radial and axial systems of ventilation. Several other systems have been devised for the equalization of temperature, but these need not be gone into. Figs. 2 and 3 show diagrammatically the conditions obtaining with radial and axial ventilation respectively.

The exact location of the hot spot will clearly change with the type of ventilation, and cannot therefore be determined without a close study of each particular case, but we can say that it will be located somewhere in the embedded portion of the winding, unless the ventilation of the end windings is abnormally bad. Such a case need not, however, be considered, as this is extremely unlikely in a well-designed machine. The heat will then flow from the hot spot along the conductors towards the end windings. The amount of heat to be transmitted becomes greater as we move along the conductors towards the ends, due to the fact that each unit of length generates a certain amount of heat, which must be added to that of the preceding part of the conductor.

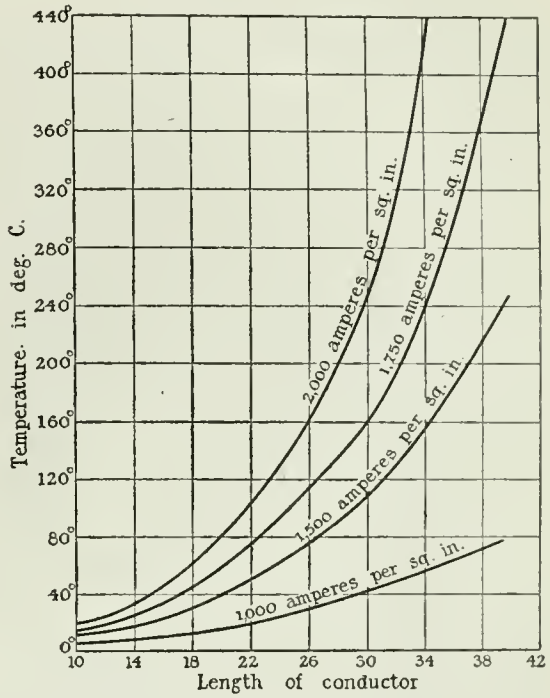

Fig. 4.—Curves showing temperature-drop for different lengths of conductor, and for different current densities. All heat generated is assumed to flow along conductors.

Moreover, the amount of heat generated per unit length is not constant, on account of the varying temperature of the copper, and the temperature-drop therefore increases very rapidly with the length of the conductor. The curves in Fig. 4 show the temperature gradient for varying lengths and current densities. These curves are based on the formula of Professor Miles Walker * and are also based on the assumption that all the heat is conducted along the copper. Such large temperature differences do not, of course, actually exist in practice, as a considerable amount of heat is transmitted through the insulation, but the curves serve to indicate the comparatively small amount of

* *Journal I.E.E.*, 1912, vol. 48, p. 674.

heat which can be conducted along the copper without excessive temperature differences.

It should be noted that in calculating these temperatures no account has been taken of eddy-current losses. The temperature-drop would therefore be greater than indicated by the curves if there should be no heat transmission through the insulation, and it is therefore obvious that, in machines with long cores, practically all the heat generated in the centre of the conductors must pass through the insulation into the lamination. In dealing with the question of taking into account the losses caused by eddy currents, it should not be overlooked that these losses diminish with increasing temperature, whereas the normal I^2R losses increase. Accurate calculations are therefore very complicated and tedious.

Heat Flow through Insulation.

The amount of heat which will flow through the insulation per unit surface is, as is well known, a function of the heat conductivity and thickness of the material. The thermal conductivity of any material may be expressed as the number of watts transmitted through a cubic inch of the material with a temperature-drop of 1 degree C. If the thermal conductivity of the insulation and the amount of heat to be transmitted are known, we can determine the temperature-drop with accuracy. A considerable amount of experimental work has been carried out in order to determine the thermal conductivity of different classes of insulation. The difficulty in applying the data thus obtained to a completed machine lies in the fact that the thermal conductivity varies to a great extent upon how tightly the insulation is applied on the coils, and on the clearance existing between the outside of the insulation and the laminations. No definite values which may be applied generally can therefore be given. Tests made on complete machines in actual service show that a thermal conductivity of 0·003 to 0·0035 watt per cubic inch per degree C. may be taken as an average figure. These values were obtained on coils insulated with mica wraps applied under heat by a special wrapping machine. Having fixed the value of the constant, the temperature-drop, T, will be obtained by the formula :—

$$T = \frac{\text{(Watts per sq. in.) (Thickness of insulation in inches)}}{\text{Thermal conductivity}}$$

We have still to determine the watts per square inch before it is possible to calculate the temperature-drop, and in this connection it is necessary to include the eddy-current losses in the copper, as these may be very large if the winding is not properly constructed. For this reason it is obviously impossible to judge the rating of a machine or ensure a liberal design by simply specifying that the current density must not exceed a certain value, because one machine may be run with, say, 2 000 amperes per square inch, and yet have a lower temperature-rise than another machine of similar size having a density of only 1 500 amperes per square inch. Moreover, the question of the proportions of the slot has a considerable influence on the temperature-rise which will be obtained with a certain current density, so that this value is no criterion as to liberality of material in a certain design.

To show the order of magnitude of the temperature-drop, we shall assume certain values for the quantities necessary to make the calculation. Consider a slot the section of which is shown in Fig. 5 and assume that all the heat generated in the copper conductors at the centre of the core has to be transmitted laterally through the insulation from point A to point B. We shall assume a temperature-rise of 40 degrees C. on the laminations, as measured on the surface, and allow 10 degrees C. for the difference of temperature between the point B and the surface of the laminations. If the temperature of the inlet air be taken as 15° C., the final temperature of the point B will then be 65° C. Assuming the watts per square inch of surface to be 0·7, which may be taken as a reasonable figure, and the thickness of the insulation to be 0·12 inch, which would be the

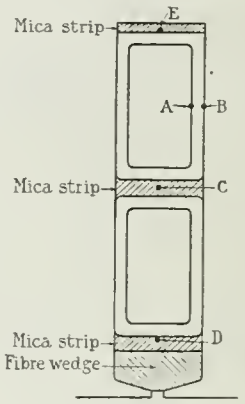

Fig. 5.—Section of slot, for which the temperature-drop is calculated.

average thickness for a 6 600-volt machine, the temperature-drop from A to B would be :—

$$T = \frac{0{\cdot}7 \times 0{\cdot}12}{0{\cdot}003} = 28 \text{ degrees C.}$$

the total temperature of point A would be 93° C., and the temperature-rise 78 degrees C. above the cooling air. For a 10 000-volt machine we should obtain a temperature-drop from A to B of 48 degrees C., and a total temperature at A of 113° C. The temperature-rise would be 98 degrees C. above the cooling air. These figures may seem high as compared with the conventional figure of 40 degrees C. measured by thermometer, but in reality they are not in any way excessive, for if the temperatures of existing machines were measured they would probably be found to be in the neighbourhood of these figures, if not higher.

It will be seen from the above example that appreciably higher internal temperatures will be met with in high-voltage machines than in low-voltage ones, if the same surface temperatures are specified. This has, of course, been recognized by the British Engineering Standards Association, and a reduction in the temperature-rise of 1½ degrees C. for each 1 000 volts or part of 1 000 volts

by which the voltage exceeds 5 000, is specified when the thermometer method is adopted. It is clear from the above that it is necessary to exercise considerable discretion when fixing temperature limits for machines of different voltage, and it also follows that higher surface temperatures may be permitted for low-voltage machines than is the case for high-tension machines, without exceeding the internal temperatures of the latter. In addition to the temperature-drop through the insulation there will, of course, be a temperature gradient in the stator laminations, the temperature decreasing towards the outer periphery.

The difference in temperature between the iron at the root of the teeth and the outer periphery will clearly depend on the losses in the laminations, and on the radial depth of the core and the system of ventilation adopted. The temperature conditions of the stator winding in the slot and of the laminations as regards the radial direction are shown diagrammatically in Fig. 6. The difficulties involved in making allowances in surface temperatures so as to obtain approximate

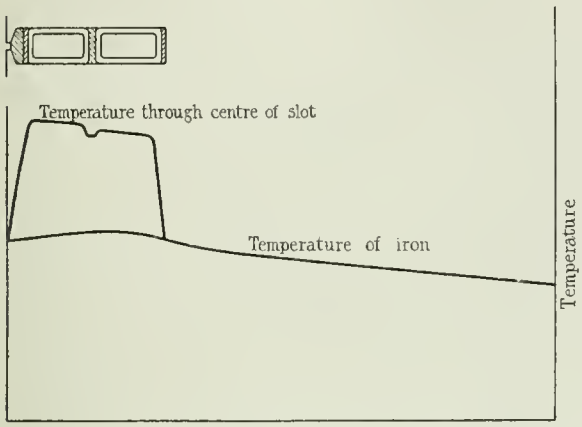

Fig. 6.—Temperature distribution in a radial direction.

internal temperatures are obviously very great, and for this reason it is of importance to consider the question of temperature measurements so far as the different methods are concerned.

Temperature Measurements.

Engineers are, of course, familiar with the different means which may be adopted for temperature measurements, but the significance of the results obtained by the various methods are not always clearly appreciated, and it may therefore not be out of place to consider the various methods at our disposal. We may measure temperatures of electrical machinery by :—

(1) Thermometer.
(2) The increase in resistance of the windings, and
(3) Embedded temperature detectors.

The thermometer method is of course the simplest and, so far as surface temperatures are concerned, the most reliable.

It is hardly necessary to discuss the precautions required in order to obtain satisfactory readings on surfaces over which an air blast is passing, as the necessity of shielding the thermometer bulbs from the blast is obvious. Temperatures of bare surfaces can be obtained with accuracy by this method. In cases where no appreciable temperature-drop exists between the inside of the body, the temperature of which we wish to measure, and the surface on which the thermometer is placed, accurate results can be obtained. In measuring the temperatures of a body covered by a poor conductor of heat, considerable variations may be obtained in the readings of the thermometer, depending on the thickness of the covering over the thermometer bulb.

Hot pockets may also be created by the pad used, and these may result in false readings. The use of the thermometer is clearly limited to accessible surfaces, and no true indications can therefore be obtained as to the internal temperatures of coils embedded in the iron of a machine. It is, moreover, not possible to measure the temperatures of rotating parts until the machine has come to rest, and this may be, in the case of large turbos, a considerable time, during which the temperature may drop to a considerable extent. A closer approximation of the true internal temperatures can be obtained by the use of the increase-of-resistance method.

It is clear that this method will only indicate the *average* increase in temperature of the whole winding, but it will nevertheless give a very close indication of the internal temperatures of the winding. Considerable care will have to be exercised in measuring the resistance, as a small error in this reading has a great influence on the percentage increase in resistance, and therefore on the temperature-rise. This method is readily applicable to windings carrying direct current, but, in the case of alternating-current windings, it is difficult to employ on account of the necessity of requiring a separate direct-current supply of steady voltage. Moreover, it is necessary to arrange for means of disconnecting the winding from the system (tests on large machines usually being made with the machine on commercial load), and for connecting it to the direct-current supply in order to take resistance readings. It is frequently difficult to take a large machine off load, in which case it is not possible to adopt the resistance method for the stator winding. For the field windings of turbo-alternators the method is readily applicable, and readings are easily obtainable during the run.

In dealing with the question of heat flow through insulation, the author endeavoured to emphasize the discrepancy between internal temperatures of the copper and those obtained on the external surface. This point is frequently lost sight of in preparing specifications, for manufacturers are not infrequently asked to quote on machines having a temperature-rise of 40 degrees C. as measured by resistance. Machines built to meet such conditions would be very expensive and wasteful as regards material, besides being inefficient on account of their size for a given output as compared with those built for a higher temperature-rise. The margin of safety on such machines would be

beyond the requirements of even ordinary cotton insulation.

The question of embedded temperature detectors has, up to the present, received comparatively little attention in this country as a means for determining the temperature of machines in commercial operation. They have, of course, been employed for special investigations and have been included in the Rules issued by the British Engineering Standards Association as a means for measuring the temperature-rise. In America this method has been adopted as a standard, and machines are now generally provided with embedded temperature detectors during construction. Special instruments are provided, so that the temperature of the different parts of a machine can be read at any time. Some of the advantages, from the operating point of view, of such an arrangement are quite obvious, whereas others are not quite so apparent. For example, let us consider a machine, the stator winding of which is insulated with mica on the embedded portion and by fibrous material on the ends. Now, the limits for the temperature-rise laid down by the British Engineering Standards Association for fibrous insulation are 55 degrees C. measured by resistance, or 50 degrees C. by thermometer, with certain reductions for high voltages, and, as a portion of the winding is insulated by fibrous material, these limits must be applied for a machine insulated as described. The temperature-rise permitted for mica insulation is 75 degrees C. by resistance method, but it is not possible to take advantage of this higher limit owing to the fact that, as the end windings are insulated with fibrous material, the temperature-rise for the whole winding must not exceed that specified for fibrous material. In such a case it would clearly be advantageous to measure the temperature-rise of each portion of the winding independently.

The effect of short time overloads on large turbo-alternators is of importance from the temperature point of view. Referring to Fig. 1 it will be seen that the drop of temperature through the insulation increases very rapidly with the load, which of course would be expected from the nature of the losses in the windings. At the same time, the temperature-drop through the iron is increased to some extent. Consider the case of a machine required to give 50 per cent overload for half an hour. Experience has shown that the windings reach their maximum temperature in less than an hour starting from cold, and would reach a steady temperature due to the overload in well under the half-hour, starting hot.

The large temperature gradients in the insulation and the iron cannot be measured by thermometers, and it is therefore impossible to form any judgment by this method as to the internal temperatures. By the use of embedded temperature detectors we shall be able to determine very closely the actual temperature of the copper, and thus prevent the machine being overheated. Similarly they will enable operating engineers to take full advantage of the capacity of a machine and operate it up to the ultimate hot-spot temperature permitted for the class of insulation employed. When embedded temperature detectors are used, the temperature guarantees should, in the opinion of the author, be based on the total temperature and not on a specific temperature-rise based on a given air temperature. The advantage obtained by operating large turbo-alternators on this basis are of importance and deserve consideration. Consider two identical machines, one operating on the basis of a given temperature-rise with a fixed value for the air temperature of 40° C., and the other on the principle of a maximum total temperature. The average air temperature in this country is less than one-half of the assumed air temperature of 40° C., and it is obvious that we are not taking advantage of the permitted temperature limit of the insulation when operating on the temperature-rise basis. By providing embedded detectors it is possible to take the full advantage of low cooling air temperatures, as readings can readily be taken at any time.

During the winter months the difference between the actual air temperature and the basis temperature will be still greater and, when it is considered that the load usually is heavier at this time of the year, the advantage is of considerable value in augmenting the output of a station at a time of heavy load. The increase in output due to low air temperature will vary somewhat depending on the design of the particular machine under consideration. Taking the curve in Fig. 1 as a basis, we should reach the maximum permissible temperature-rise based on an air temperature of 40° C. at 115 per cent full load. Assuming an air temperature of 15° C., which is, if anything, rather high for the winter months, we should be able to run at 140 per cent full load without exceeding the total temperature on which the temperature-rise reached at 120 per cent load was based, i.e. 120° C. In operating on the temperature-rise basis we lose 18 per cent of the output the machine would be able to give if run up to the limiting total temperature.

Basing the increase on the average air temperature throughout the year, the increase in output would probably be not more than 10 per cent, but even at this figure the saving in capital expenditure on a 20 000-kW set would be very considerable. As has already been pointed out, the possible increase in output depends largely on the design, and the increase in load given above may not always be realized, but at the same time it should not be overlooked that the air temperature is frequently lower than 15° C., and that the lower this value the greater is the increase it is possible to obtain from the alternator. The rating of large plants on the basis of ultimate temperature is standard practice in America, both for turbo-alternators and for water-wheel generators. The practice is also becoming general in Scandinavia, as many operating engineers ask for guarantees of the ultimate temperatures machines are capable of withstanding.

With regard to the type of detectors, there are two kinds which have found extensive use for electrical work, namely, the thermo-couple and the resistance thermometer. Both types are well known and do not require description. It may be of interest to consider their merits and demerits as well as the question of location in the slots, in order to decide which will give results closest to the actual copper temperatures. The thermo-couple has the advantage of being very small and

readily placed in the winding. With reference to the actual temperature measurements, two methods have been adopted. In one case, the electromotive force of the thermo-couple is measured direct by a millivoltmeter, in which case the thermo-couple has to be calibrated, as the resistance of the couples will of course affect the readings obtained. This method is very simple, and has been found by experience to meet the requirements. The second method is a zero-current method, i.e. no current flows through the thermo-couple, the electromotive force of which is balanced against a cell. This makes the reading independent of the resistance of the couple, but it is somewhat more complicated, and more time is required to obtain the readings, and it also has the disadvantage that a special cell and adjusting resistance are required. In order to obtain direct readings of the total temperature with thermo-couples, it is necessary to keep the cold junctions at a constant temperature. This necessitates special arrangements, such as burying the cold junctions in the earth, which keeps at a very constant temperature, or by keeping them in a bath of constant temperature. Several schemes of this kind have been devised, and the problem does not present any difficulties.

These special arrangements can be avoided if desired, as the instrument used can be provided with a movable pointer which can be set so that the total temperature is read direct from the instrument. A thermometer, together with the cold junctions, would be mounted close to the instrument so that the cold-junction temperature could be read and the instrument set accordingly when temperature readings were desired.

The resistance thermometers have the disadvantage that they occupy considerably more room than the thermo-couple, and are therefore more difficult to shield from air currents. A cell of constant voltage is also required in this case, in order to obtain the resistance. These thermometers possess the advantage of enabling us to read the total temperature direct on the instrument without any special arrangement such as is required when thermo-couples are employed. As this special apparatus may be dispensed with, the advantage is not great, and the opinion of the author is that the thermo-couple is the most satisfactory means of measuring internal temperatures, owing to the small space occupied which makes it possible to measure the temperature of any particular spot in a machine. The location of the thermo-couples must be carefully considered, in order to ensure that the temperatures indicated do not deviate materially from the actual copper temperatures. In order to obtain the absolute hot-spot temperatures, it would be necessary to place the detectors in direct contact with the copper. This arrangement would clearly be undesirable on account of the necessity of taking the leads through the insulation. The detectors would of course be placed in the slots next to the neutral point, but as this point may, during a disturbance of the system, be raised to full phase potential above earth, it would constitute a danger. It is then important to study the question of placing the couples outside the insulation, and the temperatures obtaining at the different points in the slots where detectors are located.

Referring to Fig. 5, it will be clear that a detector placed at the point B between the coil and the slot lining, which is generally introduced between the coil and the lamination, would indicate a temperature somewhere between that of the copper and the iron. If it were placed between the two coils at the point C, the reading obtained would closely approximate to that of the copper itself, for the reason that there is practically no heat flow between the two coils, except the very small amount which will flow through the edges of the packing strip between the two coils. This amount will be almost negligible unless the strip is very thick, and for this reason it is desirable to reduce this packing to a minimum. The temperature at point D will also approximate to the copper temperature, although not so closely as at C. A certain amount of heat will be transmitted through the wedge, but, if this is thick as compared with the coil insulation, the drop through the insulation will be comparatively small. By providing a substantial strip of insulating material between the coil and the lamination at the bottom of the slot, and placing the detector between the strip and the coil at E, we should obtain a temperature varying but little from the true copper temperature. American standard rules specify that, when there are two coils per slot, detectors are to be placed between the coils, i.e. at C, and if only one coil per slot is used it must be located at the bottom of the slot. It is probable that manufacturers in this country will not at present be prepared to guarantee temperatures measured by embedded detectors, on account of the low temperature limits specified, but it is hoped that operating engineers will encourage the provision of detectors, and thus put the manufacturers in a position to gain the experience which cannot be obtained by running machines light at the works.

Temperature Limits.

The determination of the point at which failures of insulation may be expected is very difficult and largely a matter of opinion. Insulation which, if not disturbed, would last almost indefinitely, would fail very quickly if subjected to vibration or shocks. Insulation failures may be divided under two distinct headings:—

(1) Mechanical.
(2) Electrical.

Mechanical failures.—These may be caused by:—

(*a*) Severe strains which crack or otherwise damage the insulation.
(*b*) Brittleness of the insulation due to drying out, which causes it to lose its elasticity and break if subject to the smallest displacement.
(*c*) Destruction of the binding material used in building up the insulation. This causes looseness and makes it liable to destruction by vibration.

Electrical failures.—These may be due to:—

(*a*) Direct puncture of the insulation.
(*b*) Reduction in insulation resistance

In order to determine the limiting temperatures to which a particular grade of insulation can safely be subjected, it is necessary to study carefully its properties as regards the various conditions which may cause failures. In the American and British Standardization Rules three different grades of insulation are considered, and the following table gives the temperature limits which it is considered should not be exceeded :—

Class	Description	Maximum Temperature to which the Material may be subjected continuously
A	Cotton, silk, paper and similar materials not treated, impregnated or immersed in oil	95° C.
	Similar materials to the above but treated, impregnated or immersed in oil	105° C.
B	Mica, asbestos and other materials capable of resisting high temperatures. If Class A material is used in conjunction with Class B material for structural purposes only, and may be destroyed without impairing the insulating or mechanical qualities of the combined material, then the material may be considered as Class B	125° C.
C	Fireproof and refractory materials, such as pure mica, porcelain, quartz, etc.	Limits not determined

It is generally agreed that the limits fixed for Class A insulation cannot safely be exceeded, and as Class C insulation is not employed in machine construction, except to a very limited extent, only Class B material will be considered. Of the materials which come under this heading mica is the most important. It is used either in the form of tape on bent portions of the coil, or in the form of a wrap applied on the straight portions of the coil. In either case some material such as paper or cambric is used as a backing on which mica splittings are placed. Various bonds are used for sticking the mica on the backing, the most usual being shellac. In dealing with the question of internal temperatures, it was pointed out that the maximum temperatures are met with in the embedded portion of the coils, and for this reason we shall consider the effect of temperature on mica wraps, such as are employed on the coils of large turbo-alternators. In order to determine the effect of temperature on this class of insulation, a number of test samples were made up for testing. Each complete bar consisted of three conductors insulated from each other by half-lap layers of mica tape. This tape consisted of a layer of thin cambric which served as a backing for the mica. A special varnish which remains flexible for a considerable time was used as a bond. The insulation round each complete bar consisted of a mica wrap. For half the number of samples the mica wraps were made with thin cambric as a backing for the mica, the remaining ones being insulated with the standard mica folium in which the mica is built up on thin paper.

Both of these insulations would therefore be graded as Class B under the B.E.S.A. Rules. The total thickness of insulation from copper to earth was 0·14 inch, the mica wrap being 0·106 inch in each case. These mica wraps were applied by the wrapping machine employed for standard products, and were therefore in no way special. Care was taken to ensure uniform thickness of insulation, as comparative tests had to be made on the samples. Bending tests were made on a number of bars in the condition they were in after having been insulated in the wrapping machine in the usual way. Other bars were clamped between iron strips to avoid undue swelling of the insulation, and heated for 6 weeks at a temperature of 150° C. by means of a current passing through the conductors. They were afterwards subjected to the same bending test as the bars which had not been heated. In making these bending tests the bars were clamped as shown in Fig. 7, and gradually increasing loads were applied. The deflections were measured for each load and in all cases reduced to the equivalent deflection at 1 ft. radius. The deflection remained constant for a given load, so long as it did not approach the breaking load. As the breaking load was approached, the deflection increased rapidly even with the load constant. For this reason it was not possible to determine the deflection at the breaking point with any degree of accuracy.

The results of these tests are shown in Figs. 8 and 9. The upper parts of the curves are dotted for the reason just given. A voltage of 11 000 was applied between the conductors and the clamp during the time the tests were carried out. In all cases it was found that mechanical failure of the wrap preceded electrical failure. The voltage at which the wrap broke down depended upon the distance from the iron clamp at which the wrap cracked, and varied between 11 000 and 19 000 volts. Examination of the heated bars showed the wrap to be in excellent condition and, as will be seen from the curves, the effect of the prolonged heating was only to reduce slightly the load required to produce a given deflection, and also to reduce the load at which the insulation cracked. The actual mechanical strength of the insulation is, however, of secondary importance so far as turbo-alternators are concerned, for the reason that we cannot usually rely to any extent on the strength of either copper or insulation to prevent displacements of the winding in the case of short-circuits. The point at issue is whether high temperatures reduce the amount of deflection

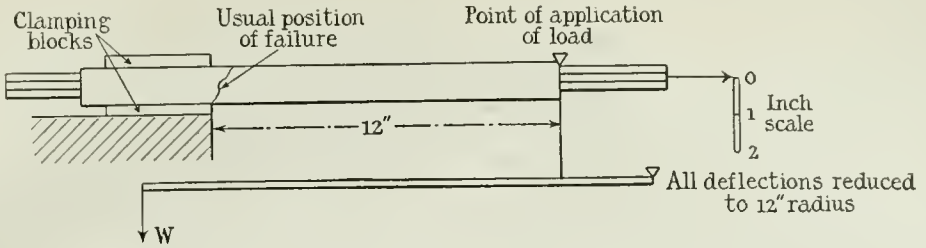

FIG. 7.—Diagrammatic arrangement of bending tests on stator bars.

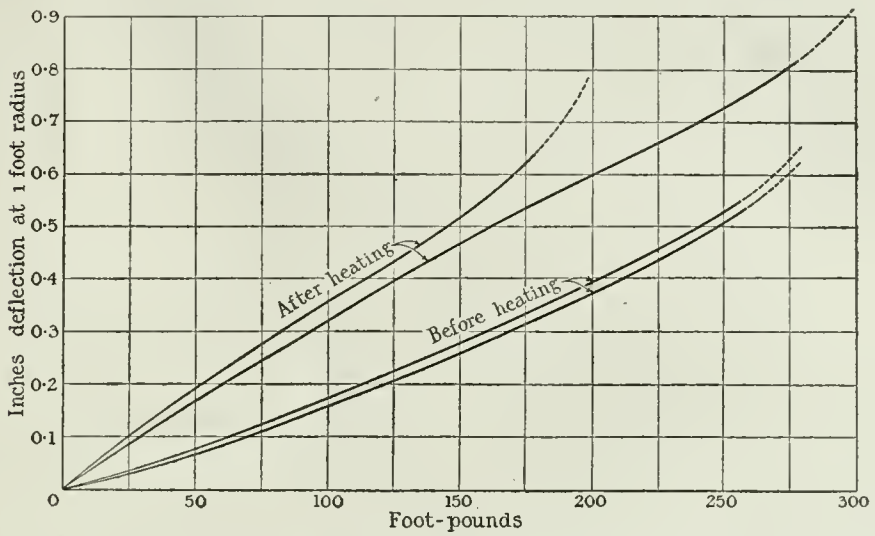

FIG. 8.—Curves showing relation between load and deflection of insulated copper bars, before and after heating up to 150° C. for six weeks. The insulation consisted of paper-mica wrap.

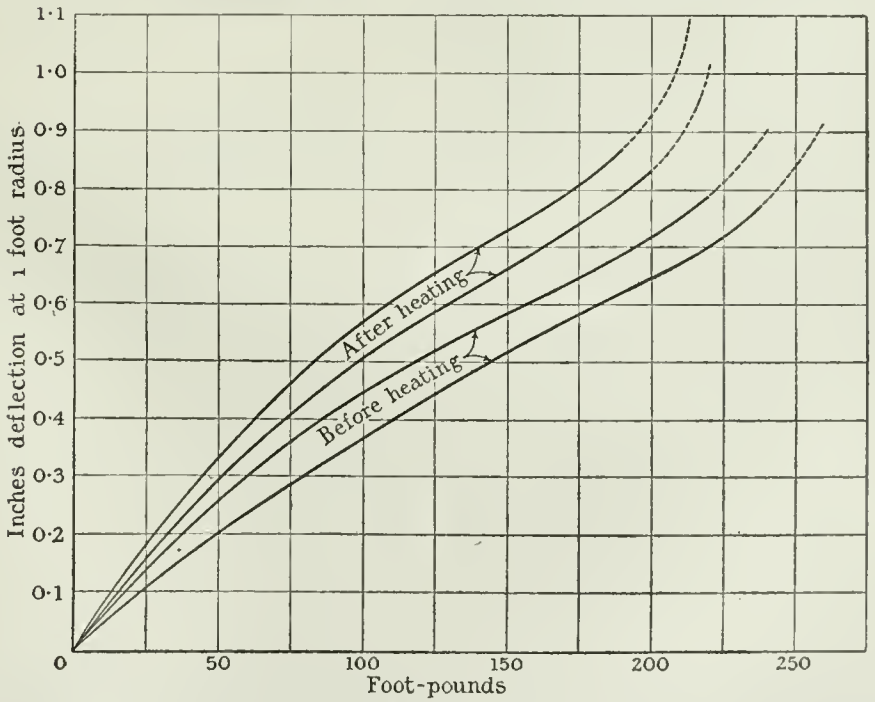

FIG. 9.—Curves showing relation between load and deflection of insulated copper bars, before and after heating up to 150° C. for six weeks. The insulation consisted of cambric-mica wrap.

which mica insulation will stand without mechanical damage.

The tests show clearly that no diminution in the elasticity of the insulation is caused by temperatures up to 150° C. This result may at first sight seem doubtful, but when it is considered that the insulation is heated to approximately 150° C. during its application on the conductors, and that this temperature is high enough to evaporate most of the volatile constituents in the bond but not to destroy either the backing or the bond, there seems no reason why any loss in elasticity should occur with prolonged heating. It may, on the contrary, be expected that the deflection which the bars would stand without breaking should increase after heating to a temperature high enough to destroy the bond, as the individual mica pieces would then have a certain amount of freedom to move. It is probable, however, that in such a case electrical failure would precede mechanical failure. The reduction in mechanical strength of the bars against bending is no doubt due to the fact that the backing used has lost its strength to some extent, and this would clearly cause a reduction in the strength of the complete bar.

Dielectric tests were carried out on all samples after the bending tests. There was no difference in breakdown strength of the insulation between the bars before and after heating. All samples failed between 30 000 and 35 000 volts, which may be considered satisfactory for the thickness of insulation employed, especially in view of the fact that the insulation had been subjected to considerable strain during the bending tests.

On account of the excellent condition of the insulation after having been subjected to a temperature of 150° C., further tests were arranged in order to determine the effect of still higher temperatures. A further set of sample bars similar to those previously used was made, the thickness and quality being the same as for the samples used for the previous tests. In order to approximate as far as possible to actual service conditions, the samples were placed in semi-enclosed slots of a sector of laminations taken from a standard machine and assembled for the purpose of the tests. The bars were heated by passing an alternating current at 25 periods per second through them. Temperatures were measured by means of thermo-couples placed directly on the copper in each slot and kept as near as possible to 200° C. during the whole run.

Slight variations in temperature of the different bars could not be avoided, owing to the variations in the temperature of the iron from slot to slot. A typical temperature chart of one bar is shown in Fig. 10, from which it will be seen that the total time of the heat run was 4 300 hours and that this particular bar reached a maximum temperature of 236° C., the average temperature being just over 200° C. The laminations were kept at a potential of 11 000 volts above the copper during the daytime, or for about half the time of the run, so that any failure of the insulation could be detected at once and thus make it possible to determine the time of failure. No insulation troubles were, however, experienced.

Dielectric tests were made on the bars after the heat run. Two of the samples failed at 18 000 volts. These failures were found to be due to mechanical damage caused by bending. One bar was, in fact, known to be damaged. All the other samples withstood 25 000 volts, and it was not possible to carry the voltage any higher, due to arcing over at the ends. The test, however, is sufficient to show that no appreciable deterioration of this insulation had taken place.

The bars were then removed from the slots for inspection and were found to be in a satisfactory condition. It is of special interest to know that the bars were tighter in the slot after the run than before, and considerable care had to be exercised in order to remove them without damage. This may seem peculiar, as one would naturally expect the insulation to shrink due to the high temperature, and there is no doubt that a certain amount of shrinkage does take place, but this shrinking is more than compensated for by the elasticity of the mica. The mica splittings act in the manner of a multi-layer spring which presses against the slot sides and so keeps the coil in position.

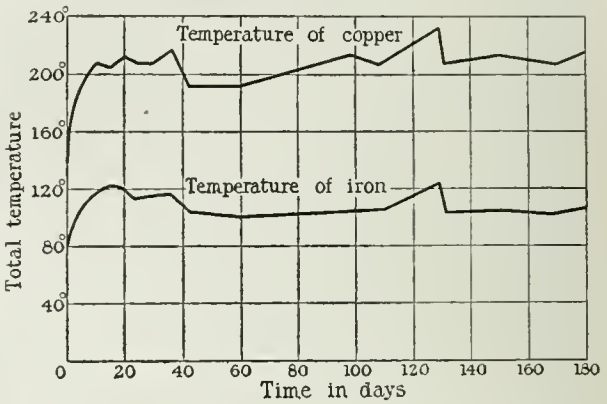

FIG. 10.—Curves showing temperature during heating test.

This is borne out by ordinary experience, because it is usually exceedingly difficult to withdraw bars from machines which have been in operation for some time, due to this elastic expansion of the mica insulation. The paper and cambric used for building the mica upon was found to be carbonized, but remained intact on the inside layers. The outside paper covering was destroyed in most cases, but this was done in removing the bars, which had to be pulled out of the slots.

The fibre wedges employed at the top of the slots for holding the bars in position were very brittle and loose due to shrinkage. This was caused partly by the iron temperature, which was considerably higher than met with in actual practice owing to the lack of ventilation of the surface of the lamination, and partly by the absence of the mica strip which is normally used between the bars and the wedges.

The surface temperature of the iron is shown in Fig. 10. It has been recognized for a long time that the solidity of the wrap is of great importance as far as its heat-resisting qualities are concerned. Of still greater importance is, of course, the percentage of mica employed. In order to determine the influence of these factors several sets of bars were made and insulated with wraps

containing different percentages of mica varying from 20 per cent in the old hand-wrapped type, up to practically pure mica in some special wraps. It is of interest to note that it was found possible after considerable experimenting to apply mica wraps containing no paper, by means of the Haefly wrapping machine.

The bars used for this test were taken from some coils of a standard water-wheel alternator in process of manufacture. The insulation between individual turns consisted of the cotton covering of the conductors and, in addition, mica strips between each conductor. The thickness of the insulation was that normally employed for an 11 000-volt machine. All these bars were subjected to a temperature of 190° C. continuously for 10 months, or about 7 000 hours. A voltage of 11 000 was applied between the winding and the core each day to ascertain whether any insulation failure had occurred. As shown in the following tables, the bars withstood considerably higher voltages than this at the conclusion of the test. The various samples are given in the order of percentage of mica contained in the wraps and, as will be seen, the breakdown value of the insulation follows the same order.

Table showing Breakdown Voltage of Various Classes of Mica Wraps after heating for 7 000 *hours at* 190° C.

Description of wraps on the various samples	Breakdown voltage	
	Minimum	Maximum
(1) Empire cloth and mica, hand-wrapped	15 000	26 000
(2) Cambric and mica, machine-wrapped	31 000	35 000
(3) Paper and mica, machine-wrapped	32 000	45 000
(4) Paper and mica, machine-wrapped	47 000	48 000
(5) Mica and shellac only, machine-wrapped	50 000	52 000

The insulations (3) and (4) are, as will be seen, of the same kind, but they differ somewhat in the percentage of mica due to a different thickness of paper being employed. In applying the insulation it was found that the bars wrapped with No. (3) insulation were not as solid as those wrapped with No. (4). These high-pressure tests are highly satisfactory with the exception of those on the Empire cloth samples, and these were expected to show low breakdown values. All the other samples show a satisfactory margin of safety over the operating voltage, which would be only 6 360 volts to earth.

At the end of the test the bars were still a good fit in the slot, but the cotton covering on the individual conductors was entirely destroyed, all the conductors being quite bare. The mica strips between the conductors were, however, in perfect condition. The general condition of the wraps was the same as in the previous tests, i.e. the fibrous materials used for building the mica on were carbonized, but the wraps were in all cases quite intact. The solidity varied, as would be expected, with the amount of mica embodied in the wraps. The hand-wrapped Empire cloth and mica insulation was spongy, but the paper mica wraps were quite solid, the pure mica wraps being practically as hard as when the test began.

The above tests confirm the results obtained from a series of similar tests carried out by the Westinghouse Electric and Manufacturing Company, Ltd., at Pittsburgh, U.S.A. In these tests mica insulation such as used in the tests described in this paper was subjected to temperatures as high as 300° to 400° C. without any serious loss in dielectric strength. Reference may also be made to a series of tests (*see* paper by Newbury, *Transactions of the American Institute of Electrical Engineers*, 1916, vol. 35, p. 1489) carried out by the same Company on one of the original Niagara Falls machines, from which exceedingly valuable data on the question of maximum safe temperature of mica insulation were obtained. It may not be out of place to give the table summarizing the operating record of the machine in question.

Length of service in hours	Range of operating temperatures in ° C. (based on 35° C. air)
40 000	120 to 145
13 800	145 to 175
8 200	175 to 210
2 600	210 to 245
100	245 to 285

No insulation failures which could in any way be attributed to the deterioration of the insulation were experienced during the whole of the time the machine was in service. The author believes that this machine is not unique so far as temperature is concerned, but that there are other machines operating at temperatures almost as high as those given in the table.

Rotor Insulation.

As the experiments mentioned have been carried out only in connection with insulation as applied to stator bars, it may be of interest to consider briefly the conditions of temperature distribution and the quality of the insulation applied in rotors. Regarding temperature distribution, it may be said that it is generally more even than in the case of the stator. Tests indicate that the temperature of the end windings is greater than that of the slot portion. This may readily be explained by the fact that the rotor body is at a low temperature and rapidly carries away the heat from the copper in the slots, whereas the ventilation of the ends is, at the best, somewhat inefficient.

The problem of insulating the rotor winding is not as difficult as that of the stator winding, as practically pure mica can be employed, both for the insulation between turns and for the slot insulation. The insulation of the retaining ring for the end windings can also be pure mica moulded into the rings. There is, therefore,

practically no destructible material in the insulation, and it may consequently be classed under heading (5) in the Table given in connection with the temperature tests previously mentioned.

Moreover, the margin of safety of the rotor insulation is much greater than that of the stator. The danger of breakdowns caused by deterioration of the insulation due to high temperatures is, in the author's opinion, considerably less in the rotor than in the stator, and higher temperatures may therefore be adopted for this part of the machine than for the stator.

There is no difficulty with regard to the question of other materials than mica being used in turbo rotor construction, as machines are now constructed in which nothing but asbestos tape is used for protecting the coils from dirt, and special asbestos "Bakelite" material is used for any packing blocks and distance pieces required. These materials are absolutely unaffected by temperatures considerably higher than those which it is suggested mica insulation may be subjected to.

Conclusions.

It is not possible to come to a conclusion with regard to the maximum temperature to which mica, as used at present in the insulating of electrical machines, may be subjected. To reach such a point it would be necessary to carry out tests covering a greater range of temperatures. The author is of the opinion, however, that the tests described show conclusively that there is no foundation whatever for the doubt which seems to exist, that the temperature limits laid down by the British Engineering Standards Association are too high, but that on the contrary they may with safety be increased. The tests show that mica insulation of the type employed in the construction of up-to-date alternators is capable of withstanding temperatures up to 200° C. without losing its electrical or mechanical properties.

In order to take full advantage of this, the maximum temperatures should be increased above the present limits. The author would suggest that 160° C. should be adopted as a limit for the maximum permissible temperature, based on embedded temperature-detector measurements for the stator winding, and by the increase-of-resistance methods for the rotor winding. Allowing a correction of 10° C. for the difference between the maximum temperature and the observable temperature, the latter would then be 150° C. This correction should refer to double-layer coils, with the temperature detectors placed between layers. In cases where the detectors are placed between the winding and the laminations, a greater correction factor would be necessary.

The values of these correction factors should be based on the voltage, as the difference in temperature between the inside of the insulation and the lamination will depend on the voltage, the thickness of the insulation varying with the latter. This has been recognized in connection with temperatures measured by thermometer on the surface of stator end windings, and there seems to be no reason why it should not be applied to the slot portion also.

In considering a departure from existing practice, the question of the advantages or otherwise which may be derived from such a change will naturally be of great importance. Admitting that no reduction in the margin of safety will result, which it is believed the tests have demonstrated to be the case, there is no argument against the change. There are considerable advantages to be obtained by adopting the higher temperature limits, and these may be summarized as follows:—

(*a*) In the case of machines running at the same speed, those with high temperature rating are cheaper.
(*b*) Higher efficiencies are obtainable.
(*c*) Larger units can be built, or
(*d*) Higher speeds can be adopted for machines of certain sizes.
(*e*) Less space is required.

Advantage (*d*) above is of great importance because an increase in speed reduces the cost, not only of the alternator itself, but also of the turbine. Better steam consumption is also obtained under certain conditions for the high-speed turbines. The increase in the temperature limits is therefore in some cases very far-reaching.

It is hardly necessary to emphasize the importance to all concerned in industry, of taking full advantage of the materials at our disposal, and of keeping abreast with developments elsewhere. In this connection it is of interest to note that there is a distinct tendency among American engineers to accept higher temperatures, and the two largest manufacturing companies have adopted a guarantee of 150° C. for the ultimate temperature of their turbo-alternator rotors, and one of the companies gives this guarantee for the stator windings.

The following table gives the number of machines

Table giving Number of Alternators and Total kVA *Capacity installed by the Westinghouse Electric and Manufacturing Company, Pittsburgh, U.S.A.*

Installed in	No. of units	Total kVA
1913..	10	173 200
1914..	8	139 450
1915..	19	223 450
1916..	21	143 700
1917..	24	381 525
1918..	27	354 850
1919..	28	332 125
1920 (to 1st May) ..	23	255 800
	160	2 004 100
On order	90	1 477 500
Total	250	3 481 600

installed and on order, based on a guarantee of a maximum temperature of 150° C. The sizes of these

machines vary up to 35 300 kVA in one unit, and operate at pressures up to 13 200 volts.

It is clear that makers who have to compete for orders for machinery of this class are placed at a disadvantage if they are unable to offer standard machines with the higher temperature limits.

It has already been pointed out that, in designing machines to meet specifications calling for low temperature guarantees to be obtained by thermometer measurements on external surfaces, manufacturers had to consider their safety in view of the high temperatures known to exist in the interior. Reliance had therefore to be placed on the makers in this respect, and it may be taken for granted that no manufacturer would advocate a departure from existing practice except with the knowledge that no risks would be incurred.

It is encouraging to note that some engineers in this country are disposed to accept higher temperature limits than existing standards, but it is of great importance from the point of view of standardization that it should be generally recognized and accepted that higher temperatures may be adopted without reducing the margin of safety.

The author wishes to express his thanks to the Metropolitan-Vickers Electrical Company, Limited, for affording facilities for carrying out the tests, and to Mr. Churcher of the Research Department for the interest and care taken in making the tests.

Discussion before The Institution, 27 January, 1921.

Mr. C. H. Merz : It is in large machines particularly that the adoption of any increase in the allowable temperature would show a benefit, and it should result in reducing the cost, a point of very great importance at the present time. It will not only cheapen the cost of the machine; it will also allow us to obtain, under all conditions, the maximum output. In other words, we should be better able to know how we stood if we were to work more definitely and continuously by the maximum recorded temperature on the switchboard. I think that, after reading this paper, with its very clear evidence of the advantages of working on definite total temperatures, as distinct from somewhat artificial temperature-rises, few of us will hesitate, wherever possible, to have such temperature detectors installed in alternators, and to test them in practice in the way the author suggests. I am sure that it will result in a very great step forward in the manufacture of large turbo-alternators. There are one or two questions upon which I should like the author to give more information. I presume that if we run the machines at a higher copper temperature the question arises whether there are any other parts of the machine that will suffer. The author refers to the question of distortion of the mechanical parts. Is there any question of the iron in the magnetic circuit suffering deterioration, and, if so, what is the limit in this case? Then there is the question of the insulation used between the laminations. Have we to consider the specification from that point of view or is it, in the author's opinion, entirely beyond the region of danger? I think that, apart altogether from this special way of dealing with the question of temperature, the electrical industry should permit higher temperatures. I do not know that those of us who have been concerned with high-speed turbo-alternators of recent years would in general, without going into statistics, be inclined to support the idea that there has been any great reduction in the number of failures, but I think we should agree that there has been a great reduction in the number of failures which could be clearly attributed to excessive temperature. If there has not been a reduction in the number of failures with turbo-alternators, say, during the last five years as compared with the previous five or ten years, I think it will probably be found to be due to the much harder conditions which the machines are called upon to fulfil in general, such as running at very much higher speeds for given output, and so on, in which case no doubt everything is stressed to a much greater amount. As systems have grown in size, the short-circuit and shock conditions which the windings of large turbo-alternators have had to meet have been very greatly increased. I think that there is a fairly simple way of dealing with this question of temperature in buying machinery at the present time. We are most of us accustomed, in inviting tenders for turbo-alternators, to allow the manufacturer to fill in the steam consumption he is prepared to guarantee, and I do not see why, for a machine of given size, he should not in the same way be allowed to state the temperature at which he proposes to run the machine. If any of us accept or allow too high temperatures, at any rate we shall have the satisfaction of knowing that we are helping to advance the knowledge of the subject.

Mr. R. Orsettich : The paper raises a serious question for designers and manufacturers of large units. I agree with the author in every respect when he states that the copper temperature is very different from the temperature one measures by ordinary means of surface temperature measurements, and that the correct way to arrive at the approximate temperature is by installing temperature detectors, but I disagree with him emphatically when he implies that, as we are exceeding the temperature which we thought we should get, we should go further and use much higher temperatures. I think that the tests which the author has made, and of which he gives interesting curves, have no real connection with the conditions under which the coils work inside the machine. They are not representative, because they refer only to one question, namely, to bending stresses. The coils in the machine, however, have to stand all manner of conditions, and the tests only cover one of them. Take the question of expansion. If one calculates the linear expansion of a coil in a machine, one will find that with a temperature difference of about 100 degrees C. there will be an expansion of about 3 mm. in a length of core of 2 metres, which is a very large figure. The insulation will probably expand to only one-third of that amount. The core

will also expand, but again at a different rate, and a certain amount of rubbing and perishing will be going on all the time. In addition, the copper core of the coil will tend to press against the insulation at the two ends which, being bent, are opposing its expansion. I do not think it would be possible to take account of these facts in any tests. Then there is the question of moisture. Machines generally work with air which is usually cleaned, or sometimes even cooled, by means of a water spray. The dampness of the air plays a very great part in the life of the insulation, and the condensation of ordinary air is also very important, and so are also the continual alternations of heat due to variations of load. If air which is hotter than the machine is admitted, say after the machine has been standing over the week-end, the machine will be entirely coated by moisture on the inside, and most stations to-day use all manner of dampers at the air inlet and outlet to avoid this, some even going so far as to provide a number of heating elements inside the air duct to maintain a reasonably even temperature under all conditions, whether the machine is working or not. This is a very important point. Some experiments were made at our laboratory, and it was found that the insulation of the coil after being kept for six months at a temperature of 150° C. had become more hygroscopic and could absorb as much as 10 per cent of its own weight of moisture. Again, in a machine taken out of service after a year or two, the coils are always covered with dirt and grit in spite of the use of the best cleaners. If the grit contains any metallic substance the life of the insulation will be considerably shortened as compared with that of a machine tested under pure-air conditions. The presence of dirt also causes a reduction of the radiation of the heat and, as a result, after three or four years the temperature is very much higher than before. That must be taken into account, as machines which might have a temperature of only 100° C. in the copper when new would have a very much higher temperature later on. If we start with 160° C. we shall probably have a temperature of 200° C., or even higher, after a few years. Vibration is also always present and plays an important part in reducing the life of the insulation. Our knowledge of what happens under these conditions is very restricted, and I think it would be very rash to adopt a policy concerning which there is not sufficient experience available.

Mr. F. H. Clough: In the past I think we have had machines working at much higher internal temperatures than we were aware of, particularly in the stator windings. These high internal temperatures were in a great measure due to eddy currents in the large conductors of turbo-generators, but in modern machines this difficulty has been largely overcome. I agree with the author that it is desirable to work to temperatures as high as possible, as in many cases the alternative to high temperatures in turbo-alternators is high mechanical stresses and, if the insulating materials employed are capable of standing high temperatures, it is obviously wise to take advantage of this, and so reduce the mechanical stresses. The most congested part of the design of the modern turbo-alternator is the rotor and, as the voltages in the rotor winding are quite low, I am inclined to agree with the author that we should adopt the temperatures he advocates, but I think that it is not advisable to go to quite such high temperatures in the stator. Stator windings are subjected to high voltages, and it is a little doubtful whether mica insulation which has been subjected to high temperatures is capable of withstanding high voltages as readily as it does when the binding material has not been affected by high temperatures. The currents in the rotor and stator are approximately proportional to one another and, therefore, if the machine is operated at the maximum current and temperature in the rotor winding consistent with safety, the stator winding under such conditions should be designed with a rather lower temperature. This should be quite feasible, as the mechanical stresses in stator windings are not very difficult to provide against, and it is very largely a question of proper elimination of eddy currents. The majority of manufacturers are inserting temperature detecting devices in their machines, and I hope that shortly, with the assistance of the operating engineers in the country, the information gained from the use of these detectors will enable us to estimate with considerable certainty what temperatures are safe in the interior of large machines.

Mr. J. Shepherd: I think the reason we are so accustomed to buying machines rated on temperature-rises by thermometer is that it has been practically impossible, until recently, to be sure what the internal and maximum temperatures are. Of course, the thermo-couple in small sizes and applied between windings is of comparatively recent introduction, and one cannot always be sure it reads correctly. Also, it is not always certain that it is placed at the hottest spot or, if it is near the hottest spot, that it is giving the highest temperature of the copper. It has been used in America to some extent, and Mr. Newbury is one of the most experienced users, but in a recent paper* he expresses considerable doubts as to the true and accurate readings of thermo-couples placed either at the bottom of the stator slot or at the top between the winding and the wedge. The question of ultimate temperatures must be considered, because higher temperatures may mean a considerable saving in plant. But I do not think that we can afford to sacrifice the running safety of the machine by suddenly accepting very high temperatures; and, further, a temperature of 160° C. may become 170° C. or 200° C. when the air ducts are choked. I do not think the author's experiments represent what is really happening to the insulation of the machine. There are far more factors involved than the bending of the insulation and treating it as a bar. There is the risk of the joints breaking down, and this of course is greater with increase of temperature. The vibration difficulty is not mentioned at all, and the temperature increment of the copper over that of the insulation and iron has not been considered at all in the experiments. If, however, we can restrict the insulation to its legitimate uses, viz. to insulate and to conduct the heat away, and then bind the insulation tightly against the metal in a similar way to the insulation between the sectors of a commutator, we can undoubtedly

* F. D. Newbury: "Some Practical Experience with Embedded Temperature Detectors," *Journal of the American Institute of Electrical Engineers*, 1920 vol. 39, p. 549.

run at higher temperatures than at present, because then we should have restricted the function of the insulation and relieved it of mechanical and vibration stresses. I see no reason why, if we can only hold the laminated bars together in a better way than by means of the insulation, a temperature of 150° C. could not be contemplated with safety when the insulated bars are supported in continuous winding slots, and soldered joints are replaced by welded or riveted ones.

Dr. S. P. Smith : The author points out that the standards which have been recommended in Report No. 72 of the B.E.S.A. are not those commonly used. That is unfortunate and is a state of affairs that should not be allowed to continue. It would be very desirable, in the interests of both manufacturers and users, if some agreement were reached, and rules to which all must work were developed and kept up to date. With regard to the question of detectors I am inclined to agree with the author's suggestions, because we have no better means at present of obtaining the temperatures inside the slots. Our troubles at present in connection with the design of turbo-alternators arise largely from ignorance. The measuring of surface temperatures is of very little use in informing us what is going on inside the slots. It is true that the temperature detector outside the insulation does not give us, strictly speaking, the temperature of the copper, but it certainly gives more useful results than the mercury thermometer gives. There are, I know, objections to its use from the manufacturer's point of view ; it has to be applied with great care ; also, unless several are used, they may not be of much value. It is necessary to choose, as judiciously as possible, a certain number of points at which to place thermo-couples ; and, in addition, in a station there must be a registering instrument with a multiple-contact switch, which should be read at regular intervals and the results recorded. Though such a method has defects, it tells us something definite about the distribution of the temperature along the core. The author mentions the well-known axial and radial ventilation methods, and nowadays these have to be combined in large units. By means of temperature detectors, we could get more information as to the superiority of one method over the other. In addition, we should be able to check our calculations and the work in the shops. From Field's curves we can calculate fairly well what the increased copper losses will be for any proposed arrangement, but to predict the copper losses accurately we must know the temperature at which the copper will have to work. Further, until we know the temperature inside the core we cannot say whether the insulation between the plates is being subjected to too high a temperature or not. Thus, it is not safe to increase the present limits until we know what the existing temperatures are. There are, of course, many views as to what should be the final temperature. Mr. Orsettich mentioned the expansion of copper when heated, and it seems to me that that alone is a sufficient argument in favour of using temperature detectors. There is another point which is very important to all designers. The limiting factor in many designs, especially with large machines, is the amount of air that can be forced through the machine. The skill of the designer is exercised in getting this limited quantity of air through the machine in the most efficient manner, and in reducing the losses as much as possible in order to get the largest possible output from a given weight of material. In this connection also, the temperature detector would be of considerable assistance. Therefore I think we ought to make a practice, as far as possible, of introducing these detectors and recording the results for the information of all concerned. The method of finding the mean temperature-rise by means of resistance increase is well known, but the form given in the B.E.S.A. Report No. 72 entails the use of a table, and is not as simple as it should be for those who have often occasion to find the average temperature of rotor windings. For their assistance I should like to submit this formula :—

$$t_2 = \frac{R_2}{R_1}(234{\cdot}5 + t_1) - 234{\cdot}5\,^\circ\mathrm{C}.$$

or, on the Fahrenheit scale,

$$t_2 = \frac{R_2}{R_1}(390 + t_1) - 390\,^\circ\mathrm{F}.$$

The resistance R_1 is measured at temperature t_1 ; R_2 is the resistance when hot ; and t_2 is the mean final temperature of the winding. The average temperature-rise is, of course, t_2 minus the temperature of the cooling air. For any particular machine installed in a station a graph of the above equation should obviously be plotted, so that the temperature of the rotor winding can be recorded from day to day.

Mr. A. R. Everest : The question of thinking in terms of higher temperature values can be considered under three heads. First, we have the higher temperature values due to the adoption of new methods of measurement. The International Electrotechnical Commission pointed out years ago that the expectation of life of a machine depended upon the highest temperatures to which its insulating materials are subjected, and that it would be desirable to measure the maximum temperature occurring within the machine. Since that time, temperature-detecting devices which may be embedded within the machine have been developed in a practical way, and the only remaining point in this connection is the establishment of the due value of increased temperature which should be recognized for this method as compared with the more familiar temperature limits applying to the same machines when measured by the older methods. The determination of these proper limits is now under active consideration by the British Committee. The application of this method is particularly desirable for the stators of turbo-alternators, since otherwise only surface measurements are available, temperature measurements by resistance of the windings immediately the load is taken off being impracticable for the stator of a turbo-alternator on account of the disturbance created by the residual magnetism of the field while the machine continues to run. It is evident, therefore, that these new methods of measurement constitute one necessity for thinking in terms of higher temperatures. As a second consideration we have the modern

employment of materials actually intended to operate at higher temperatures, i.e. the extended employment of mica insulations. The use of embedded temperature detectors is not practicable in the rotor windings of a turbo-alternator; the temperature of these parts should be determined by the increase of resistance, employing for these measurements suitable special brush-gear on the slip-rings. We have, therefore, also to become familiar with the higher temperatures associated with the use of these modern insulations. The third consideration is the proposal made at various times that, when employing material such as mica, the actual temperature limits occurring in good modern machines might be safely increased. It is my impression that the author's argument for the recognition of higher temperatures applies principally to the first and second considerations mentioned above, rather than to proposals for actually increasing the operating temperatures beyond the limits found to-day in a good machine. Regarding the reference to the tests upon the mica-insulated machine at Niagara, I think we must be careful not to draw erroneous conclusions. The operating voltage of this generator (2 200 volts) is only one-third to one-fourth of that which would be considered proper to-day for the thickness of mica-paper insulation which was actually provided, and the fact that the machine continued to operate at this abnormally low voltage after the insulation had been subjected to the damaging effects of the high temperatures which the investigation disclosed is no proof that the modern machine with thickness of insulation duly proportioned for its working voltage could safely operate at the same temperatures. Incidentally, it is of interest to know that when these tests were made at Niagara there were in an adjacent power house 11 generators of another make which contained no mica insulation, but were designed to operate at low temperatures. These generators had been in service for 12 or 13 years without a single failure, beyond one due to an operator's mistake. In connection with this question of high temperature limits on turbo-alternators it is of interest to note the opinion recently expressed by Mr. P. Torchio, chief engineer to the New York Edison system. A few years ago Mr. Torchio was supporting the arguments in favour of increasing the limit of temperature permitted on mica insulation, but in a paper * before the American Institute last summer dealing with the failures of large turbo-generators he said: "Too great range of operating temperatures is bound to cause generator failures. Mica insulation in hydro-electric generators operated at steady load will last indefinitely at high temperatures, while under similar conditions mica insulation in steam turbo-generators operated intermittently will fail."

(*Communicated*): When a machine is equipped with suitable embedded temperature detectors it is easy to arrange that the internal temperature of the machine shall be continuously indicated by an instrument on the switchboard. This practice is very common in the United States. It is sometimes suggested that the chief advantage of such an arrangement is that it permits operation at outputs limited only by the maximum temperature, thus utilizing increased output capacity at times when the temperature of the cooling air is very low. This argument is somewhat difficult to follow. The output of a turbo-generator set is physically limited by the capacity of the steam turbine. If this is large enough to supply the full output from the generator in very cold weather without exceeding assigned temperature limits, it appears that during the chief part of the year, when the cooling air is initially warmer, the set will be obliged to work at less than the available capacity of the steam turbine. If the set is designed to utilize the full capacity of the steam turbine with a moderate temperature-rise on the generator, the limitations of the turbine will prevent advantage being taken of increased generator output, which might appear possible in cold weather. But the switchboard temperature-indicating device performs another very useful function. If the ventilating air is not properly filtered and commences to clog the air-ducts, the temperature indicator at once gives warning of an increased temperature.

Mr. A. B. Field: The qualities upon which we place most importance are, I think, reliability and efficiency, and of these reliability is generally the more important. I think it has not always been recognized in this country that in limiting the manufacturer too closely in the matter of temperature the purchaser is actually getting a machine less reliable, and very frequently less efficient, than that with which the manufacturer would otherwise supply him. I refer at the moment particularly to rotor temperatures. At the present time, in this country, the great bulk of the demand for turbo-generators can be met by machines running at a speed of 3 000 r.p.m., as 50-period units are offered at this speed up to about 14 000 kW normal rating, or 20 000 kVA maximum rating. Now for this speed a solid forging is rightly used for the rotor, and simplicity and soundness of mechanical design are of the first importance. Methods of construction now available, using all mica insulation, allow us to use a simple rotor without ventilation provided a reasonable temperature-rise by resistance be accepted; whereas insistence upon a temperature which would have been suitable for the older methods of insulating merely encourages the designer to indulge in undesirable rotor-ventilating schemes and forces him to distort the magnetic and electric proportions of the design. We should, on the contrary, try to obtain the greatest possible mechanical soundness and operating reliability in these machines, together with high efficiency, rather than certain results on paper. The general idea of specifying temperature limits by thermometer measurements was carried over, some 12 or 15 years ago, from the smaller machines and machines of other types to the case of large turbo-generators, but, although American manufacturers continued to supply turbo-generators on this selling basis, they quickly discovered the neighbourhood of the actual internal operating temperatures corresponding to thermometer temperature-rises of 40 and 50 degrees C., and they gradually evolved insulating methods to suit both the rotor and the stator. Such methods are now in use in this country and, as they are not hedged in by patents, purchasers should insist upon their use. It is useless to ignore this

* *Journal of the American Institute of Electrical Engineers*, 1920, vol. 39, p. 548.

position, and merely to think that we have recently found improved means of internal temperature measurement for which but a small extra allowance, as compared with the figures given by thermometer, need be made.

Mr. H. W. Taylor: The question of the internal temperatures of turbo-alternators came prominently to the attention of designers a few years ago. It arose in the natural course of development as larger ratings were built. There is no doubt that some machines built at that time attained on load internal temperatures corresponding to the highest figures quoted by the author, but nevertheless they are still giving satisfactory service. The question arises as to whether such high temperatures can continue to be tolerated by manufacturers in their designs for the still larger machines which are now being built and proposed. The vital parts of an electrical machine consist of a heterogeneous assemblage of unmechanical details, such as thin steel laminations, copper bars of comparatively small cross-section, and insulation consisting of an assemblage of mica flakes. The problem of moulding these components into a satisfactory complete turbo-alternator design presents considerable mechanical difficulties, and it is probable that excessive temperature variations between different parts of the structure tend to increase these difficulties. While agreeing that the present standardized temperature limits are unnecessarily low, I do not think it will be necessary, with the refinements in the details of the design which are continually being made, to recognize the highest temperatures which the author has proposed. In any event it can be definitely stated that large machines are being built to-day which are cooler than the smaller machines of a few years ago.

(*Communicated*): I think the author should have devoted a short portion of the paper to explaining with what difficulties designers have to contend in building machines of the present ratings to comply with narrow temperature limits. Fig. A shows the relative proportions, without any attempt at the inclusion of full mechancial details, of the largest machine which was being produced by one large firm in or about 1907 and of the largest machine which is being manufactured at the present time. The ratings are in the ratio of 1 : 15. The diameter of the rotating part has been increased nearly 40 per cent and has attained a limit from the point of view of mechanical stresses. The length of the machine has increased practically three times, and the difficulty of carrying the cooling medium on the outside of the machine to the innermost portions and again to the outside of the machine will be appreciated. The increased rating has, however, not only been obtained by increases in the mechanical dimensions, but also by the use of heavier copper loading in both stator and rotor, the relative dimensions of which are again shown in Fig. A. If the heat to be carried

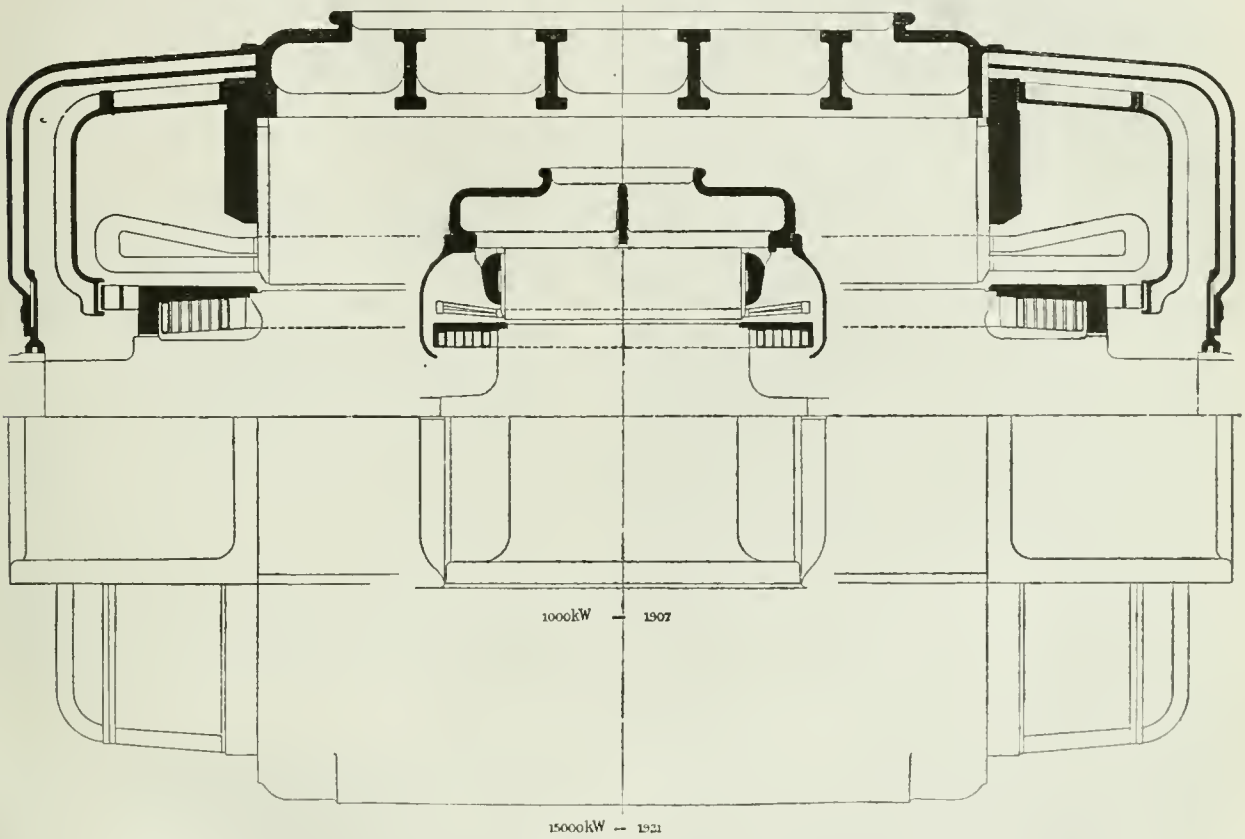

FIG. A.—Relative proportions of large 3 000-r.p.m. turbo-alternators built in 1907 and 1921.

away from them be taken as proportional to the volume of winding used it will be further appreciated that not only has the cooling medium to travel over greater distance, but it has to be provided in larger quantities to carry away with it an increased quantity of heat.

Mr. H. M. Dowsett (*communicated*): The author has raised the question of temperature limits in connection with its application to large turbo-alternators, but this subject may be discussed with advantage in its relation to other classes of machines covered by the Reports of the British Engineering Standards Association. Take the case of the high-frequency alternator which is to-day a commercial proposition up to units of 200 kW. As it is a resonance machine its possible output is limited solely by its internal losses, which of course increase as the copper and iron temperatures increase and, suitable insulation being provided, it is the limitation of these losses which in this case should determine the permissible maximum temperature. It must be remembered that the smaller the section of the conductor and its insulation the larger will be the possible number of poles and the higher the frequency, so that to impose a low temperature limit may prevent the designer from obtaining as high a frequency from a given size of machine as he otherwise might do. The small direct-current high-tension machine is another case which may be quoted. Its rotor usually runs at high speed, and it has many turns per slot, heavy slot insulation, solid end insulation and poor ventilation. In any case the heat conduction is poor, and the temperature of the windings tends to be high, but this should not increase the risk of failure if a systematic use is made of the vacuum impregnating process for thoroughly insulating the windings at every point with one of the many forms of fluid bakelite now obtainable, which on solidifying is able to stand the working temperature without flowing. Finally I would refer to machines built for service in the tropics. They have to work under conditions of moist heat and of heavy condensation which is very conducive to chemical change, and only those insulating materials should be used which after prolonged exposure to such conditions show no chemical reaction whatever. In this instance one would not advocate an increase in the existing temperature limits, but one would like to see an addition to the standardization rules to the effect that no absorbent material of any kind should be employed for such machines, and that plenty of clearance should be allowed between live parts in order to maintain a good value of insulation resistance.

Professor M. Walker (*communicated*): The data given by the author relating to the effect of high temperatures on insulated stator conductors are exactly what the electrical designer and the user of electrical machinery want. We know that mica insulation will resist the long application of high temperatures, but we have no quantitative measurements showing the effect of long application upon the mechanical properties of the coil. The figures that he gives are very reassuring to users of electrical machinery, many of whom know that the temperature inside the copper in their machines is considerably over 100° C. As stated in the paper, the formula by which the curves in Fig. 4 were obtained is not really applicable to the case of stator bars. A calculation of the temperature distribution inside a stator conductor, when there is heat conduction both along the conductor and through the walls of the tube, affords an interesting problem; the solution is comparatively simple, and I give it for the benefit of electrical engineering students.

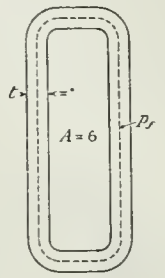

Fig. B.

Let t = thickness of the insulation in cm. (see Fig. B).
A = sectional area of copper.
p_f = periphery (mean) of insulating tube.
$s = (0{\cdot}0012/t) \times (p_f/A)$ = watts per cubic cm. conducted through the walls per degree C. rise in temperature of copper above iron.
$\rho = (1{\cdot}6 \times 10^{-6} \times T)/235 = aT$.
T = temperature of copper + 235° C.
i = current density (allowing for eddy currents).
T_i = temperature of iron + 235° C.

Then at a distance x from the centre of the machine

$$\int ai^2 T dx - \int s(T - T_i)dx = -3{\cdot}5\frac{dT}{dx}$$

That is to say, the negative temperature gradient multiplied by the heat conductivity of copper is equal to the difference between all the heat generated, $\int ai^2 T dx$, and all the heat dissipated through the tube, $\int s(T - T_i)dx$.

Integrating, we get $3{\cdot}5\dfrac{d^2T}{dx^2} - (s - ai^2)T + sT_i = 0$

and $$T = C_1 \cosh \beta x + C_2$$

when $C_2 = sT_i/(s - ai^2)$, and C_1 depends upon the length of machine and the external temperature of the copper, and β^2 is $(s - ai^2)/3{\cdot}5$.

Particular case: When $x = 88{\cdot}6$ cm. let

$$T = (70° + 235°)\text{ C.} = 305°\text{ C.}$$

and let $$T_i = (60° + 235°)\text{ C.} = 295°\text{ C.}$$

Let $t = 0{\cdot}3$ cm. and $p_f/A = 2{\cdot}3$, then $s = 0{\cdot}0092$.

Take the virtual current density at 500 amperes per square centimetre, equivalent to a density of 350, and $K_d = 2$. Then

$i^2 = 250\,000$, $ai^2 = 0{\cdot}0017$, $(s - ai^2) = 0{\cdot}0075$,
$\beta^2 = (s - ai^2)/3{\cdot}5 = 0{\cdot}00215$, $\therefore$ $\beta = 0{\cdot}0463$, and

$$\frac{sT_i}{(s - ai^2)} = \frac{0{\cdot}0092}{0{\cdot}0075} \times 295 = 362°\text{ C.}$$

Filling in values for $x = 88{\cdot}6$, we get

$$305 - 362 = C_1 \cosh 0{\cdot}0463 \times 88{\cdot}6 = C_1 \cosh 4{\cdot}1.$$

Now $\cosh 4{\cdot}1 = 30{\cdot}178$, therefore $C_1 = -1{\cdot}9$, and $T = 362 - 1{\cdot}9 \cosh 0{\cdot}0463x$.

Taking the particular case in which the stator has a total length of iron equal to 177·2 cm. (about 70 in.), in which the temperature of the teeth is 60° C., that of the copper outside the slots is 70° C., and in which the other values are as above, the expression for T takes the form

$$T = 362 - 1{\cdot}9 \cosh 0{\cdot}0463x.$$

The full-line curve in Fig. C shows the temperature distribution under these conditions. It will be seen that the effect of heat conduction along 35 inches of conductor only reduces the temperature in the centre of the machine by about 2 degrees C.

If, however, we apply the formula to a machine having a total iron length of 32 inches and an insulating tube 4 mm. thick, we find that the conduction of heat along the copper towards the end-connectors reduces the temperature in the centre by 35 degrees C. below the figure that would be attained if there were no heat conduction along the conductor.

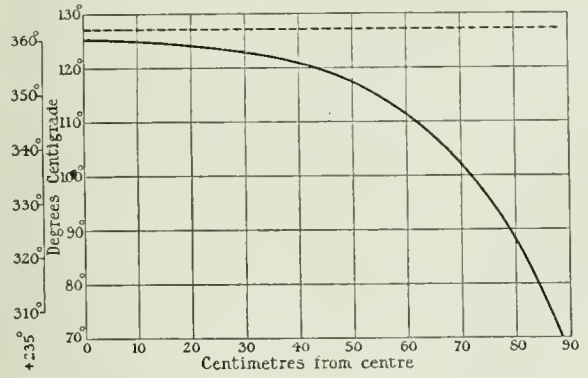

Fig. C.

It is very desirable that thermo-couples, or other temperature-measuring appliances, should be placed in turbo-generators so as to enable the operator to determine accurately at any time the value of the maximum temperature of the stator copper. For some reasons it is undesirable to place the thermo-couple directly adjacent to the copper, even though a conductor close to the neutral point is chosen, because it may be desirable to insulate the neutral point, and in that case a third harmonic might cause considerable difference of potential between the junction and earth. I suggest that a better plan is to place the thermo-junction at a point half-way through the thickness of the insulating tube when the insulation of one of the stator coils is being made up; it will then register approximately half the temperature difference between the copper and the iron. It may be more exactly calibrated if necessary by placing a thermo-couple next to the copper, the latter couple being cut out ultimately. It would then be easy to arrange the scale of the temperature-indicating instrument so that it reads the actual temperature of the copper. While I agree that the data in the paper sufficiently establish the fact that machines can be operated at high temperatures without suffering injury, I do not agree that one should deliberately aim at temperatures as high as 150° C. in the case of stators. In considering the amount of copper to use in designing the stator conductors, one must have regard to the fact that the greater the amount of copper the greater the amount of eddy-current loss. The eddy currents that we must have in view are not only those that arise from the field due to the magnetomotive force of the current in the slot, but also those arising from the cross field and the longitudinal field due to the saturation of the teeth. These cross fields and longitudinal fields are exceedingly strong in many modern machines and, unless very great care is taken in the lamination and crossing-over of the conductors within the slots, the eddy-current losses arising from them may be greater than the normal copper losses. With very careful lamination and crossing-over, however, it is possible to increase the copper in a stator slot so as to make the total cross-section considerably greater than it is in many modern machines, and to reduce the copper losses at full load by many kilowatts. In a case that I recently worked out I found it possible to reduce the copper losses in a 20 000-kW machine by 40 kW, while the total cost of material and labour did not exceed £600. If the interest on this is taken at 10 per cent, the cost to the consumer need not exceed £60 per annum, whereas the saving of 40 kW is worth several times that amount. At the same time, the internal temperature would be so much reduced that the fibrous materials, some of which still enter to a certain extent into the composition of the insulation, would be very much better preserved. Reliability is, of course, the first consideration, and we must only consider first cost in conjunction with efficiency.

Mr. G. A. Juhlin (*in reply*): The views of Mr. Merz on the question of higher temperature limits are of very great interest and value, and it is gratifying to note that he is in general agreement with the suggestions made in regard to increasing the temperature limits, as well as to operating machines on final temperature rather than temperature-rise. With regard to the question as to whether increasing the copper temperature would cause difficulties with any other parts of the machine, I would say that there would be little or no increase in the temperature of the laminations of the machine; and, as we are operating to-day at temperatures far removed from the limits for the insulation used between the laminations, we should be entirely beyond the region of danger. Mr. Merz raises a very important question in connection with the number of failures of turbo-alternators, and in this connection I would refer to Fig. D on which are shown the total kVA output installed and running since 1911, as well as the number of machines. In addition to this the total kVA of breakdowns and the number of breakdowns have been shown for both stators and rotors, together with these failures given as a percentage of the total kVA of plant running at the time of the breakdowns. It has not been possible to obtain accurate records of the breakdowns further

back than 1917, but it is of interest to note the reduction which has taken place in the number of failures from that date. It should also be pointed out, in connection with the number of failures, that many of these are very minor ones, especially those in connection with rotors, and in no case is a breakdown recorded on a modern type of rotor, most of the machines being very old. There is no case on record where the insulation breakdown can be attributed to damage of insulation due to overheating. As pointed out by Mr. Merz, the increase in speeds for a given output and the growth of the systems on which machines operate to-day have an important bearing on the question of failures.

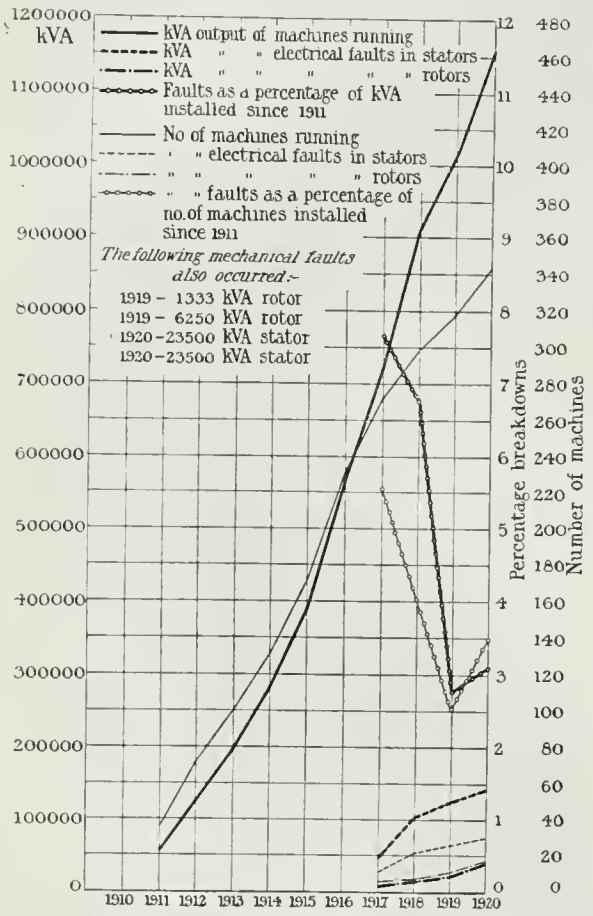

FIG. D.—Number and output of turbo-alternators in service.

I think the broad-minded view taken, when he suggests that manufacturers should be allowed to state the temperature at which a particular machine should be run, must commend itself to all concerned.

I agree with Mr. Clough that the rotor is the most congested part, and higher limits may be adopted for this portion of the machine than for the stator. I do not think that the temperature suggested in the paper is the highest to which rotor insulation may safely be subjected.

I cannot quite follow Mr. Orsettich when he states that it is implied in the paper that we should increase the temperatures because we are exceeding the temperatures we thought we should get. What I suggest is, that we should recognize the fact that machines have been operating with satisfactory results for many years at higher internal temperatures than are now accepted. Manufacturers have known this to be a fact for many years, and I think it is to the advantage of the industry as a whole that it should be known to all concerned. It is probable that opinion in America is divided on the question of maximum temperature-rise, but I would suggest the possibility of this being due to the fact that different types of insulation may be in use, and that different engineers have had varying experience depending on the class of insulation employed in their machines. I hardly think that the argument put forward by Mr. Orsettich, that the station staffs do not use the detectors, should be taken as evidence against the installation of embedded temperature detectors. Such a state of affairs would indicate indifferent operation methods, and does not seem to be general in America. I quite agree with Mr. Orsettich that insulation is subjected during operation to other stresses than bending. I think bending stresses are the most important, and the bending tests were chosen because they indicated as well as any other test the condition of the insulation. With reference to expansion, there are many machines having longer cores than 2 metres which have been in operation for several years at copper temperatures as high as, if not higher than, 100° C. with perfectly satisfactory results. I quite agree with the provision of dampers as a protection against the possibility of moisture condensing on the windings due to changes in temperature of the atmosphere. It has been the standard practice of the company with which I am associated to install such dampers, but I would suggest that this precaution is necessary at whatever temperature the machine is operating. I am of the opinion that machines operating at a high temperature will be less affected if operating with a wet filter than those at low temperatures. The figures giving the percentage moisture absorbed by insulation after heating at 150° C. are interesting, but would have been more valuable if the type of insulation had been stated. I think Mr. Orsettich provides a very good argument for the embedded temperature detector when he states that after a few years' operation the temperature may increase due to accumulations of dirt. Such increases in temperature cannot, as a rule, be detected by the thermometer measurements on the external surfaces, but will be indicated by embedded detectors. I think it follows that if we eliminate the necessity for making allowances for increases in temperature due to such causes, it is possible to operate at higher temperatures because of the knowledge that our maximum limit will not be exceeded.

Mr. Shepherd seems to have some doubts as to the accuracy of the thermo-couple. It is our practice to provide several couples placed in similar positions in the slots in order to provide against possible errors in any of the couples. With regard to the question of the accuracy of the readings of thermo-couples placed either at the bottom of the slot or at the top between the coil and wedge, this point has been recog-

nized, and increased hot-spot corrections have been adopted by the American Standards Committee for couples so placed. Whether all these corrections are sufficient is open to some doubt. I entirely agree with the view that we cannot afford to sacrifice the running safety of machinery, but I do not think this would be the case in adopting the temperature limits suggested; and in connection with the possibilities of the temperature limits being exceeded due to ventilating ducts becoming choked, this would be taken care of by the fact that we are measuring the internal temperatures, and should therefore be able to record any increase due to reduced ventilation. With regard to the question of joints, Mr. Shepherd has some doubts as to these being satisfactory, but I think it should be borne in mind that the temperature of the joints will, of course, be very much lower than the limits suggested for the centre of the machine, due to the fact that the end windings are well ventilated. It is our standard practice to-day to make mechanical joints and, in addition to this, a sweated sleeve, so that I do not think any fear need be entertained on this point. I am pleased to know that Mr. Shepherd is in general agreement with the necessity for increasing the temperature limits.

Dr. Smith has very clearly set forth some of the difficulties confronting the designer of high-speed turbo-alternators of large outputs, and I am glad to know that he is generally in agreement with the use of embedded temperature detectors. I think the formulæ given for the calculation of temperature-increase from the resistance method are very useful, as they obviate the use of any specially prepared tables.

Mr. Everest raises a very important point when dealing with the question of actual methods employed for the determination of temperatures. If new methods are adopted, clearly the limits should be changed so as to conform with these new methods. With reference to the point raised in connection with the thickness of the insulation on the Niagara machines, it may be true that the insulation thickness on these machines is greater than would be employed for a similar voltage to-day, but it should not be overlooked that the type of insulation is also different from that we are now employing, so that I think the conclusions drawn from these tests are not erroneous. In any case these tests are not the only evidence in support of the contention that mica insulation will withstand considerably higher limits than have been considered safe up to the present. Mr. Everest quotes the opinion recently expressed by Mr. Torchio, Chief Engineer of the New York Edison system. Opinion from such a quarter is undoubtedly of great value, but I think it is easy to lay too much stress on it, as we have no information as to the actual class of insulation referred to. Mr. Everest expresses some doubt as to the feasibility of the suggestion of taking advantage of operating turbo-alternators on the ultimate temperature and thereby taking advantage of a lower cooling-air temperature, because of the physical limitation of the steam turbine. It is not uncommon, however, to find specifications where overload capacity is called for on the turbine, and in such cases it would certainly be possible to take advantage of low cooling temperature.

The question of mechanical soundness and reliability of the turbo rotor raised by Mr. Field is, in my opinion, of extreme importance, and I agree with him that this point is in many cases of greater importance than the question of temperature. Simplicity of the rotor construction is undoubtedly of primary importance, and, as rightly pointed out, increased temperature limits enable us to simplify the rotor construction considerably.

I agree with Mr. Taylor as to the difficulties confronting the manufacturer in constructing the large turbo-alternator, but I think these difficulties have been overcome successfully. While one agrees that there may be no absolute necessity to increase the temperature limits, it does not, to my mind, follow that we should not take advantage of the higher limits if this can be done with safety, which I believe to be the case. I quite agree with Mr. Taylor's statement that large machines are being built to-day with lower temperatures than in the case of the smaller machines of a few years ago.

With regard to the point raised by Mr. Dowsett in connection with high-frequency machines, it is probable that other limitations than the insulation will determine the temperature-rise of such machines. Referring to machines built for the tropics, I think that the question of ultimate temperature instead of temperature-rise is of more interest, as this eliminates the question of the air temperature, and I do not think it would be necessary to adopt, for the ultimate temperature, limits lower than those in machines built for operation in this country. The question of the use of non-hygroscopic material in damp and humid climates has of course received the closest consideration of manufacturers of electrical machinery, and this class of material is employed as far as possible.

The formulæ given by Professor Walker are of great interest, and will be useful for obtaining approximate figures for the temperature gradient in the axial direction. The suggestion to place the thermo-couples half-way through the thickness of the insulation tube is very interesting, but it is of course necessary to select the bars next to the neutral point, as the value of the insulation would be reduced to one-half if the couples were placed half-way, because the thermo-couples would be earthed. Even with this scheme, however, it would be necessary to estimate the temperature-drop between the point of the couple and the copper itself, so that it is doubtful whether any real advantage would be gained. I quite agree with Professor Walker that close attention must be given to the question of eddy currents when increasing the depth of the copper, and in some cases it is necessary to consider the stray losses due to the saturation of the teeth. Most designers have probably from time to time investigated this point and, in the cases of some large machines which were investigated from this point of view, the losses due to the stray flux caused by saturation were found to be comparatively small. Professor Walker emphasizes the fact that reliability must be the first consideration, and I think this is a point with which all engineers heartily agree.

North-Western Centre, at Manchester, 25 January, 1921.

Dr. S. F. Barclay: The author has not made quite clear the relationship that his proposals bear to present-day practice. He says in one part of the paper that he proposes to run machines with a higher temperature-rise than is at present permitted. On the other hand, however, he rightly points out that actual "hot spot" temperatures exceed considerably the rise specified by thermometer. On page 284, in connection with some calculations, he refers to a hot-spot temperature-rise of 78 degrees C. above the cooling air for a 6 600-volt machine, and of 98 degrees C. for a 10 000-volt machine. He points out that although these figures may appear to be high, if the temperatures of existing machines were measured they would probably be found to be in their neighbourhood, if not higher. Most people will be inclined to agree with the author in his opinion with regard to the actual temperatures of machines built and passed to the conventional specification of a 40 degree C. rise measured by thermometer, but how does he reconcile this opinion with his proposal to increase the temperature-rise to 75 degrees C. (actual temperature)? Clearly he is recommending the adoption of a specification that will ensure hot-spot temperatures being lower than he considers they are at present, a state of affairs that is not consistent with the substance of the paper. Another point to which reference should be made is the author's proposal to measure the temperature of the rotor winding by the resistance method. In consequence of the good cooling of the part of the rotor winding that is embedded in the rotor, and the relatively poor cooling of the part of the winding that is enclosed by the end coils, the hot-spot temperature of the rotor must be considerably greater than the average temperature. Clearly then, so far as the insulation is concerned—and it is from that point only that the author considers the case—the temperature-rise that is suitable for the stator where the hot-spot temperature can be taken fairly accurately is excessive for the rotor with which the hot-spot temperature is an unknown quantity. The author might be disposed to consider that the difference between the average and the maximum temperatures of the rotor should not be very great with a well-designed machine, but he has to remember that if his proposal were adopted it would be applicable to badly designed as well as to well-designed machines. For this reason I would express the considered opinion that an ultimate limit of temperature of 160° C. for the rotor as determined by the resistance method is excessive. In several parts of the paper the author refers to the importance of economy in first cost and in operation, but he has not considered what appears to me to be the most important quality of all, i.e. reliability of operation. In my opinion, for generating plant, reliability should have the first consideration from all concerned, and to impair this first essential for any small gain in first cost or efficiency is an unsound policy. An unduly high percentage of failures with turbo-alternators continues to take place from unknown causes, and it might quite well be that relative movement due to large temperature variations may account for some of these failures. Before higher temperatures are adopted it is important that the matter should be considered from other standpoints than merely that of the suitability of the insulation to withstand high temperatures.

Mr. J. A. Kuyser: The author states at the commencement of the paper that the B.E.S.A. Rules allow higher limits than the old practice of guarantee by thermometer. There is a certain amount of misunderstanding on this point, and I should like to bring forward some arguments to show that the B.E.S.A. Rules mean, on the contrary, lower limits and larger machines than the old guarantee of 40 degrees C. as measured by thermometer. This will be best brought out by a comparison between a machine designed on the old basis and one designed to the B.E.S.A. Rules. If we take a 1 000-kW machine designed on the old basis this would have a temperature-rise on the outside of the stator iron of 40 degrees C. by thermometer, and the same on the stator end-windings. It is practically impossible to measure the rotor temperature, and for a normal design this would be approximately 75° C. if measured by the resistance method. If this same machine is operated at the specified overload of 25 per cent the temperatures would be as follows: Stator iron 45° C., stator end-winding 60° C., and rotor, by resistance, 90° C. Now the machine rated to the B.E.S.A. Rules must have a maximum continuous rating corresponding to the 25 per cent overload on the machine designed on the old basis, or 1 250 kW, and the temperatures specified in the Rules are: Stator iron 75° C., stator end-winding by thermometer 50° C., and 55° C. by resistance, rotor winding by resistance 75° C. In the machine rated to the B.E.S.A. Rules we must therefore operate at a considerably lower temperature in the rotor winding (75° C. against 90° C.) also in the stator windings (50° C. against 60° C.) and this will result in a larger and more expensive machine. The price of the latter machine will therefore be approximately 10 per cent higher than that of the former. A further point is whether for this extra outlay extra value is obtained, and in this respect I will show that instead of more value a lower efficiency and poorer performance will generally be obtained. The stator iron, according to the B.E.S.A. Rules, can be operated at a much higher temperature-rise than the corresponding machines based on the 40 degrees C. rise, namely 75° C. instead of 45° C. (for overload conditions). To obtain the largest B.E.S.A. rating out of a certain machine, and for a given cost, the designer will therefore take advantage of the extra allowed on the stator iron and increase the core density to the maximum possible. This can be done by using a larger rotor diameter for a certain stator diameter. The larger rotor will be necessary to find room for the extra rotor copper. A further reduction may be made in the length of the air gap to reduce the rotor heating. In general, everything would be sacrificed to reduce the temperature of the rotor winding, because

that is as a rule the most dangerous guarantee in the whole contract. All these changes will result in a poor design compared with the machine designed on the old basis, in which the designer has practically a free hand except for the stator iron temperature. The machine will have a high flux and few ampere-turns, whereas a turbo-alternator should be designed for a minimum flux and maximum ampere-turns. The iron losses and the windage losses will be high, due to the larger rotor diameter. The total temperature in the stator, and the hot-spot temperature, will be higher and the efficiency will be reduced. All these disadvantages can be eliminated to a large extent by specifying the more reasonable temperature limits recommended in the paper, particularly for the rotor which is at present insulated by means of fire-proof materials only. By specifying the higher limits, operating engineers will obtain better designed machines having higher efficiencies at a lower cost.

Mr. A. E. Clayton: It seems to me that one result of this paper will be that purchasers of turbo-alternators will be quite convinced that any rating on the basis of temperature-rise by thermometer is absolutely hopeless. When we read on page 284 that a 10 000-volt machine has an actual temperature-rise of 98 degrees C. instead of 40 degrees C. as measured by thermometer, it is obvious that any rating based on temperature measurement by thermometer is of little use. With regard to the measurement of the actual temperature of the winding, the author is apparently in favour of the use of thermo-couples. I should be very glad if he would give us in rather more detail the reasons why he has not adopted the method of increase in resistance. I remember reading some few months ago of an installation in France where temperature indicators of the resistance type had been installed in hydro-electric plant for supplying energy to a single-phase railway. I should like the author to tell us why he has adopted the thermo-couple. Of course a thermo-couple enables one to obtain the temperature in a particular spot very accurately, but it seems to me that the resistance method is rather less delicate and more suited to actual operating conditions. It seems to me also that in the case of a rotor the author has only allowed a difference of 10 degrees C. between the temperature of the hottest part and the temperature measured by increase of resistance. I think he has come to the conclusion that 160° C. is a safe temperature at which to run mica insulation. If he were to take a temperature of 150° C. by resistance for the rotor I should imagine that it would correspond to a maximum temperature of well over 200° C. in many cases, the actual figure depending partly upon the type of ventilation, i.e. whether axial or radial.

Mr. G. F. Sills: The author showed slides of a number of coils which have been tested. There appear to be four conductors per slot. Presumably these conductors are not in parallel and, if so, there is presumably mica between each individual conductor. I raise this point because in a number of instances in the past, although the slot insulation surrounding the conductors has been mica, the insulation between the conductors forming the coil has not always been of the same material, and in view of the high temperature suggested it would be necessary to have nothing but mica. I should like to know what effect the high temperatures have on the core, particularly if paper insulation is used between the laminations, bearing in mind, of course, that the temperature is lower on the main body of the core. Have the Americans found any core trouble due to the expansion of the core bolts when using these high temperatures? Perhaps the author will also tell us what insulation was used between the stator laminations on some of the machines he mentions. References are made in the paper to a test carried out on water-wheel generator coils. I should say that these coils, at any rate so far as the rotor is concerned, would be working under more favourable conditions than the rotor of a turbo-generator. One speaker has calculated that by using an extra high temperature on a 20 000-kW machine one could show a net gain of about £250 a year. I rather think that when the capital cost of a complete 20 000-kW set and its auxiliaries is taken into consideration a saving of £250 a year would have no influence with a buyer if it would in any way affect the reliability of the set. With reference to the author's remarks on foreign competition it should be possible to put forward higher-temperature machines for export work where it has been found from experience that foreign competitors are doing the same, supplying the lower-temperature machines to the home market, until the engineers in this country are willing to use higher-temperature machines. With the extra-high-temperature machine it might be necessary to have a larger air-filter, which would occupy more space, and with building prices as they are to-day this may offset the reduction in price of the higher-rated turbo-generator.

Mr. B. G. Churcher: Voltage was applied during the bending tests, and it was found that electrical breakdown occurred just after mechanical failure. In regard to the question of thermo-couples, it is necessary to measure the cold-junction temperature whether one employs the deflection method or by a potentiometer. With the latter method one does not, of course, balance the voltage of the thermo-couple against a cell but against a slide-wire which is calibrated with the cell. Mr. Clayton mentioned the use of the resistance thermometer. I think it will be found that the argument of robustness will not work out in practice, because with thermo-couples one can use a fairly thick wire which is sufficiently strong to stand ordinary shop usage, whereas with the resistance thermometer one finds that in order to get sufficient resistance in the arm of the bridge which forms the resistance element, and to make the element reasonably small, one has to use very fine wire. I think it will be found that the resistance thermometer will be no better than an ordinary mercury thermometer as far as robustness and accessibility are concerned. That is where the advantage of the thermo-couple comes in. On page 285 the statement is made that surface temperatures can be measured accurately by means of a thermometer. That does not seem to be quite correct. Thermometers are primarily intended for measuring the temperature of liquids or gases and are calibrated by being immersed

in a liquid up to the level of the reading. The emergent mercury column has to be corrected for when they are used under other conditions. For commercial testing the correction may be negligible, but, in measuring surface temperatures, if the thermometer be placed against the surface it will measure the temperature of the air just outside the surface plus a certain amount of radiation from the surface. The author points out that pads introduce abnormal conditions because the cooling conditions are altered where the temperature is being measured. I have measured the temperature-rise of a commutator by the two methods, that is to say by putting a thermo-couple and a thermometer on to the commutator at the same time. The reading by thermometer was 56 and by the thermo-couple 71, thus indicating that the thermometer method was in error by 21 per cent. It is simply due to the fact that the thermometer reads the temperature of the pad, not the temperature of the commutator, and the temperature of the pad was nothing like that of the commutator owing to the poor thermal conductivity of any material that can be used for a pad. I think that in surface temperature measurements where any sort of pretension is made to accuracy, one cannot use a thermometer, especially when there is an air blast. In the latter case, I do not think such a method could be considered for a moment. The only way to get surface temperatures accurately, especially where there is an air blast, is to use thermo-couples and sweat them on to the surface. This will give perfectly satisfactory results.

Mr. T. Baxendale: Last year we opened up a generator rotor, insulated with press-spahn, which had been in use for 7 years. The insulation was practically as good as when first put in and, so far as can be seen, will last at least another 10 years. This set runs with an average temperature of 120° F. on the rotor, so that there is something to be said for low temperatures on rotors. I should like to ask the author if he has considered what effect wet-air filters will have on machines with high temperatures. With air filtered through cloth the insulation resistances will, in general, be high, but with wet filters the insulation resistance drops very quickly. Dr. Barclay mentioned the expansion of the copper. Although the conductors may be held by the mica there is always the possibility that the insulation may give at the joints between the end connections and slot windings, due to expansion, especially if insulated with ordinary Empire cloth (which so far has been the general custom). Insulation made of fibrous material will quickly carbonize at high temperatures. In drying out a machine we find that if the temperature rises much above 180° F. the varnish comes up in small bubbles, which raises a point as to what quality of varnish should be used for the temperatures suggested by the author. With regard to rotors running at high temperatures, we have one, mica insulated, running continuously at nearly 100° C., which up to the present has not given any trouble due to deterioration of insulation. I would suggest that makers extend the mica insulation of the slot winding a little further from the core, as any dirt collecting at this point is apt to earth the winding, especially if any moisture comes over from the wet-air filters. The author mentions the hot spots in a machine. The thermo-couples we had on a large machine showed a temperature of 117° C. on the stator winding. The makers of the generator expressed some doubt as to the accuracy of the temperature, but after reading the paper I am inclined to think that the temperature shown was about correct. On this machine the average temperature of the winding by the resistance method was 55° C. During the manufacture of one of the smaller units, rather more than 10 years ago, it was arranged with the makers that resistance test-coils should be fixed in the stator slot windings which are mica insulated. These on test showed that the temperature varied from 69° C. on the outer ends of the slot windings to 81° C. in the centre. The set is still in constant use.

[**Mr. A. B. Field** and **Professor Miles Walker** also took part in the discussion. The substance of their remarks will be found on pages 296 and 298 respectively, in connection with the discussion before the Institution.]

Mr. G. A. Juhlin (*in reply*): Dr. Barclay seems to have misunderstood the limits advocated in the paper. The limit suggested is 160° C. final temperature and not 75 degrees C. temperature-rise as mentioned by him. Dr. Barclay expresses the opinion that 160° C. temperature limit is too high for the rotor because of the difference between the temperature as measured by resistance and the hot-spot temperatures, particularly in a badly designed machine. It hardly seems logical to settle the limits on the basis of a badly designed machine. Surely the correct way would be to weed out such bad designs and not to penalize the industry on account of these. I do not think one can agree with Dr. Barclay that an unduly high percentage of failures due to unknown causes occur to turbo-alternators, as our experience is that the percentage of breakdowns has shown a general decrease.

Mr. Kuyser is perfectly correct in his statements regarding the actual internal temperatures of machines rated at 40 degrees C. temperature-rise by thermometer, as compared with the B.E.S.A. limits, but if we consider the observable temperatures in each case, those of the B.E.S.A.-rated machine will of course be higher than the 40 degrees C. by thermometer. I agree with Mr. Kuyser's analysis of the machines designed for different temperature ratings, as well as his statements in connection with efficiency and performance generally, of the machine designed for a higher temperature limit.

Mr. Clayton's question as to why the thermo-couple has been advocated in preference to the resistance thermometer has been answered by Mr. Churcher's contribution to the discussion, with which I agree. I think Mr. Clayton considerably over-estimates the difference between the temperature as measured by resistance, and the hot-spot temperatures in the rotor, as there should not be anything like this figure in a well-ventilated machine.

In reply to Mr. Sills, I would say that nothing but mica insulation is used between turns, as well as to earth. The question of the influence of temperature

on the insulation between laminations has been dealt with in reply to other speakers. With regard to the question of tests carried out on water-wheel generator coils, these coils were only chosen because they were available for tests, and the question of the insulation used between turns has no bearing on the general results of the mica insulation. It is of course quite possible to supply high-temperature machines for export work where such temperatures are acceptable, but it entails considerably more work for the manufacturer to carry several different lines of designs, and does not tend to efficient production. In reference to the question of air filters, I should say that these would be smaller, rather than larger, for the higher-temperature-limit machine.

Mr. Churcher's remarks are of interest and show clearly the varying results which may be obtained by thermometer measurements, and there is no doubt that considerable care has to be exercised to obtain correct readings by means of thermometers.

Mr. Baxendale desires to know what effect wet-air filters will have on high-temperature machines. I am of the opinion that there will be very little difference in this respect with varying temperature, but any difference would be in favour of the higher-temperature machines. The information given regarding machines running continuously at nearly 100° C. is of considerable interest, and one would venture to predict that no trouble need be anticipated at this temperature. I do not think there would be much doubt that the temperature of 117° C. obtained by means of thermo-couples on the machine referred to by Mr. Baxendale. is correct, as this temperature was obtained on the bare copper at considerably above normal load, and could therefore be considered to be a very satisfactory result.

South Midland Centre, at Birmingham, 2 February, 1921.

Dr. G. Kapp: The author in his opening remarks disclaims any intention of suggesting a general rating up of existing sizes of alternators on account of temperature-rise, but the logic of his experimental evidence points that way. To my mind the most important matter is to obtain, not cheaper machines, but safer machines, and that involves an exact knowledge of the heating conditions. The method of temperature test by thermometer is stated in the paper to be the easiest and simplest, but I cannot agree with this statement. It appears to be easy, but in reality it is not so, and is very misleading. During the run a thermometer cannot even give the real surface temperature, as it is blown upon by the ventilating air, and it is a common experience to find that, after the machine has been stopped, the thermometers show a sensibly higher temperature, at any rate in machines which can be stopped quickly. A true hot-spot temperature cannot be recorded by thermometer, for the simple reason that the hot spot is inside the material and is therefore not accessible to the thermometer. We have to guess it more or less from the difference between the readings of the thermometers placed on the surface of the body near the point where the hot spot is presumed to be, and those of the outside thermometers. The latter are exposed to local draughts and show great variations, so that a temperature chart shows an uneven line and to use this curve for the determination of the time-constant becomes mere guess-work. What really matters is not the temperature-rise, but the actual temperature at the hot spot, and to get this we must have a temperature detector which can be placed actually at the hot spot or at a point where the temperature is actually the same as at the hot spot. As it is obviously impracticable to place the detector in metallic contact with the bar in the slot, we must place it so as to insulate it from the bar. The conception of a hot spot implies that no heat must flow past it, for if there were a flow of heat there would necessarily be a hotter spot somewhere else, in our case in the bar itself. The author has recognized this essential condition and illustrated it in Fig. 5. The ideal condition is attained by placing the detector at the point C, for this point is in what may be termed thermic equilibrium. Point D is not quite as good, but still acceptable, but point B is quite useless as it involves an evaluation of the temperature-drop between points A and B. Detectors based on the principle of increase of resistance with temperature are rather bulky in comparison with thermo-couples and, as such a couple can be accommodated at the ideal point C, it would seem that the thermo-couple is the right instrument to use. But here we come to a difficulty; if we attempt to make it direct-reading we introduce an error due to what is known as the "Peltier effect." A potentiometric method is therefore more exact, but this requires some manipulation and an exact knowledge of the thermoelectric coefficient. Copper-eureka with about 44 microvolts per degree C. is a very suitable combination, but this figure is not a rigid constant, so that a coefficient would have to be supplied with each machine according to the particular couple used. It is also necessary to know the temperature of the cold junction, and therefore at least one thermometric observation is required in addition to a calculation. There is a simple method to avoid the calculation and obtain the exact hot-spot temperature direct. All we need is to use for the outside circuit the same metals as are embedded in the hot spot and connect the two circuits in opposition with a galvanometer in circuit. The so-called cold junction may then be immersed in liquid which is heated by a spiral, and whose temperature is observed. If the temperature of the liquid under the influence of the heating spiral has attained that of the hot spot, the galvanometer will read zero and the thermometer will indicate the hot-spot temperature. One beaker, heating spiral and thermometer can, by an arrangement of switches, be used for the determination of any number of hot-spot temperatures. Can the author tell us if the readings of thermometers are influenced by eddy currents from stray fields being produced in the mercury? I have not observed such an effect, but my experience is

limited; on theoretical grounds, however, such an effect should exist.

Major A. M. Taylor: I am very interested in the results of the tests given on pages 289, 290 and 291, but I feel that it is necessary for engineers to go very carefully in this matter of temperature. One thing is quite certain—that if there is any tendency for the alternators to break down under present conditions, due to temperature, that tendency would be increased by raising the temperature—it certainly could not be altered. The tests appear at first sight to give ground for the proposed increase in temperature, but it must be remembered that the author was able to examine in detail the condition of the insulation when opened up after the end of the test, and no doubt he is far better aware than the users of the machine can possibly be of all the little points and weaknesses which might develop into trouble, and which can hardly be conveyed to the engineering public through the medium of a paper. The most conclusive way of backing up his opinions would be for his firm, and other firms, to give guarantees for a number of years with alternators constructed to work at certain temperatures measured by temperature detectors. The second point to which I should like to refer is the question of the detector method versus the resistance method. The latter has, it appears to me, been condemned off-hand because it only measures the average temperature of the complete circuit. This no doubt is a very serious disadvantage, but on the other hand, from the calculations given on page 284, in connection with Fig. 5, it would appear that between points A and B there is a difference of temperature of 28 degrees C. with insulation for 6 600 volts, and of 48 degrees C. with insulation for 10 000 volts. This difference of temperature is only a matter of calculation and is a factor as to which there may be considerable uncertainty, the temperature detector only being of service as far as the point B. Looking into the question of the error introduced by measuring the average increase of resistance over the whole circuit as compared with the desired "hot-spot" increase of resistance I have arrived at the conclusion that, if one knew what proportion of the length of the circuit the hot spot covered, and if one could measure the increase of resistance over the whole circuit due to the combined action of the normal part and the hot-spot, and also could measure independently the rise of resistance of the complete circuit when the hot spot was not active (as, for example, when the stator was cool), one could from these two measurements obtain the actual temperature of the hot spot. It may be argued against this that it would not be sufficient merely to take the length of the core along the axis and find the proportion of the copper bars lying in that to the total length including the end connections; but if the core could be assumed to be at a uniform temperature along this length this course is feasible. If, on the other hand, the core is not at a uniform temperature along its length, it would be practicable to introduce indicators in the form of a tinned sheet-iron lamina inserted every 6 inches or so, which would give a clue to any one part of the core being at a higher temperature than any other (working along the axial length). I believe it to be possible, by taking a cooling curve with the machine kept running, but without any excitation on the field (and if necessary with a small counter excitation, so as to prevent any electromotive force being generated in the stator winding), to obtain a curve which at its highest points will tend to read high, due to its including the rise of resistance of the hot-spot part, whereas at its lowest point it would tend to be nearly a correct measure of the rate of cooling when there was no hot spot, due to conduction along the windings. If, when the machine has reached its lowest temperature, a comparatively short run were to be taken on a rising temperature curve—preferably obtained on full load for a very short period, but, alternatively, by passing a direct current through the stator winding, sufficient to give the same I^2R losses as the full-load alternating current—and the heating curve were to be deduced under these conditions (initial and final temperatures), and if, finally, we were, in the case of the alternative test, to run the generator on short-circuit and with the excitation arranged to give only just the necessary voltage to obtain full-load current, measuring the initial and final temperatures also under this condition (the latter test being carried out merely to correct the direct-current test) a correction could then be made on to the direct-current curve to allow for alternating current, and the starting curve obtained. Having thus obtained the initial portion of the heating curve by one of the two methods indicated, this may now be inverted and taken as the initial portion of the cooling curve, and we thus get the true cooling curve when there is no hot spot. Having thus obtained the difference between the part of the cooling curve for that part of the machine which does not include the hot spot, and for the included hot spot, it is possible, knowing the proportion of the length of the hot spot to the total length of circuit as stated above, to get at the true hot-spot temperature in the way I have indicated above and to plot a third cooling curve, which should at its peak give the highest temperature reached by the copper itself in the hot-spot portion of the circuit. The whole of the above depends, as already stated, upon being able to estimate approximately the length of the hot-spot portion of the circuit, and from the curve given in Fig. 2 it appears to be a not very difficult matter to make a fair estimate, where radial ventilation is employed, of the length of this portion of the circuit, which could be checked by the tinned laminæ, if these had been inserted during construction.

Mr. A. T. Bartlett: The author has set forth very clearly and fully the case for the adoption of temperatures higher than those already agreed upon by the British Engineering Standards Association for turbo-alternators. It is most difficult in experiments of this class to imitate even approximately the working conditions which obtain in practice. For instance, in the experiments on the strength of mica tubes shown diagrammatically in Fig. 7, the tests were made on a composite cantilever consisting of a mica tube with a more or less solid copper centre. It seems to me that it is almost impossible from the results obtained to draw any useful conclusion, as the strength of the copper will be comparable with, if not greater than,

the strength of the mica tube. I suggest that had a finely stranded and therefore weaker bar been used the experiment would have given much more satisfactory results. Then, again, we cannot draw definite conclusions from experiments even when they are prolonged for 7 000 hours. Experiments which I have carried out show that, even with the B.E.S.A. specification for materials, class A and class B are already on the high side. In my opinion, therefore, we should fix a maximum temperature of, say, 110° C., and if this proves satisfactory we might adopt a higher temperature in the future. As regards the use of temperature detectors, in my opinion this is the only correct method of running plant. For instance, in the case of a power house, if one is starting up a set cold and the steam end is capable of doing considerable overload, can there be any reason why this machine should not be run at a heavy overload for a short time, readings being taken on the temperature detector so that the safe temperature is not exceeded? Reference has been made to methods of ascertaining temperatures which need balancing in order to get the temperature readings. In my opinion this is wrong; all such detectors should be of the inspectional type, as the average power-house attendant has already enough to do without work of this kind. With the inspectional instrument there can be no excuse for any neglect of definite instructions as to the temperatures at which to run various machines.

Dr. M. Kahn: Many engineers still specify for large turbo-alternators a temperature-rise of 40 degrees C. measured by thermometer, a specification which goes back to the early days of electrical engineering. High-voltage machines were then practically unknown and a 100-kW set was considered a large unit. On small low-tension machines the temperature is nearly uniform all over the armature after a 6 hours' full-load run, and the thermometer measurements give a fairly accurate idea of the temperature-rise. The large turbo-alternators which we build to-day differ from those early machines in size, construction, insulation, in method of ventilation, in short in every respect, and it is obvious that the experience on which the rating and testing of these small units was based does not now apply. The author has given a very comprehensive treatise on the main points on which the rating and testing of the modern large units should be based. Temperature measurements by thermometer on these machines are deceptive, and the only possible and correct way to ascertain the temperature-rise is by measurements by temperature detectors. For these a temperature-rise must be allowed which has been found safe for the mica insulation which is now universally applied on large turbo-alternators. The temperature limit stipulated in the B.E.S.A. Report No. 72 is 120° C. for large high-tension machines, and this is certainly on the safe side. A machine insulated with mica and asbestos only and working with a temperature-rise of 80 degrees C. corresponding to this 120° C. limit is safer than a motor insulated with cotton insulation and a temperature-rise of 40 degrees C. For cotton insulation this temperature-rise is nearer the safety limit than that of 80 degrees C. is for mica. The *sine qua non* for this is, of course, that cotton is not used except on the surface of the end winding which is directly exposed to the incoming cooling air, where, in consequence, high temperatures cannot possibly occur. On page 291 the author mentions coils with cotton insulation between turns. The insulation between turns causes trouble more often than the insulation to earth, and only mica insulation should be employed between turns and to earth throughout the coil. I favour a gradual development rather than an immediate increase of the temperature limits. All firms building these large units, and many operating engineers, are alive to the question, and data and experience are continuously accumulated. The question is not an urgent one, as it is quite possible to produce the sizes at present required within the limits of a total working temperature of 120° C. Manufacturers and the operating engineers who have to rely on these units for a large percentage of their load have no interest to run any risk. If we consider what the breakdown of a 20 000- or 30 000-kW set means in a power station and for the manufacturer, this is self evident. The accumulation of experience will naturally tend to lead in course of time to development up to a safe limit. A point which may conceivably determine the temperatures which can be allowed has not been mentioned by the author. I refer to the question of heat expansion of long coils on large turbo-alternators. These coils may be 10 ft. and more in length. With a temperature-rise of 160 degrees C. the expansion of the copper conductors is over $\frac{1}{4}$ inch and, as the mica tube will not expand anything like as much, the copper conductors in the tubes may get loose in course of time and the tubes may be damaged by wear due to vibration of the loose conductors. There are two points in the paper with which I do not agree. One is Figs. 2 and 3; I believe the author has not been quite fair to machines with axial ventilation. The temperature-rise of the iron depends on the temperature at which the cooling air leaves the machine, the specific loss per unit of cooling surface, and the air velocity. These three factors are practically the same for machines with axial and radial ventilation, except that the temperature-rise of the cooling air, given equal amounts for both types, may be rather less for machines with axial ventilation, and this is rather a point in favour of this type. The maximum temperature of both types will therefore be more or less the same, the balance being if anything in favour of the machine with axial ventilation. The only difference is that the maximum temperatures on machines with axial ventilation occur at one end, and on machines with radial ventilation in the centre. The other point is the author's rather optimistic views as to rotor windings. The temperature-rise on the rotor is often quite as high as in the stator, and the enormous stresses to which the rotor insulation of large turbo-alternators is subjected, together with the hammering effect of vibration in case of out-of-balance, and the effect of short-circuits, make the problem of rotor insulation of first importance. The mica insulation between turns and to earth has, however, proved very efficient, and asbestos bakelite is also satisfactory if used for distance pieces.

Mr. R. Orsettich: I entirely agree with the author in his plea for the adoption of temperature detectors

in all large turbo-generators or any other large machines of the totally-enclosed type, because only in this manner shall we be able to obtain an approximate idea of the real temperatures existing in the coils of such machines under full-load conditions. I am so convinced of this that I arranged some time ago that all machines from 5 000 kW upwards made by my company should be fitted with temperature detectors. Dealing with the higher limit of temperature proposed in the paper, and the suggestion that such higher limits should be adopted because they are already in use in America and also because they will enable us to produce larger and cheaper machines, I should like to say that opinion in America is very much divided on this question and that some of the leading makers are against it. Mr. Reist stated in a recent paper before the American Institute of Electrical Engineers that in the design of a turbo-generator it is important that the limits of temperature adopted should not exceed those laid down for class A materials in the Standardization Rules of the Institute, also that the increase of cost of a machine designed for a low temperature is very small compared with that of a machine designed for a high temperature. Mr. W. J. Foster, in a discussion on another paper on turbo-generators before the American Institute of Electrical Engineers, in November 1919, stated that it had been his experience to design turbo-generators for temperatures of 105° C., and that this also was the case with practically all the 60-period generators which had been built by the General Electric Company up to that date. Mr. Torchio, the chief engineer of the New York Edison Company, assured me that in the light of recent experience he would refuse to discuss a tender embodying special guarantees for higher temperatures, as he was certain that the principle of increasing the temperature was based on a misunderstanding and not on actual experience. In a paper which he read at the June Convention of the American Institute of Electrical Engineers he gave details of 55 breakdowns of turbo-generators of sizes ranging from 5 to 30 000 kW. Of these breakdowns 33 were due to armature failures, of which 17 were caused by high temperatures. My recent visit to the United States did not confirm the generally accepted belief that generators in that country are loaded on the basis of temperature as recorded by detectors. The detectors are installed, but usually on the wall opposite the switchboard and not on the switchboard itself. They are not read by the station staff when the machine is being loaded, and most managers confirmed that this was not their practice.

Mr. W. J. Line: Although the title of the paper is "Temperature Limits of Large Alternators," only the questions of the heating of the copper and its insulation are considered. It seems desirable to consider also the heating of the iron due to the various losses occurring in it. If iron temperatures are excessive, damage to the insulation paper or varnish between the laminations may result, thus destroying its utility. Undue expansion due to heat may also cause undue axial expansion of the assembled core, resulting possibly in excessive stress and stretching of the core clamping parts, or compression of the core laminations and insulation, in either case causing slackness of core and vibration at lower temperatures and further destroying the insulation of laminations. Even if iron temperatures are moderate and lower than those of the copper, the temperature of the iron due to its own losses tends to cause higher temperature in the copper windings which are embedded in it than would otherwise be the case. The particular design of slot insulation considered in the paper is that in which composite insulation, consisting mainly of mica, with some support of less dielectric strength, is wrapped or wound round the bars, which are then pushed into the slots. Other forms which have been used in alternators are, first, micanite tubes; these are pushed into the slots and the bars then inserted; secondly, micanite or other troughs, the two outer edges of which are turned down over the conductor and secured tightly by wedges, in the case of slots partially closed at outer ends. A form of resistance construction introduced a good many years ago by a leading firm and still, I believe, in successful operation, is electrically and mechanically practically the same. It consists of troughs in a heavy cast-iron box, lined with micanite troughs, the top edges of which are turned down and suitably secured over the "brick" resistance. The latter is made up of folds of resistance material (with heat-absorbing plates and insulation between the folds) laid in the troughs in blocks or "bricks," and forced up from the ends. The heat produced in the resistance is dissipated through the mica to the iron. Temperatures up to 240° C. have been commonly used on this and have given little trouble, the only result being carbonization of the shellac or binding material in the micanite. The insulation remains satisfactory if undisturbed, but if removed is very brittle and breaks up. It is not suggested that machines should be worked at anything approaching such a high temperature, as the margin of safety on large and expensive machines, where breakdowns and interruptions are serious, must be above suspicion, whereas on resistances which are relatively small a narrower margin may be justified. But experience on these resistances tends to show that somewhat higher temperatures than have so far been commonly employed might be adopted without undue risk on machines with good mica insulation. It appears to follow from the mechanical tests described by the author that breakdowns under high voltage may be expected at the point where mechanical breakdown may occur, i.e. where the insulated conductor leaves the slot. I believe it is a fact that breakdowns occur more frequently at this point than elsewhere, but is this due to the bending of the insulated conductor where it emerges from the slot? In connection with the results published in the *Electrician* some years ago of dielectric and other tests by Professor E. Wilson and Messrs. W. H. Wilson and T. Mitchell on samples of mica, it is suggested that, as the density of the dielectric force at the ends of the slots in which high-tension coils are embedded is considerably increased, the ends of such slots should be rounded off. The suggestion seems worthy of consideration, if it is practicable in ordinary manufacture. In the section entitled "Heat Flow through Insulation" the author gives a dissipation figure of 0·7 watt per square inch. I should

like to know the numerical limits of such a figure under varying conditions. In my own experience on many kinds of apparatus I have found the following figures:—

Description	Temperature-rise		Watts per sq. in.
	deg. F.	deg. C.	
Solenoid coils in free air—small to large, black varnished ..	*75	*41·7	0·7—0·4
Solenoid coils in iron covers (some air space)	*75	*41·7	0·5—0·25
Large coils in iron covers, filled with bitumen	85	47·2	0·46
" Brick " resistance, as already described	216	120	4·0
" Brick " resistance, as already described	432	240	8·5

* These are on surface. Increase of resistance 20 per cent, corresponding to 85 degrees F. average internal rise.

considered, since the dissipation of heat is the chief factor. The table showing the breakdown voltage of various classes of mica wraps after heating for 7 000 hours is very interesting. I notice that the temperature during this test was maintained at 190° C. It would be interesting to know if any tests were carried out to ascertain the mechanical strength of the material, in addition to the dielectric strength, after the temperature test was completed. I quite agree with the author that it is hardly possible to come to any definite decision with regard to the maximum temperature to which mica, as an insulator, may be subjected. It would appear that the temperature limit would depend upon the binding agent, in this case shellac. There is no doubt room for considerable research in this direction, since machines with a high temperature rating are cheaper to produce from a manufacturing point of view. The latter is very necessary if we are to prevent further orders for machines going abroad.

Mr. C. Sylvester (*communicated*): I am interested in the author's remarks concerning temperature measurements, and I agree that the use of felt pads for determining the stator temperature is likely to result in false readings. As a general rule these pads, which hold the thermometers, are tied to the ends of the stator winding and after the test they are removed and the readings taken as being the maximum temperature-rise of the machine or windings. I cannot see how this can be, because I have seen these pads loosely tied to the ends of the coils in such a manner that in all probability, when the machine is running, they are not actually touching the coils at all. In this case the temperature indicated on the thermometers would be merely that of the forced draught, after allowing for a slight error due to the heat dissipated by the end shields. I consider the embedded temperature detector (the thermo-couple) is very reliable for obtaining temperatures at any given point but, as the author points out, considerable care has to be taken in locating these, or serious trouble may result. When in America last year I visited some of the large electrical engineering works, and I found that the use of the thermo-couple was greatly resorted to. I should like the author to express his opinion as to whether, in the majority of cases, the present systems of air cooling are all that is to be desired. My reason for asking this is because I consider there is room for considerable improvement in the lay-out of some plants. I know of a large power house in the Midland counties where the air filters are situated in the basement in close proximity to the small steam turbines which operate the circulating-water impellors. Moreover, the main steam-range pipes also run along the wall, in addition to a large 2-ft. pipe through which the steam passes when the air pumps fail and the turbine exhausts to atmosphere. It is hardly necessary to say that when the turbine is running on atmosphere the temperature in the basement is unbearable, and this air has to pass through the alternator for cooling purposes. This points to the fact that, in connection with the temperature limits of large alternators, the air-cooling system should be adequately

Mr. G. A. Juhlin (*in reply*): I entirely agree with Dr. Kapp in his remarks as to the difficulties in obtaining thermometer measurements where thermometers are exposed to strong air blasts, and in such cases it is of course necessary to shield the thermometers carefully. Dr. Kapp makes an interesting suggestion with regard to temperature measurements by thermo-couples, and this would seem to be perfectly feasible, although some difficulties may be experienced where a large number of couples have to be read quickly. For general power-station work, however, the suggestion is certainly worthy of attention.

Major Taylor considers that the most conclusive way for a firm to back its opinion with regard to temperature would be to give guarantees for a number of machines with alternators constructed to work on certain temperatures measured by temperature detectors. It is extremely difficult for manufacturers to give prolonged guarantees of machinery, and I think it should not be overlooked that purchasers of electrical plant in the past have had guarantees covering only, say, 12 months, but at the same time no manufacturer would shirk his responsibilities if conclusive proof could be given of a breakdown having been caused by excessive temperature; and it should not be overlooked that, in the past, the question of internal temperature-

rise has been very largely a matter for conjecture, so that the purchaser's risk would certainly be greater under such conditions than where internal temperatures were known. I am afraid considerable difficulty would be met with in carrying out the suggestion of obtaining heating and cooling curves, as large alternators cannot be tested except on commercial load, and the time taken in shutting down is so long that the temperatures would be more or less uniform by the time the machine came to rest. It is, of course, a fact that the temperature of the embedded portion of the windings is not by any means constant over the whole length, but varies from point to point.

Mr. Bartlett is doubtful as to the value of the experiments on the strength of mica tubes shown diagrammatically in Fig. 7. There are, of course, difficulties in obtaining absolute measurements, but, as the same size of copper was used for the coils before and after heating, the question of the strength of copper would be eliminated, and any change in the strength of the insulated bar would be due to a change in the insulating material. It is doubtful whether a stranded conductor would give any better results. Mr. Bartlett suggests a limit of 110° C. as the maximum. I do not think, however, that engineers with experience of modern insulation would agree that this is a satisfactory limit. Experience extending over a considerable number of years has shown that such a limit is altogether too low, and it would certainly be a retrograde step to adopt it. Mr. Bartlett's suggestion would indicate unfortunate experience with insulation, and could hardly be considered typical of high-class mica insulation. I agree with the statement that detectors should be of the simplest type without any special arrangements.

With reference to Dr. Kahn's remarks that cotton insulation between turns is mentioned on page 291, it should be noted that it is stated the samples for the test referred to were taken from a standard water-wheel machine, which is not of the same class of machine as a turbo-alternator, but is more in the line of an ordinary low-speed machine. These coils were taken because they were available for the test; in addition to this, an excellent comparison between the cotton insulation and the mica insulation was obtained. Nothing but mica insulation is used for turbo-alternator work. One can hardly agree with Dr. Kahn that the need of data and experience is not urgent. I am of the opinion that no time should be lost in securing all the information that can possibly be obtained, in view of the demand for large high-speed machines. The question as to the possibility of producing units of the sizes required within a temperature limit of 120° C. may be justified, but it can only be done at greatly increased cost. Dr. Kahn seems to be of the opinion that Figs. 2 and 3 do not give a fair indication of the temperature distribution of the axially ventilated machine. As these figures are only intended to be diagrammatic, I do not think any definite conclusion in this respect could be arrived at from them, but in general I think one may say that the uni-directional axial ventilation system would certainly show higher hot-spot temperatures than the ordinary radial ventilation, other conditions being the same. With reference to the question of rotor insulation, I do not think it would be satisfactory to allow any machine to run under conditions which would give rise to a hammering action, because this would destroy any type of insulation independent of the question of temperature. Dr. Kahn must have misunderstood the reference to the rotor temperatures. I agree that the temperature of the rotor is often as high as that of the stator, and this part is, in general, able to withstand higher temperatures than the stator.

Mr. Line suggests that the heating of the iron should be considered. This question has been raised by previous speakers, and no fear need be entertained for the safety of the armature laminations when operating at the temperature limit suggested. Mr. Line referred to the normal limits of the loading watts per square inch under varying conditions. It is not possible to give information on this point in a general way, as full consideration would have to be given to the conditions of ventilation for each individual case. It is not possible to express a definite opinion on the present systems of air cooling, as they vary very greatly in different stations, and one could not therefore express a general opinion. I think, however, that the closed air circuit with air coolers will find general favour, as against the present system of using filters, since it obviates some of the greatest difficulties experienced at the present time in the way of filtering the air and keeping it free from loose moisture.

North Midland Centre, at Leeds, 8 February, 1921.

Mr. W. M. Selvey: In listening to the author, I at first had an uneasy feeling that he was breaking the news to us that, whereas for a number of years we had been taking temperatures by thermometer and thinking these temperatures were correct, we had actually been having in the hottest spot temperatures approaching those which he proposes to standardize. If that is the correct view, he is justified in bringing forward his plea for the adoption of higher temperature limits. In the early days the load grew more rapidly than machines could be built, and I think that is the case to-day, and will be for at any rate the next five years or so. The earlier experiences of electrical power were based on a load in which diversity, as we know it to-day, had not proved its full benefit in levelling up the load. There were times of very great fluctuation, and one of the conditions in the early specifications for turbines was a type of governing which should operate with very wide fluctuations without any appreciable change of speed and maintain a constant voltage. That resulted in turbines being erected with very sensitive forms of governors. The only way to meet the specification was that the overload, or the load which could be given by the turbine, should be very large compared with its normal rating. The reason for that may easily be seen; if a governor has to open

very suddenly, it must open widely. The consequence was that in our early turbines, particularly those for which Messrs. Parsons were responsible, we could always give 50 or perhaps 100 per cent overload according to the conditions at the steam end: the generators were wound, as we thought then, in very generous and ample proportions. Judging by thermometer tests the rotors and stators were remarkably cool. Therefore a machine which was put in to work from 3 000 to 3 500 kW almost immediately got rated up, because the load was growing so fast. The consequence was that those machines had no sooner been put on load than their everyday load was not 3 500 but 5 000 kW. The question of heat dissipation operates in a reverse direction on economy of material. It is economy of material to adopt a higher speed if the heat losses are the same. Therefore one has to get rid of a great deal more heat from the same mass of material, or conversely the same amount of heat from a smaller amount of material. This is done by forcing air through the machine at a high pressure. The earlier machines took perhaps $\frac{1}{8}$ inch pressure to force the air through them. A modern machine takes up to 2 inches, and I have even heard of a projected design of a larger machine which might require 6 inches' pressure. There are many reasons why we should adopt these large machines at 3 000 r.p.m. if we can trust the insulation. The author mentions the possibility of an entirely inorganic mineral form of insulation. He has not so far entirely eliminated the organic part, because I think he still proposes to use some organic binding material, but at any rate it is getting very much less. The drawback to this is that a good electrical insulator is also a good heat insulator. Therefore in adopting the mineral or silicate form of insulation we adopt a type whch tends to keep up the temperature of the metal which it encloses. We have to reconstruct our point of view, therefore, and we must now endeavour to make the temperature of the hottest spot the standard, because that is all that really matters. The way to do this is to employ an embedded temperature detector at the hot spot. This idea is not new, but I doubt if it is possible to make this measurement accurately without interfering in some way with the general solidity of the generator as a whole. The question of a standard voltage is going to be serious if we are to have, as we have been led to believe, stations at the seaside with 30- or 40-mile transmissions. We should allow the designer to select any voltage he likes, because in any case it would be transformed. It would be not less than 33 000 and probably 66 000. But to-day we are facing the problem of 11 000- versus 6 000-volt machines. Pressures of 10 000 and 11 000 volts have been employed without any transformer, and have proved very satisfactory. The author says that in this case we must run the machines at still higher temperatures, because with pure mica insulation there will be a larger temperature gradient. Also, the more insulation we use, the less will be the amount of available copper, and therefore the copper losses will be higher. If that be so, there may be a difference of the order of 5 to 10 per cent in the output per pound of 6 000- and 11 000-volt machines.

Mr. F. Creedy: I am very interested in the author's application of Professor Miles Walker's method of calculating temperature-rises. I have tried from time to time to make use of this method but, no doubt due to the causes he mentions, i.e. the variations in the tightness of packing of insulation and other factors, I have never been able to get results closely in accordance with test. I am chiefly referring to low-speed machines. This method seems to me to give the hot-spot temperatures not only in turbo-alternators but in machines of all kinds. I should be very glad if the author would say to what extent that is practically possible and how closely calculation agrees with test results. Referring to the stresses on the rotor of a turbo-generator in the case of short-circuit, I should have thought they were equal, as action and reaction are equal and opposite. The author does not explain the precise methods of bracing adopted, but no doubt they are the same as in the case of the stator. I should like to know whether there is any method of limiting these stresses by the use of choking coils or similar apparatus to cut down the short-circuit current. Instead of providing for such very heavy stresses one would think that it would be desirable to diminish the stresses if possible.

Mr. R. D. Spurr: The best of the author's designs can be wrecked by careless handling or assembling under bad conditions. I have in mind large stators which are assembled on site, or else are handled on the railway. The handling of a 15 000-kW stator weighing 45 tons entails heavy work. Although armature winders are very careful, they are sometimes expected to make a good job under very adverse conditions. I believe a large machine wound recently under very bad conditions has since broken down. Another probable cause of failure is the air filter, of which there are two types, wet and dry. I do not think we have yet arrived at the final development of wet filters. I believe the practice of blast-furnace engineers in the United States is to refrigerate the air after it passes the wet filter, in order to dry it. If we passed the air from a wet-air filter over refrigerator pipes there would be no necessity to run machines at a higher load in the winter than in the summer.

Mr. J. D. Bailie: Nearly 25 years ago we used asbestos insulation on direct-current generators of 150-kW rating at 500 volts, and these machines are still in use. I am not quite clear whether the high temperatures referred to in the author's tests were maintained continuously over the long period of time named. If they were, I think it would have been of great interest if more or less similar experiments had been carried out with the current applied for 12 hours and cut off for 12 hours alternately over the whole period of the test, the object being to ascertain the effect of the resulting alternate expansion and contraction of the conductors. Referring to the method of anchoring the end windings of large machines by wide metal rings, I think the object was to provide a rigid support. Would not the tendency of these rings, however, when the windings are subjected to the high temperatures contemplated, be to prevent their longitudinal expansion and so stress the windings against the rings? A point in favour of the higher temperatures proposed, which does not appear to have been referred to so far,

unless indirectly by the author, is that they would enable a reduction to be effected in the diameter of the rotor. This, in turn, would be favourable to the adoption of a higher speed of rotation by reducing the peripheral speed of the rotor. I believe there has been a tendency of late to run rotors at higher peripheral speeds than desirable, i.e. up to something like 450 ft. per second. The firm with which I am connected was perhaps the first to criticize wet-air filters adversely. We carried out many experiments on them, and found that they passed moisture. We now make a practice of testing the air for moisture by a method which seems to be fairly satisfactory. I am in favour of the closed-circuit air coolers which are, I believe, now being adopted extensively. I should like the author to tell us which type he prefers. Mr. Selvey referred to the advantages of 11 000 volts over 6 000 volts as the generating voltage. There are many machines now generating at 11 000 volts—for example, in the stations of the Sheffield Corporation and the Yorkshire Electric Power Company—and I think they have given very little trouble. Some 15 years ago we installed two 1 500-kW turbo-alternators generating at 11 000 volts, and they are still running satisfactorily. Where the requirements are in favour of 11 000 volts—for example, when the supply is for what may be termed local use, rather than for long-distance transmission when a step-up transformer would be used—there seems to be no very sound objection to it as the generating voltage. I am entirely in agreement with the author in his advocacy of the ultimate temperature being specified rather than the rise in temperature.

Mr. G. A. Juhlin (*in reply*): With reference to Mr. Selvey's remarks, I do not think there is any doubt that machines have actually been operating in the past with hot-spot temperatures approaching those suggested in the paper. In connection with the question of providing an entirely inorganic mineral form of insulation, while it was found possible to produce mica wraps without the use of any paper, and with shellac only as bonding material, it should be pointed out that such insulation is not at the present time commercial, as there are many difficulties connected with its production, and the cost would be prohibitive. The value of the tests, in this connection, lies in the fact that they show the importance of obtaining a high percentage of mica in the wrap. I should like to correct what appears to be a misunderstanding regarding high-voltage machines. The suggestion is not to run high-voltage machines at temperatures still higher than is the case at lower voltages. In discussing the increased temperature-drop, I wished to emphasize the fact that the surface temperatures did not indicate hot-spot temperatures unless all factors were taken into consideration. There is no difficulty in producing 11 000-volt machines.

In reply to Mr. Creedy, I would say that it is exceedingly difficult to obtain test results sufficiently accurate to make a comparison with calculations owing to the many variables which are encountered; and, in general, the calculation of internal hot spots can only be regarded as approximate.

With regard to the question of wet-air filters, raised by Mr. Spurr and Mr. Baillie, a satisfactory solution of this difficulty has been found in a closed air circuit which obviates all possibility of loose moisture in the machine.

The other points raised by Mr. Baillie have been dealt with already in reply to other speakers.

North-Eastern Centre, at Newcastle, 14 February, 1921.

Mr. E. Fawssett: The paper contains some valuable experimental results, and I find myself in almost entire agreement with the author's conclusions. He says that the sources of heat are well known, and enumerates them. But with the increasing duty expected from materials in general at the present day there is an increasing risk that some small unexpected cause of heating may be magnified into a serious trouble. One or two recent accidents of this sort, although no doubt mechanical in origin, and perhaps even "bad luck" (if one can justify such a term in such important design), developed before becoming apparent as electrical trouble causing intense local heat. If one loads a machine according to the readings of an embedded temperature detector one is too apt to assume that it really indicates the temperature of the hot spot; yet it is safe to say in such a case as above referred to no such warning was given. Hence, I think the conservative limits asked for with embedded detectors are understandable, even if not quite justified. When the author states that "temperatures of bare surfaces can be obtained with accuracy by the thermometer method" I should suggest he has omitted the word "not." He goes on to point out the difficulties so far as any part of a turbo-alternator is concerned, but even then he lets the thermometer method down too gently: there is virtue in an understated case. He mentions the application of the resistance method to turbo-alternator rotor windings, and I venture to supplement his remarks by means of a diagram (see Fig. E) showing how a Harris ohmmeter can be used to give a continuous indication of temperature while the machine is on load. This method has been applied commercially in this district. As at present installed the instrument is momentarily sensitive to changes in the field current, and it is proposed to correct this by inserting a mutual inductance in the circuit, as shown in the diagram, if thought advisable. Continuous indication of actual temperature of both rotor and stator is most valuable to the operating engineer at all times, but especially to enable him to deal with short-time overloads with the minimum of running plant. All machines should be run to a total temperature and not a temperature-rise, and with continuously indicating instruments installed this should be a much easier matter than hitherto; indeed, such instruments should be the most important instruments on the machine panel. While a machine can be operated to a maximum temperature I do not see how a guarantee can be framed on that alone, unless it is merely a guide

to safe temperature and not a criterion of performance on a given load; for that, surely, temperature-rise must be taken as the basis. I agree with the author in preferring thermo-couple methods for general practical work. The problem of the cold junction can be attacked in several ways; if the instrument is close to the machine and both couple wires are taken to it, the Darling compensator is possibly the best solution; if the instrument is remote (as is usually the case) it is not convenient to run the couple wires so far, and a "steady junction" (not necessarily "cold") is provided near the machine with an all-copper pilot to the instrument. The temperature of the earth, even at a depth of 6 feet, is subject to a considerable seasonal change, and the reference junction is best placed in a small oil bath maintained at a constant temperature by a thermostatically-controlled immersion

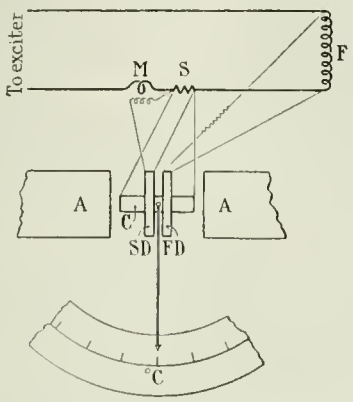

FIG. E.—Method of connecting the Harris ohmmeter to give continuous indication of temperature.

F = alternator field.
S = shunt.
M = mutual inductance.
C = ohmmeter control coil.
SD = standard deflecting coil.
FD = field deflecting coil.
A = poles of permanent magnet.

heater. We adopted a temperature of 40° C. for this as being always higher than the engine-room temperature, and being attained with a very small watt loss and wear on the thermostat contacts.

Mr. T. Carter: Of all the problems that beset the designer of electrical machinery, that of the predetermination of temperature-rises is perhaps the most difficult, and the author has thrown light on some of the obscure points in connection with this particular problem. I think it should not be too lightly assumed that "the difference between the internal and external temperature is comparatively small in the case of low-speed machines." No doubt the particular reference is to large low-speed alternators, but the statement reads rather as a general one, and it ought to be clearly pointed out that the difference between the internal and external temperature in the case, say, of the field coils of ordinary small dynamos and motors may be really very excessive. It seems to me that the reason that very few specifications are based on Report No. 72 of the B.E.S.A. is that the provisions are not understood. Particularly is that the case with regard to overloads: it is not recognized that the Report simply does not permit overloads, and yet it is common to find a demand that machines shall be in all respects in accordance with the Report and shall have in addition an overload capacity of 25 per cent for two hours, and so on. The definition at the beginning of the section of the paper entitled "Heat Flow through Insulation" is incorrect. A cubic inch is a perfectly indefinite quantity, as it might be either long and thin, or short and flat; or, as is intended, a cube the sides of which are one inch in length. It is not even sufficient to say, instead, "a one-inch cube" because it is not stated how the flow shall enter and leave the cube, and it seems to me that the only really correct way to deal with the point (and the same applies to other definitions in many places, even in standard textbooks) is to talk of the flow through a piece of material one inch long and one square inch in cross-section. The author rightly points out in more than one place how often the hands of a designer are tied by the inclusion in specifications of limiting conditions as regards, say, current densities or temperatures to be ascertained in definitely stated ways. All that should be asked is that a machine shall be supplied that will work, and will be generally in accordance with what is recognized as good practice, of which the manufacturer ought to know more than the consultant, so far, at least, as ultimate details are concerned in regard to the smaller matters about the machine. The result of the practice of putting all these clauses in specifications is that the manufacturer's staff too often have to read page after page of verbiage in order to discover if there are, after all, somewhere in the specification perhaps one or two words that matter. I should like further information in regard to one point mentioned in the paper, i.e. the limit of temperature-rise for fibrous insulation, which is 55 degrees C. measured by resistance, or 50 degrees C. measured by thermometer. I have never regarded this as a sufficient difference. A rise of 50 degrees C. measured by thermometer nearly always represents something much more than 55 degrees C. by resistance, and the two methods, which are intended to be equivalent, seem to me to be really very widely different in what they allow. If the rise by resistance corresponding to 50 degrees C. by thermometer is not too high, then the figure of 55 degrees C. is too low; if the figure of 55 degrees C. by resistance is correct, then that of 50 degrees C. by thermometer is too high. In the section headed "Conclusions" the author instances the cheapening and the increase in efficiency and the saving in space, and so on, that will be secured if his suggestions are adopted. It would be interesting and useful to have some quantitative statement on this point, and I hope that he will give some figures in his reply. The whole matter seems to be one, first of all of finding out the facts of the case, as the author has tried to do, and then of educating the consultants and general buyers of machinery up to the point of understanding that it will be to their advantage to buy on the terms laid down in the paper. Low temperatures are called for at present in order to provide a margin of safety, because people are not sure of how far it is safe to go and, unless the real meaning of the indications of temperature-rise by present methods is known, safety will naturally always be aimed at.

Even then, as the author points out, the results may not be satisfactory. Some engineers are already willing to take advantage of the possibilities of the method given in the paper, but others have yet to realize what it is all about, and if the author can convert some of these to his views, he will have done a very useful piece of work. The state of ignorance of some of the people who are in a position to place orders for large quantities of machinery is appalling. The principle of the paper is sound, but we must be very sure that the method of measurement of temperature-rises is equally sound, because if we are to work closer to the limits of safety it is more and more essential to know exactly what is going on in those parts of the machine that we cannot see. I think it is also clear that the usefulness of the general principle depends on whether the general design of the machine is good, so that there shall be equal use made of all the material, and not just a few hot spots carefully measured while the rest of the machine is too cool. It is, I imagine, no part of the function of a designer to give away information, and the paper necessarily gives only an outline of a very wide subject, but it is none the less useful on that account.

Mr. J. N. Waite: The subject of this paper is one that has required publicity for some years. From Mr. Fawssett's remarks it will be patent that the subject has been under consideration by the engineers of the North-East Coast group of power companies for a long period, and the conclusions arrived at are, in principle, identical with those of the author. The data given will be of material assistance in formulating schemes to enable the operating staff to run their generating plant at its full capacity. It is obvious that the capacity of air-cooled generators will vary with the air temperatures, and the fallacy of settling capacity on a fixed load basis is so apparent that it seems scarcely necessary to point it out. As stated by the author, the practice up to the present has been to specify a definite temperature-rise for a given kVA output, the temperature-rise to be measured by thermometer. It must be remembered that an interval of at least 30 minutes must elapse after taking off the load and taking temperature measurements by thermometer. Also, as pointed out, these measurements are surface insulation measurements and their relation to the actual copper temperature will depend on the temperature gradient across the insulation. In addition, the presumably highest copper temperature—that in the slots—cannot be measured, while the rotor copper temperature cannot be taken at any point owing to the construction of the modern rotor. The following figures, taken from comparatively recent tests, supply a commentary on the uselessness of figures obtained by thermometers. Stator temperature by thermo-couple, machine running, 84° C.; reading with load off and fan off, 92·7° C. (This shows the serious effect of incomplete shielding from the air blast.) Temperature by thermometer, machine standing, 79·5° C. (thermometer under clamp on end windings). Average temperature by resistance, machine standing, 73·5° C. Rotor (water-cooled, water circulating until at rest); average temperature of copper by resistance, when running, 123·7° C.; average temperature, standing, 85° C. Another rotor (air-cooled by fans on rotor): Temperature of rotor end-cap, 61° C.; average copper temperature by resistance, 126·3° C. The operating engineer requires the maximum safe temperature for his stators and rotors, and a reliable means of indicating the same. For operating work the instruments used should be as simple as possible. For this purpose I should rule out resistance thermometers, and also the zero-current method to measure thermo-couple electromotive force, as these involve the use of a cell and adjustable resistances. The essential details of thermo-couple indication are covered in the paper, so that it is quite unnecessary to comment on them, with the exception that it should be emphasized that the absolute accuracy of the temperature indicators is not so important as reliability and relative accuracy. The possible saving to be effected by the operation of alternators right up to their maximum safe capacity is quite considerable, whether viewed from the point of view of capital expenditure or from that of operating cost. There are three representative cases:—(1) *Multiple station undertaking.* Load allocation: Assume that one station (A) has a coal figure 10 per cent lower than the next best station (B). Assume also that a 10 per cent increase in load over the rated full load of the turbine increases the consumption by 1·0 per cent. Then, neglecting distribution costs, any extra load that can be carried on (A) will result in a net gain of 7·5 per cent on the units so generated. (2) *Single station.* The wattless current increases so that an extra generator is put on load. Assume 4 generators on load in the first instance, and that the no-load steam consumption of a turbo-alternator is 10 per cent of the full-load consumption. Then, if the extra generator is put on with no increase in the load, the steam consumption for the output will increase by 2·5 per cent. (3) *Single station.* The load rises. Making the same assumptions as in the two preceding cases, it will pay to run on overload capacity up to the point where the increase in steam consumption of the machines on load balances the no-load steam consumption and the auxiliary power for another machine. These three cases, or a combination of them, cover the field from an operating point of view, and illustrate the probability that, given the extra necessary generator capacity through scientific rating, considerable economies would often be possible.

Mr. C. H. Davidson: There is no doubt, bearing in mind the temperatures being successfully worked to abroad, that British makers must follow suit unless they are indifferent to the loss of foreign markets. Experience has shown that the present B.E.S.A. limits are on the safe side, and the tendency for some time past has been for manufacturers to quote on machines having maximum temperatures considerably higher than those laid down in the Rules. I am not quite clear whether the author wishes the increased temperatures he advocates to apply to machines built to the present B.E.S.A. standards, or to others which would show a gain in the utilization of material. Does he mean that the increased temperatures are only to apply to thermo-couple measurements, whilst normal

B.E.S.A. temperatures would be obtained by the methods laid down in the Rules? If so, it is difficult to see where any gain comes in, and he is apparently asking for the Rules to be altered to cover conditions which thermo-couples have exposed as existing in hot spots in machines which otherwise conform to the Rules if the temperatures are measured as laid down. It is rather disquieting to think that we have still to deal with hot spots, as designers have been trying for years to eliminate them. In some cases they have been very successful, although no machine can ever have an even temperature all through, radially and longitudinally. It is certain that, with the use of increased temperatures, more attention will have to be paid to the various materials used in building up mica to form tubes, etc., as it is these materials and not the mica itself which determine the limit to which machines can be run. If the temperature limits for alternators are to be raised, progress should be along very conservative lines, and full advantage should be taken of the experience gained at home and abroad, and new limits should not be fixed until more experience has been obtained.

Mr. R. M. Longman: The author is quite right in condemning the method of determining the temperature of a machine by applying a thermometer to the hottest accessible spot, as the larger the machine the smaller the chance of getting at any hot spot; in fact, one would almost think that the makers particularly rendered inaccessible any spot which might possibly be warmer than the rest. It is the actual temperature of the copper which is the danger point of a machine, and no temperature can be permitted which allows the insulation next to the copper to be charred. The slight movement of the copper due to expansion, and the far more important movement caused by a short-circuit on the system, rapidly create a pounding action, which will soon cause a breakdown on the machine. On page 284 the temperature-drop between the copper and the iron of a 6 000-volt machine is given as 28 degrees C., and for a 10 000-volt machine as 48 degrees C. This, at first sight, seems to be a point in favour of the 6 000-volt machine, particularly as the extra difference allowed by the B.E.S.A. is only about 9 degrees C. I should be glad if the author would give us his views as to the relative safety of the two machines. I believe the 10 000-volt machine is quite as safe and is a better proposition than even the 6 000-volt machine. With regard to the use of thermo-couples in conjunction with a multi-contact switch, it is of the highest importance that these contacts should be kept absolutely clean, and that the switch should always make thoroughly satisfactory contact, otherwise very incorrect readings may be obtained which, it should be noted, would always be on the low side. The test under which the bars were heated for six weeks continuously would have been made much more severe had the heating not been continuous and if cooling had been allowed to take place at times. I should like to ask the author whether the heating and bending tests on page 289 were carried out on the bare copper, with no insulation whatever. Another point which should be noticed is that if a specification allows of a maximum temperature of 150° C. it does not allow of the overloads being put on to the turbo-alternators, to which station engineers have in the past been accustomed. After rating a machine at, say, 10 000 kW for a maximum temperature not to exceed 150° C. one must not expect to be able to add another 25 per cent overload with safety. The question of rotor insulation is only touched on in the paper. This is an extremely important point and must only be carried out with the best materials. Many of the failures during the last 10 years have been due to insufficient attention to the insulation, and in some cases also to the use of soldered joints.

Mr. G. A. Juhlin (*in reply*): Mr. Fawssett's remarks are of considerable interest, and especially the diagram of the Harris ohmmeter, which apparently is used in connection with turbo-alternator temperature measurements. I am pleased to note that Mr. Fawssett's experience, in general, agrees with the suggestions made in the paper.

I agree with Mr. Carter that the definition which he gives in connection with heat flow is better than the one adopted in the paper. As regards the question of temperature-rises measured by resistance and by thermometer, it is hardly possible to give any definite statement on this point, as it of course depends entirely on the cross-section of the coil under consideration. In general I think one may say that the difference is hardly sufficient; probably a figure of 10 degrees C. would be better for an average coil.

Mr. Waite gives some very interesting figures, which show clearly the discrepancies between temperature measurements by thermometer and by resistance. It is very interesting to find operating engineers agreeing that it is advantageous to operate machines on ultimate temperature rather than on temperature-rise, and the examples quoted by Mr. Waite show that economies would often be obtained by doing so.

With regard to Mr. Davidson's question, I would say that the suggestion made in the paper is that machines should be rated on 160° C. ultimate temperature obtained by means of thermo-couples in the stator, and by resistance in the rotor. The B.E.S.A. Rules give only 75 degrees C. rise by resistance, so that there would be a definite gain by adopting the higher limits, especially as regards the rotor. I think it is necessary to recognize that hot spots have not been eliminated altogether, although great improvements have been made. At the same time there is certainly no reason to be uneasy about this point, as the hot-spot corrections are very small for well-designed machines.

In connection with the comparison between the temperature-drop for 6 600- and 10 000-volt machines, raised by Mr. Longman, it should be remembered that the temperature-drop is given on the assumption that the electrical loading is the same in each case. There is no difficulty in building 10 000- to 12 000-volt machines with the same degree of safety as for 6 600 volts. No bending tests were carried out on the bare copper. The question of overloads would, of course, have to be taken into account when specifying the rating of a machine. Short-time overloads should be avoided, as they tend to produce uneconomical designs. More consideration was given to the stator insulation because

this problem is more difficult than that of the rotor insulation. I think all engineers will agree with Mr. Longman that only the very best materials should be employed for insulation, whether for stator or rotor. Ordinary soft solder should not be used for joints in the rotor.

Liverpool Sub-Centre, at Liverpool, 28 February, 1921.

Mr. E. M. Hollingsworth: We should try to further any proposition towards reduced capital cost and increased efficiency of electric generating plant, provided it is not at the sacrifice of reliability. I think the crux of the question is this: Are we going to sacrifice anything in the direction of reliability by adopting the proposals put forward by the author? Experience with alternators has been generally satisfactory during recent years, and unless there is going to be a very great advantage obtained it is not advisable to make any great change in the design or construction of such plant. Many engineers have from time to time been concerned about "hot spots," and I can sympathize with the author in his desire for more co-operation between manufacturers and users of detectors. In the case of one machine of which I had control, manufactured by Messrs. Dick Kerr of Preston, and designed by the author, I passed some sleepless nights owing to a hot spot on the top of the yoke. It is not an easy matter to ascertain the correct temperature of such a surface, and the spot in question seemed to get hotter every time we felt it. The machine has not to my knowledge given any trouble and, in all probability, is working at a higher temperature than that which the author advocates. I do not think engineers will disagree with the author's plea for the adoption of temperature detectors, and I am rather surprised that manufacturers have not done more in this direction in the past. In this connection I should like to ask what increase in capital cost the temperature detectors suggested would represent. I consider manufacturers have been backward in this respect, for, speaking as an operating engineer, I should have welcomed years ago such an instrument to give us some indication that we were not exceeding at any spot the temperature guaranteed. I should like to ask the author what is the possibility of exceeding the guaranteed temperature in the case of an alternator which has been allowed to become dirty. He will probably advocate the closed air system, but not all engineers are using this system. Now we are all agreed that some revision of the determination of temperature-rise is required, and the author will tell us it can be done at very little expense. I should like to refer to his summary of the arguments in support of an increase in temperature (see page 292): "In the case of machines running at the same speed, those with high temperature rating are cheaper." That is true, but will this mean any great decrease in capital cost? It is a question of material; I know that there will be a reduction in material, but the labour costs will not be reduced very much, and in any case will the reduced capital cost compensate for the reduced reliability? I cannot understand what the author means when he says: "Higher efficiencies are obtainable," for I cannot see how a decrease in copper can mean increased efficiency. Perhaps he will clear up this point in his reply. He goes on to say: "Larger units can be built, or higher speeds can be adopted for machines of certain sizes," but I maintain that with our present knowledge of the steam turbine this is the limiting factor, and we cannot deal with the alternators without taking the turbines into consideration. The author states that he is prepared to build alternators of considerably greater capacity than at present, at a speed of 3 000 r.p.m. With my present knowledge of turbo-alternators I should not like to install one of more than 8 000 kW capacity at 3 000 r.p.m., therefore I do not see that any great claim can be made for higher alternator speed until we can get a reliable turbine. I should like to endorse what the author says in reference to the rotor windings and the present methods of building up the same. On two or three occasions one of our machines broke down owing to the movement of the coils in the rotor, but the rotor in question was not on the lines of the one shown by the author on the screen, the design of which gives much more confidence.

Mr. F. J. Teago: The two fundamental points in the paper are: first, that maximum total temperature is the rational way of specifying the output of a machine in terms of safe heating; and second, that the current maximum value of the total temperature is, if anything, low. I am surprised that one still hears temperature-rise referred to. With regard to the second point, the author proposes that the limit of the ultimate temperature, as measured on the conductors in the core, should be 160° C. The existing rule is that, for mica, paper, and such materials, the limiting ultimate temperature shall be 125° C. This figure, then, would be measured on the end windings and not in the slots, since the insulation in the slots is practically pure mica. Now, in general, if the end windings were worked at a total temperature of 125° C., the temperature of the windings in the slots would be 160° C., which is perfectly safe, since micanite tubes will withstand temperatures far in excess of anything yet proposed. Since the end windings form the weak spot, and 125° C. measured there would rate the machine as high as 160° C. measured in the slots, why not use the end windings as the point of reference? The end windings also have the advantage of accessibility. In any case, the author's proposals are in no way revolutionary, because the running temperature of existing plant is close to the figures which he gives.

Professor E. W. Marchant: I should like to ask the author whether he has ever adopted a method of measuring the temperature-rise of windings by means of their increase in resistance, that is, to measure the increase in resistance by superimposing a small direct current on the alternator supply. I do not think a scheme of that kind would be impracticable. It would not, of course, be of very great value if thermo-couples were fitted, but if a machine were

built without thermo-couples and one wanted to find the average temperature-rise of the windings, it would be useful. With regard to thermal resistivity, we made a few experiments on some insulating materials used on buried cables and we found that apparently, although the alteration was not very marked, there was a diminution in the resistivity with increase of temperature. I should like to know whether the author has found that in his mica wrappings.

Mr. A. E. Malpas : In his opening statement the author gives 40 degrees C. as the limiting temperature-rise generally adopted in this country, but on the Continent I have generally seen 45 degrees C. specified, and the difference of 5 degrees, of course, gave the Continental makers an advantage in competition. The measurement of the temperature of a machine externally by means of a thermometer has always struck me as being a very crude method, leaving the true internal temperatures very much in the region of the unknown, and this fact, no doubt, has led to the adoption of the low temperature-rise of 40 degrees or 45 degrees C. measured externally. I have always been struck by the anomaly of the same temperature-rise being specified in any given case, no matter whether the machine was to be installed in an airy room where normal temperatures would always exist, or in a warm locality—for instance, a hot engine-room or in the tropics. With an inside temperature of 43° C. in the shade I have wondered how the insulation of a large installation of motors and other electrical machinery would stand up, although nothing very serious actually happened. Report No. 72 of the B.E.S.A. does not lay any very great stress on the use of embedded temperature detectors, except that it suggests that a liberal number of these devices should be employed if their use be adopted. This would add something to the cost of the machine, although the addition may not be very serious in the case of really large apparatus. With regard to rotor circuits, it is easy, by means of the increase-of-resistance method, to ascertain the working temperature of most of the existing machines while running, but a knowledge of this temperature alone would not enable an overload to be carried with safety. The temperature of the stator copper also must be known. With regard to the type of indicating device, some years ago I used a Le Chatelier thermo-couple instrument to measure the temperature of hot gases, but I found that the millivoltmeter used in conjunction with the thermo-couple was so sensitive as to be seriously interfered with by stray fields. There were no direct currents anywhere near the instrument, but the conclusion I came to was that atmospheric disturbances caused a great deal of interference with the readings. It was very hot, dry weather at the time, such as is very seldom experienced in this country, but it is conceivable that atmospheric influence could at times have the same effect here. For this reason, the resistance thermometer would appear to offer a great advantage, more especially as the "Null" method of measurement is more accurate than any direct-reading method, since a Clark cell or other cell of constant voltage can be used for the balancing. I agree that in the case of large machines of new design it is highly desirable to provide such embedded temperature detectors in order to determine what are the safe limits at which to run the machine, but whether the advantage to be gained by fitting these devices to every large machine constructed would justify their cost is open to some doubt. It must not be lost sight of that their use would involve some danger since, if the detectors did not happen to be placed at the hot spot of maximum temperature, their readings might induce the operator to overload his machine beyond the real danger point. Referring to Fig. 7, it may be recalled that a good many cases of breakdown have occurred in the insulation of bars at the point where they left the protection of the slot, and this was proved to be due to vibrations set up at the end connections, and was surmounted, at all events in some Continental makes of machines, by increasing the thickness of the insulation at that point of the bar. It is interesting to note that many American machines of large size have been built to a specification of a maximum temperature of 150° C., the presumption being that this guarantee applies chiefly to the rotor circuits, with some economy in field copper.

Mr. C. Rettie : Although I am chiefly interested in the electric propulsion of ships, perhaps it will not be out of place to raise one or two points in connection with the paper. I refer in particular to the design of alternators for ship work, particularly where large units are concerned, such as the battle cruisers which are being built in America, the horse-power of which is as much as 180 000 for one ship alone, the motors being of 22 000 h.p. I take it that the paper refers to present practice only, but it is within the sphere of this subject to discuss future requirements in the design of alternators and motors for ship propulsion, the conditions of which are totally different from anything we have on shore. The sudden reversal of motors totalling 180 000 h.p. from full-speed ahead to full-speed astern, and also the question of the propellers rising out of the water and sudden turning movements, when the excitation of the alternator field has to be increased much above the normal to prevent the motors falling out of step, necessitate a safe temperature limit in the design of machinery for the same. In the *General Electric Review* for April, 1919, there is an interesting account of the temperature of the alternators during the trials of the U.S. battleship "New Mexico":—

"An evidence of the conservative design of the generators is the recorded temperature-rise during the maximum-load endurance run, the power developed being 31 000 h.p., and the speed of the ship 21·3 knots. The maximum temperature-rise on the armature windings as measured by temperature coils was 30 degrees C. The temperature coils were located between the upper and lower layers of the windings in the slot at the centre of the coil, and recorded the approximate hot-spot temperatures. The rotor or field temperatures as indicated by increase in resistance of the field winding indicate a safe working margin on the guaranteed actual temperature of 150° C., allowing for the temperature of the air entering the generator to be as high as 45° C."

In the later ships, as well as in the "New Mexico,"

a new instrument has been installed for measuring the field temperatures, an account of which is given in the *General Electric Review* for February, 1920, an extract from which bears on the present paper:—

"The field temperature indicator is an instrument that shows the temperature of the field conductor and depends for its operation on the change in resistance of a conductor with change in its temperature. It really indicates electrical resistance, but instead of a scale of resistance a scale in temperature in degrees Fahrenheit is used. This instrument is employed on the alternator field only, no such instrument being necessary on the field of the synchronous motor."

The author's remarks on the construction of alternators remind me of the time when I was engaged on the construction of dynamos in Canada. We used wooden pins on the spiders for holding the coils, but the centrifugal force set up when the armature was revolving wore away the pins so much that the end wires broke. On this account the wooden pins were done away with and iron pins (insulated with asbestos and paper) used instead. In conclusion, I should like to say that though I have not had any actual experience with large alternators, particularly with reference to ship work, I feel that we should be alive to any new developments which take place from time to time. We have no experience of ship work in this country to draw from, unless we take into account the "Wulsty Castle." I feel that we are being left behind in the development of the electric propulsion of ships; other countries such as the United States are building electrically-driven ships of all kinds, and these are proving very successful.

Mr. G. A. Juhlin (*in reply*): With reference to the question of reliability which has been raised by Mr. Hollingsworth, it would not be sound policy to suggest any change in the temperature limits unless one could be perfectly satisfied that it could be done without sacrificing reliability, and I believe this to be the case. The cost of a temperature-detector equipment complete would not exceed £200, so that the expenditure is relatively small. It is, of course, quite possible to exceed the temperature guarantees on a machine which is allowed to become dirty, but by the use of embedded temperature detectors this is guarded against, because the internal temperatures are measured, whereas with the surface-temperature measurements it would be quite possible to have internal temperatures which could not be detected, with consequent danger of burning out the machine. A closed air circuit undoubtedly has very great advantages in the way of keeping machines clean, and this system is rapidly gaining favour amongst operating engineers. With regard to the question of efficiency, it is not so much a question of reducing the amount of copper in machines by adopting a higher temperature limit, as of a general change in design, and this change means higher efficiencies. The amount of copper in a machine may actually be increased on account of the higher electrical loading, which enables us to obtain lower iron losses; and in many cases it would be possible to design machines with smaller rotor diameters, which also means a gain in efficiency due to the reduced windage losses.

I think Mr. Hollingsworth is taking a pessimistic view of the turbine situation, as there are certainly no difficulties in producing satisfactory turbines of much higher output than 8 000 kW.

In reply to Professor Marchant, I would say that we have tried at different times to obtain resistance measurements on machines running on short-circuit by superimposing a small direct current on the alternating-current supply, but we have not attempted to apply it to a machine running at full voltage. With regard to thermal resistivity, we have not been able to obtain any definite figures as to whether there is a diminution in the resistivity with increase in temperature.

SUMMARY OF LECTURES ON "MAGNETIC SUSCEPTIBILITY OF LOW ORDER"

By Professor E. Wilson, Member.

(*Delivered before* The Institution 17 *and* 24 *February, and* 10 *March*, 1921.)

Introduction.

In presenting the following brief notes on Magnetic Susceptibility of low order, the author is conscious of certain restrictions, as he is primarily dealing with the subject from the standpoint of practical applications. Perhaps the most important problem at the present time is that of magnetic separation of minerals, and therefore their susceptibilities must be considered. Since the magnetic properties of certain minerals can be changed by oxidation or reduction, it follows that this and other aspects of the problem must receive attention. But apart from magnetic separation the subject of rock susceptibility has a bearing on magnetic survey work, a knowledge of the subject is necessary to those engaged on the construction of scientific instruments, and there are possibilities of its application in other directions. An inspection of Table 1 of volume susceptibilities shows how great is the variation.

Table 1.

Substance	Volume-susceptibility κ_v
(a) Minerals.	
Magnetite, maximum value varies from	3·07 to 0·12
New Zealand iron sand	0·19
Magnetite, very impure (Bettws Garmon)	0·056
Iron sulphide (artificial)	0·022
Pyrrhotite	0·00575 0·000337
Dolerite (Owthorpe)	0·0045
Franklinite	0·0025 0·0037
Ilmenite	0·0015
Specular hematite (Nova Scotia)	0·0011
Granite (Mt. Sorrel, Leicestershire)	0·0010
Chrome iron ore	0·00050 0·00032
Chalybite, massive	0·00056
Spathic ore (Cleveland)	0·00048
Green serpentine (Cornwall)	0·00035
Red serpentine (Cornwall)	0·000041
Hornblende	0·00033
Wolfram ore (Cornwall)	0·00030
Ironstone-oolitic green carbonate (Irthlingborough)	0·00030
Nickel sulphide, native	0·000183
Red hematite (Cumberland)	0·0002
Brown hematite (Nova Scotia)	0·00011
Monazite	0·00010
Zinc-blende	0·000022 0·000012

Table 1—*continued.*

Substance	Volume-susceptibility κ_v
(a) Minerals—continued.	
Iron pyrites, cubic	0·00015 0·0000015
Iron pyrites, rhombic	0·00002
Copper pyrites	0·000027
Fergusonite	0·000072
Arsenical pyrites	0·000011
Tinstone crystal	+0·000011 −0·000001
Tinstone crystals in matrix	+0·000017
Mica, Bengal ruby-clear *	0·000012 0·000008
Mica, Bengal ruby-spotted *	0·0147 0·000287
Tourmaline *	0·00013 0·000086
Limestone	0·000006
Cobaltite	0·0000058
Sapphire, Al_2O_3 (Siam)	0·0000057
Glasses (various) *	+0·0000025 −0·00000015
Antimony sulphide, native	− 0·00000085
Felspar	0·0000016
Ruby, Al_2O_3 (Mysore)	0·0000019
Zircon	0·0000005
Corundum crystal, Al_2O_3	− 0·00000136
Apatite	− 0·00000072
(b) Metals.	
Manganese-iron * (13 per cent Mn)	before heat treatment 0·0004 after heat treatment 0·530
Chromium	0·000031
Tungsten	0·000006
Platinum	0·000029
Manganese	0·000084
Aluminium	0·0000018
Tin	0·00000019
Silver	− 0·0000021
Copper	− 0·0000073
Antimony	− 0·0000063
Bismuth	− 0·000014
(c) Fluids.	
Manganese sulphate (molecular normal solution)	0·000017
Ferrous sulphate (molecular normal solution)	0·000015
Water	− 0·00000075

* *Proceedings of the Royal Society*, A, 1920, vol. 96, p. 429.

I. Instrumentation.

(1) When the volume-susceptibility (κ_v) is larger than about 0·15, the correction for "end-effect" with short thick bars becomes too uncertain if the magnetometer is employed. Weiss * in his important work on magnetite crystals prolonged the specimen in each direction by soft iron bars, the whole being placed in a solenoid. A ballistic galvanometer was placed in circuit with a secondary coil wound on the specimen in two concentric sections. The magnetizing force was inferred in the now well-known manner from the difference in electromotive force induced in the two sections on reversal of the magnetizing current. Fig. 1 has been taken from the original paper and shows the construction so well that no further comment appears necessary, beyond the fact that the whole of the apparatus shown is surrounded by the solenoid.

According to another method † the specimen was inserted in rectangular holes cut in the pole-pieces of a ring-shaped yoke of Stalloy. A ballistic galvanometer was placed in a secondary circuit wound on the specimen, and the yoke was wound with a magnetizing coil. The arrangement is illustrated in Fig. 2, from which it will be seen that the yoke is in three sections, each of which has a radial gap cut in it with parallel sides 2 cm. apart. The central section has, in addition, two

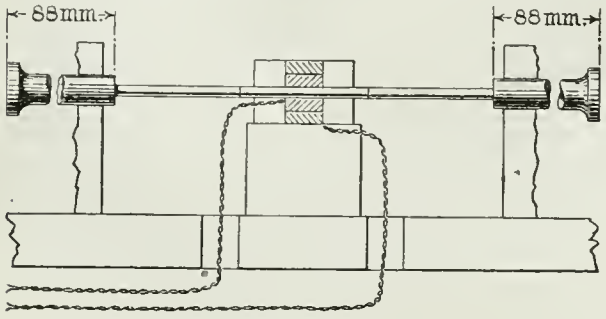

Fig. 1.

rectangular recesses to accommodate the test-piece, which is inserted by tilting the upper section. The length of the specimen is 4 cm., and it has a square section 1 cm. across each side.

(2) *The magnetometer.*—The ordinary arrangement in the case of metals is to use a long thin wire wound with a magnetizing coil. The specimen is usually placed in a vertical position, and the magnetometer needle is near the upper end. When testing rock specimens whose volume-susceptibility varies from about 0·15 to 0·001, the magnetizing solenoid is placed in a horizontal position, with the needle in a prolongation of its axis. By supplementing the magnetometer with an induction balance, Rücker ‡ was able to measure susceptibility as low as 0·00013, below which he considered no reliable figures could be obtained by this method.

(3) *The torsion balance.*—Although Tyndall in 1851 had used a torsion balance for comparing susceptibilities in his work on Diamagnetism, possibly the earliest application of this instrument to the measurement of magnetic susceptibility was that described by Wiedemann,* in which he supports the substance at the end of a horizontal beam suspended by a torsion wire, and measures susceptibility by the force of attraction or repulsion due to a magnetic field. At a later date Curie † published an account of the balance known by his name. The upper limit of volume-susceptibility with this instrument is about 0·1, or at the most 0·15, and with it measurements can be made to less than 1×10^{-6}. It will be seen, therefore, that it covers the range of the magnetometer, which can be used as a check. An idea as to the order of magnitude of the error due to "end effect" can be obtained by a test of Hey Tor magnetite. When found by the yoke method above described the value of the volume-susceptibility (κ_v) was 0·60 with a magnetic force $H = 15$. When tested as a powder in the torsion balance at this force the value was 0·10. Even with legitimately high values it is necessary to employ a weak field, since the specimen would be attracted by

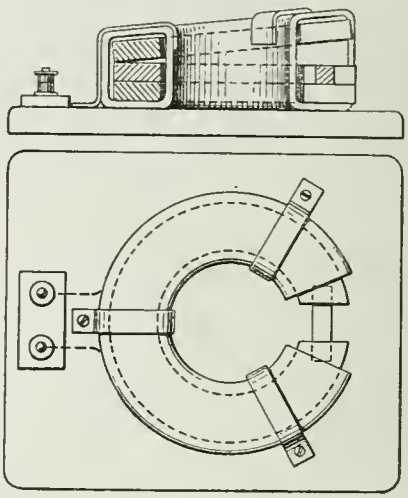

Fig. 2.

the pole-pieces and the test thus be rendered impossible. It appears, therefore, that if the instrument is to have a wide range of utility it must be so made that the strength of the magnetic field can be varied.

An instrument constructed on these lines is shown in Fig. 3, in which an electromagnet M is free to move on circular rails (1, 1) and its motion is controlled by a hand-operated screw (2). The terminals (3) are connected by flexible conductors to the magnet winding. The test-piece (4) is supported at one end of the horizontal beam (5), and the effective radius between the centre of the test-piece and the centre of suspension is 10·9 cm. A piece of sheet aluminium (6), which supports the beam, is suspended from the head-piece (7) by a strip of phosphor-bronze (8). By means of a ring (9) whose inside thread engages that cut on the outer surface of an inner tube, the moving system can be taken on or off suspension. The inner tube is prevented from rotating by screws (10, 10) which work in slots cut in the outer tube. A sliding weight (11) and

* *L'Eclairage Électrique*, 1896, vol. 7, p. 487.
† *Proceedings of the Physical Society*, 1919, vol. 31, p. 299.
‡ *Proceedings of the Royal Society*, 1900, vol. 48.

* *Monats-bericht de l'Académie de Berlin*, June, 1865.
† *Annales de Chemie et de Physique*, sec. 7, 1895, vol. 5.

a rider (12) are used to balance the specimen. A mirror (13) is employed, in conjunction with a spot of light and scale, for the purpose of measuring the angular deflections. An extension of the aluminium plate is submerged in an oil bath (14) for the purpose of damping. Four sighting pins (15) are so placed that the intersection of the two straight lines joining obliquely opposite pins coincides with the true centre of suspension. A storage cell is used to supply the magnetizing current which is measured by an ammeter. A reversing switch is placed between the magnet and cell for the purpose of removing the effects of residual magnetism.

A portable torsion balance has been developed * in which the test-piece hangs between fixed soft-iron pole-pieces which are magnetized by two concentric rings of hardened tungsten steel with north and south poles at opposite ends of a diameter. By varying the relative azimuths of the rings the field between the pole-pieces can be varied. In this manner a variable field can be obtained without the use of a magnetizing coil and battery.

(4) *An electromagnetically controlled balance.*—In the case of the torsion balance the deflection varies as the square of the magnetic force impressed upon the test-piece. In fact it is difficult to measure the susceptibility of a given specimen outside a narrow range of magnetic force of about one to four or five. An instrument has been developed † which permits of a wide range of variation of the magnetic force. The force due to torsion is replaced by an electromagnetic control in which the mechanical force exerted is due to two components—one proportional to the magnetic force impressed upon the test-piece, and the other also proportional to the magnetic force in the case of constant susceptibility, but variable if the susceptibility varies.

* *Proceedings of the Physical Society*, 1919, vol. 31, p. 338.
† *Proceedings of the Royal Society*, A, 1920, vol. 98, p. 274.

The instrument shown in Fig. 4 is a modification of the above, in that electromagnets are replaced by permanent magnets M M_1 after the manner of the portable torsion balance above described. The suspending fibre (1) which supports a moving coil (2) is of phosphor-bronze and requires 0·005 dyne-cm. to twist it through 1°. It is attached to a head-piece so arranged that the moving coil can be centralized when the instrument is level, and the position of zero torsion in the fibre is so adjusted that a pointer (3) attached to the horizontal beam (4) reads zero on the scale (5). The test-piece (6) is supported in a grip which hangs from the beam, and its position at a radius of 10 cm. is accurately secured by a knife-edge resting at the bottom of a V cut in the beam. The moving coil is rigidly fixed to the beam, and the specimen is counterweighted by a slider (7). The beam is shown just resting on a ring of brass (8) which is fixed to a frame (9) capable of an up-and-down movement

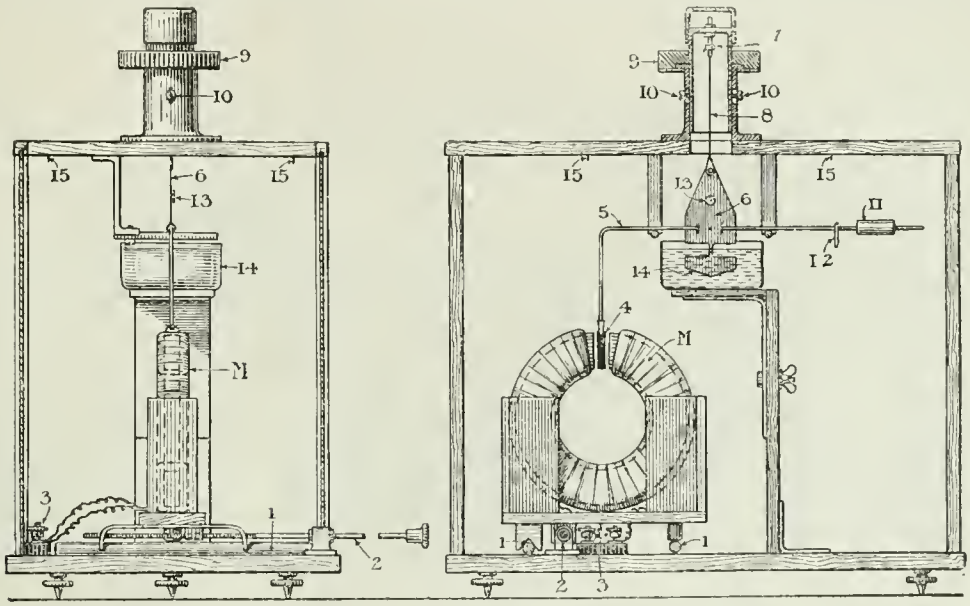

Fig. 3.

on the brass tube (10). The adjustment is made by turning the milled-edge nut (11). Lateral movement of the ring is prevented by a fork-piece (12) and a vertical guide (13). Two adjustable stops (14, 14) allow of the beam being firmly gripped when the ring is raised and the fibre is slack. A disc (15) is attached to the moving system and moves freely in the gap of a damping magnet (16). The current for the moving coil is carried by a conductor to the torsion head and passes through the coil to the terminal (17).

The position of the magnet M must be such that when the test-piece is just about to leave the field the pointer is at zero of the scale, thus eliminating the torsion in the fibre. The current in the moving coil is supplied by a dry cell working through an adjustable slide resistance of 2 300 ohms, and it is measured on a micro-ammeter, which is provided with 1/10 and 1/100 shunts. The moving coil system and ammeter combined are also provided with two shunts. The

current in the moving coil can be varied throughout its entire range by three stages, in each of which the adjustable slide resistance is used. The first stage takes the current up to 120 micro-amperes, the moving coil and ammeter being heavily shunted. The second stage varies the current from 120 to 1 200 micro-amperes, the moving coil and ammeter being less heavily shunted, and the instrument alone shunted 1/10. The third stage varies the current from 1 200 to 12 000 micro-amperes, the ammeter alone being shunted 1/100.

If the strength of the magnetic fields of M M_1 are fixed in value then the mass susceptibility, for equal weights, is proportional to the current in the moving coil. But the entire range ($\kappa_v = 0{\cdot}15$ to less than

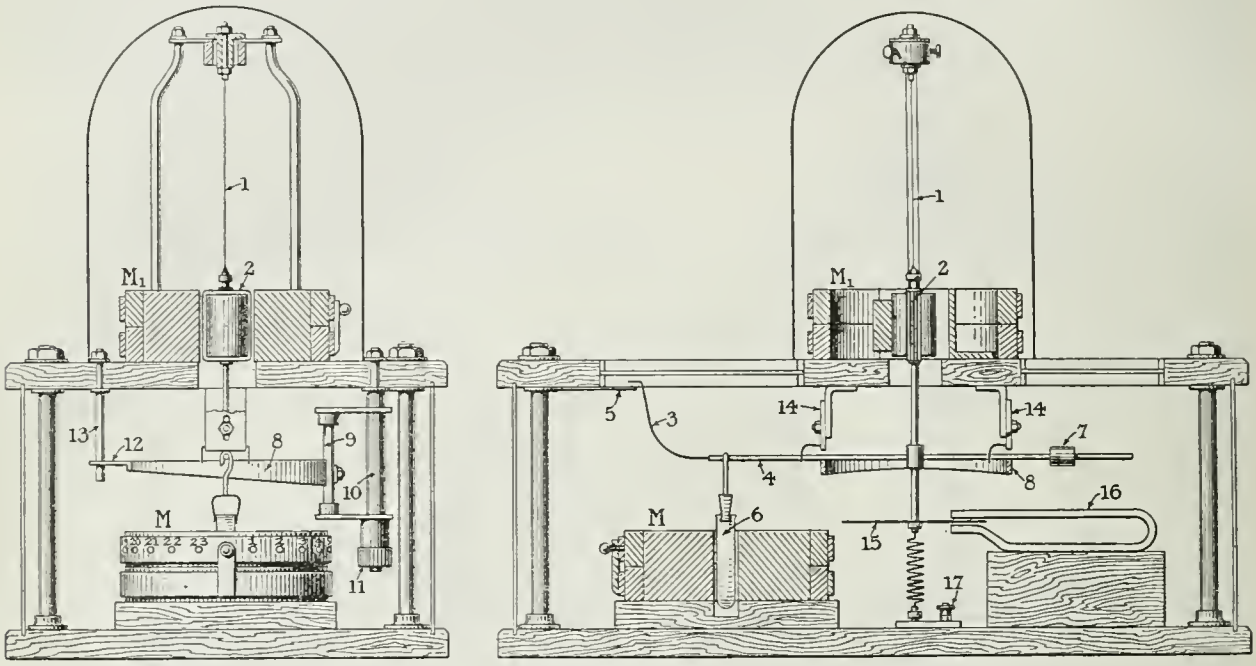

Fig. 4.—Form of torsion balance.

1×10^{-6}) would necessitate a weakening of the field in the magnet M when the susceptibility became high, in which case, assuming the field due to M_1 unaltered, the ratio of the squared values of the fields due to M has to be taken into account.

(5) Quincke's manometer method for the measurement of the susceptibility of solutions has been used by many experimenters, and requires an intense field when low susceptibility values are in question.

II. Susceptibility Values.

Magnetite.—The magnetic properties of magnetite have been examined by a number of workers. The retentivity of short and long bars has been studied by Abt (1894) who compared it with that of glass-hard steel. Weiss (1896) in his well-known work on magnetite crystals showed that they have curves of magnetization resembling those of iron, and that the susceptibility along the axes of the crystal varies in magnitude for a given magnetizing force, although all tending to the same limiting value. Weiss shows that the volume-susceptibility of magnetite crystals can have a maximum value of the order of 10 C.G.S. units, and this is the largest susceptibility of any known natural substance, if one excludes meteorites and traces of metallic iron. Holm (1903) has made a study of the susceptibility exhibited by Swedish magnetite, and has shown that it varies with the magnetizing force.

The term "magnetite" is usually taken to include rock specimens ranging from the crystal itself to compact or massive specimens and detached particles, and it exhibits a wide variation of magnetic properties. In all cases, however, the susceptibility varies with the magnetic force, the variation being comparatively small in the lower grades. Some information * can be gathered from Table 2, the data in which has been obtained from specimens of magnetite from different parts of the world. The crystal from Traversella furnishes a high value of volume-susceptibility (κ_1) which is not so high as that exhibited by Weiss in his curves.

The Arkansas magnetite will interest engineers on account of its high retentivity and coercive force. Experiment shows that the value of κ_1 for this variety was increased about 42 per cent on heating to 1 000° C. The residual magnetism was also increased but the coercive force was reduced, and this agrees with what happens in the case of hardened steel.

As Arkansas magnetite is so highly retentive and may possibly be found useful for the purposes of permanent magnets, it is of interest to compare its magnetic

* *Proceedings of the Physical Society*, 1919, vol. 31, p. 299.

properties with those of hardened tungsten steel. The ergs per cycle per cubic cm. were 90 000 when the value of $H_{max.}$ was 525, whereas they are of the order 200 000 in tungsten steel. The coercive force of the tungsten steel is roughly half that of this magnetite, but the retained magnetism is very much larger, the values being about 10 000 in the case of tungsten, and 1 300 in the case of this magnetite. That reverse force H which, being less than the coercive force gives a maximum value to the product BH, is about 39 in tungsten steel, as against 80 in this magnetite, and the maximum values of BH are 270 000 and 46 000 respectively. Table 2 shows that the intensity J retained by a 4-cm. bar after being in a magnetic field H of 18 000 C.G.S. units has diminished 20 per cent in 2¼ years.

The effect of heating Penryn magnetite (A) to 1 000° C. is to increase the susceptibility, coercive force and retentivity. The intensity of magnetization J in the unheated (C) 4-cm. bar has only diminished 3·5 per cent in 2¼ years.

Table 2.

Description	Specific Gravity of Specimen	$H_{max.}$	Magnetizing Force at Maximum Susceptibility	Maximum Susceptibility, κ_v	Coercive Force for $H_{max.}$ = 525	$B_{max.}$ for H = 525	Ergs per Cycle per c. cm. for $H_{max.}$ = 525	Intensity of Magnetization J retained after being in a field of 18 000 C.G.S. units		
								After 3 hours	After 6 days	After 2¼ years
Traversella (crystal)	5·06	567	22·5	3·12	12·2	4 495	23 200	2·81	1·84	0·9
New York	4·86	569	31·5	1·46	16·8	3 270	14 000	7·45	6·75	3·0
Hey Tor, Devon	4·30	525	49·0	0·90	23·8	2 528	15 300	7·6	2·3	0·8
Penryn, Cornwall, A	4·56	525	236·0	0·49	110·0	2 680	81 600	60·6	57·8	53·0
Penryn, Cornwall, C	4·59	525	368·0	0·31	95·0	2 300	58 000	48·2	48·0	46·5
Arkansas	4·68	525	298·0	0·363	150·0	2 580	89 600	69·7	66·3	55·2
Arkansas	4·74	525	315·0	0·348	150·0	2 450	81 800	68·7	65·2	—
Altenfjord, Norway	4·02	525	175·0	0·272	65·0	1 760	13 900	27·8	24·7	17·5
Lake Champlain, New York	4·14	525	43·8	0·172	15·2	1 112	720	3·08	2·34	2·3
S. Manchuria	3·40	525	140·0	0·127	31·0	1 132	1 660	6·12	5·35	3·2
Magnetite-calcite, Arran	3·40	525	85·0	0·129	12·1	1 080	151	1·89	1·14	0·0
Manganese steel (after heat treatment)	7·68	788	228·0	0·54	108·0	3 012	68 800	62·0	49·7	—

A very impure magnetite from Bettws Garmon, which may be described as an altered carbonate or secondary rock, has a κ_v value of 0·06. Strictly speaking, it could hardly be described as a magnetite. Magnetite calcite also cannot be regarded as a true magnetite, as it consists of detached particles of magnetite in a matrix of carbonate of lime.

Some natural minerals.—Having regard to the composition of magnetite ($FeO.Fe_2O_3$) it is of interest to examine other natural minerals whose composition can be represented by a formula of the same type. In franklinite (Zn, Mn)O.Fe_2O_3 the FeO is replaced by either of the oxides of Zn or Mn, and its volume-susceptibility (κ_v) has been examined in the case of two specimens. The values obtained were $2\,270 \times 10^{-6}$ and $3\,200 \times 10^{-6}$. Two specimens of chrome iron ore (Fe, Mg)O.(Cr, Al)$_2O_3$ gave κ_v values of 219×10^{-6} and 328×10^{-6}, and the susceptibility of pleonaste (Fe, Mg)O.Al_2O_3 was found to be $92{\cdot}3 \times 10^{-6}$. Comparing these figures with those of magnetite one sees how greatly the susceptibility is reduced by the exchange of either the ferrous or ferric oxides. The entire absence of iron as in gahnite [(ZnO.Al_2O_3); $\kappa_v = 47{\cdot}4 \times 10^{-6}$] and spinel [(MgO.$Al_2O_3$); $\kappa_v = 2{\cdot}20 \times 10^{-6}$] is associated with further reduction in susceptibility.

It is well known that the susceptibility of red hematite is small, and two specimens gave κ_v values of 120×10^{-6} and 181×10^{-6}. The brown variety is also known to be feebly magnetic, and a κ_v value of 117×10^{-6} is recorded. When this specimen was powdered and de-hydrated its susceptibility was further reduced to 64×10^{-6}. That the susceptibility of natural Fe_2O_3 is not always small is shown by three specimens of specular hematite which gave values of κ_v equal to $2\,530 \times 10^{-6}$, $1\,000 \times 10^{-6}$, and 508×10^{-6}. These values are large compared with those of red or brown hematite, but as will be shown later they are smaller than can be obtained from Fe_2O_3 prepared by natural and artificial processes.

Ferric oxide.—It is generally believed that the susceptibility of ferric oxide is extremely small in comparison with that of magnetic oxide, and this statement is certainly borne out if one considers the above figures obtained from brown or red hematite. Specimens of specular hematite, however, give considerably higher values than either the brown or red hematite, and this is suggestive of a variable susceptibility in the case of the ferric oxide. As far back as 1860, Robbins pointed out that ferric oxide obtained by oxidizing magnetic oxide was nearly as magnetic as the magnetic oxide itself. Liversidge (1892) called attention to the

fact that ferric oxide which has been produced on iron by long exposure to the air was comparatively highly magnetic, analysis having shown that no ferrous oxide was present.

A recent determination in the case of iron rust proved to be free from ferrous oxide gave a value of κ_m equal to 0·0044, and this was reduced to 0·003 after heating to redness. Taking the value of κ_m ($17·5 \times 10^{-6}$) found in the case of de-hydrated brown hematite we see that iron rust can have a value 250-fold.

Ferric oxide obtained by the ignition of magnetic oxide or ferrous oxide has a κ_m value of 0·036, which is 2 000 times the above.

It would be of interest to examine a true hematite which was known to have been produced from magnetite by natural processes. A so-called hematite formed in this way was tested by Abt,* but the analysis indicated the presence of a large proportion of magnetic oxide.

Another example which will interest engineers is that of the mineral chalybite (spathic ore or ferrous carbonate).† A given specimen which was evidently highly oxidized on the surface owing to long exposure was tested as regards the susceptibility of the surface layer and the innermost unweathered portion. The value of κ_m of the surface layer was found to be $34·7 \times 10^{-6}$, whereas for the innermost portion the value of κ_m was 103×10^{-6}. The highly oxidized portion has a susceptibility almost identical with specimens of red or brown hematite. After roasting with free access of air both the oxidized layer and the innermost portion gave almost the same values of κ_m, namely, 0·014 and 0·0138. It would appear therefore that the high susceptibility found in ferric oxide is associated with the passage through the stage of magnetite during its production. An inspection of the figures given in Table 4 in connection with chrome iron ore and hornblende shows a reverse effect, that is, a diminution is produced by heating.

Ferrites.—It has long been known that certain compounds termed "ferrites," in which the ferrous oxide is replaced by that of other elements as in franklinite, are magnetic. In this connection reference should be made to the original papers of Elliot (1862); List‡ (1878); Hilpert§ (1909) and many others.‖ An examination of these papers discloses the fact that the authors were principally concerned with transformation temperatures, and even the later workers have not apparently given absolute values of the susceptibilities. List may be said to have done pioneer work in this subject, and he examined a number of ferrites. Using a small permanent magnet which was weakly magnetic, he found that calcium ferrite ($CaO.Fe_2O_3$) and nickel ferrite ($NiO.Fe_2O_3$) were strongly attracted—to give but two illustrative examples. Hilpert has examined a large number of ferrites, and refers to earlier workers, notably Elliot, Mulagutti (1863) and List, and to the later work of Bemmelen and Klobbie (1892) and Hauser (1907). He points out that the double ferrite of lead and copper is as strongly magnetic as copper ferrite itself, and further that the susceptibility exhibited by lead-copper-ferrite depends upon whether it is quickly or slowly cooled. When slowly cooled over a period of 5 to 6 hours this ferrite is weakly magnetic. Recent experiments with copper ferrite show that its susceptibility can be reduced by slowly cooling. Mainly from the standpoint of lack of absolute values, a number of ferrites have been recently examined, and the results are summarized in Table 3. Reference should be made to the original paper* for their mode of preparation and other information.

Sulphides.—The susceptibilities of a number of sulphides are given in the general Table 1. Artificial iron sulphide (FeS) is well known to have a high susceptibility, and is extraordinarily retentive. The mineral pyrrhotite, known as magnetic iron pyrites, has a variable susceptibility, not only as regards the magnetic force H acting in a given direction in the crystal, but also as regards direction for a given magnetic force. Its susceptibility is not so high as that of the artificial sulphide, but is the highest found from natural sulphur compounds. It also is very retentive. Iron pyrites (FeS_2) occurs in the rhombic and cubic varieties, and the susceptibility in each case is very small. This is remarkable, and cannot be accounted for by iron content. The susceptibilities of nickel sulphide (native), copper pyrites, arsenical pyrites, cobaltite in descending order of magnitude are all positive, and antimony pyrites (native) has a very small negative susceptibility.

TABLE 3.

Substance	Formula	κ_m
Ferric oxide from basic acetate	Fe_2O_3	0·000063
Ferric oxide from brown hematite	Fe_2O_3	0·000017
Ferric oxide from sodium ferrite	Fe_2O_3	0·000213
Ferric oxide from Fe_3O_4 (dried artificial)	Fe_2O_3	0·036
Cobalt ferrite	$CoO.Fe_2O_3$	0·0105
Nickel ferrite	$NiO.Fe_2O_3$	0·010
Copper ferrite	$CuO.Fe_2O_3$	0·033
Zinc ferrite	$ZnO.Fe_2O_3$	0·000258
Cadmium ferrite	$CdO.Fe_2O_3$	0·000234
Magnesium ferrite	$MgO.Fe_2O_3$	0·00310
Aluminic and ferric oxides	$Al_2O_3.Fe_2O_3$	0·00108
Chromic and ferric oxides	$Cr_2O_3.Fe_2O_3$	0·000418

The effects of heating and cooling freely in air certain of the above minerals in powder form will now be dealt with, and reference must be made to Table 4. In the first experiment some rhombic iron pyrites (FeS_2) was tested after it had been heated for 30 minutes in a closed vessel. When cold the value of κ_m was found to be 600×10^{-6}. This powder was then systematically

* *Wiedemann Annalen*, 1899, vol. 68, p. 4.
† *Philosophical Transactions of the Royal Society*, A. 1919, vol. 219 (Appendix), p. 89; also *Journal I.E.E.*, 1919, vol. 57, p. 416.
‡ *Deutsche Chemische Gesellschaft Berichte*, 1878, p. 1512.
§ *Deutsche Chemische Gesellschaft Berichte*, 1909, p. 2248.
‖ See the works of Winkelmann and Chwolson.

* *Proceedings of the Physical Society*, 1921, vol. 33.

TABLE 4.

No.	Substance	Maximum Temperature	Duration of Maximum Temperature	Weight	Magnetic Force H	Mass-susceptibility κ_m in C.G.S. Units $\times 10^{-6}$
	Rhombic iron pyrites (powder)—	°C.	Mins.	g.		
1	(*a*) Before heating	13	—	6·23	670	4·7
2	(*b*) After heating (*a*) in a closed vessel	650	30	3·215	160	600
3	(*c*) After heating (*b*) in air	360	5	2·713	58	889
	(*d*) After heating (*c*) in air	450	4	2·495	24	4 000
	(*e*) After heating (*d*) in air	500	16	2·025	35	3 830
	(*f*) After heating (*e*) in air	515	60	2·00	35	2 950
	(*g*) After heating (*f*) in air	630	15	1·96	34	1 070
	(*h*) After heating (*g*) in air	610	35	1·94	34	613
	(*i*) After heating (*h*) in air	900	15	1·64	60	332
	Rhombic iron pyrites (powder)—					
1	(*a*) Before heating	12	—	5·59	318	3·0
2	(*b*) After heating some of (*a*) in air	155	30	4·72	318	3·3
3	(*a*) Before heating	12	—	6·64	610	2·6
	(*b*) After heating (*a*) in air	257	4	6·62	450	3·9
	(*c*) After heating (*b*) in air	300	5	6·61	442	4·7
	(*d*) After heating (*c*) in air	350	4	6·61	450	5·8
4	(*a*) Before heating	13	—	6·06	715	2·6
	(*b*) After heating (*a*) in air	400	4	5·83	40	1 120
	(*c*) After heating (*b*) in air	360	3	5·80	40	1 175
	(*d*) After heating (*c*) in air	500	4	4·12	34	2 410
	(*e*) After heating (*d*) in air	600	6	3·91	119	266
	(*f*) After heating some of (*e*) in air	950	30	2·42	318	120
	Cubic iron pyrites (powder)—					
1	(*a*) Before heating	12	—	5·32	616	1·5
	(*b*) After heating (*a*) in air	290	4	5·32	686	1·8
	(*c*) After heating (*b*) in air	430	7	4·97	30	1 500
	(*d*) After heating (*c*) in air	500	30	3·76	61	1 230
	(*e*) After heating some of (*d*) in air	950	15	1·61	310	49
	Copper pyrites (powder)—					
1	(*a*) Before heating	12	—	4·39	318	6·4
2	(*b*) After heating (*a*) in a closed vessel	650	30	4·17	160	480
3	(*c*) After heating (*b*) in air	290	5	3·06	60	2 880
	(*d*) After heating (*c*) in air	435	5	3·11	30	1 480
	(*e*) After heating (*d*) in air	515	34	3·54	30	1 430
	(*f*) After heating (*e*) in air	515	8	3·61	30	1 390
	(*g*) After heating (*f*) in air	950	30	3·01	30	2 650
4	(*h*) After heating some of (*a*) in air	950	30	0·94	34	4 480
	Pyrrhotite (powder)—					
1	(*a*) Before heating	13	—	6·42	60 160	400 1 060
2	(*b*) After heating (*a*) in a closed vessel	650	30	6·10	8·5 29·4	3 600 4 900
	Chrome iron ore (powder) $FeO.Cr_2O_3$—					
1	Before heating	12	—	3·84	258	124
2	After heating in air	650	30	—	257	50
	Hornblende (powder) Mg.Ca and Fe silicates—					
1	Before heating	12	—	—	99	111
2	After heating in air	750	30	—	98	85

heated in air for the stated interval of time, at temperatures gradually increasing in value until 900° C. was reached, the powder being tested when cold between each successive pair. A remarkable increase in the value of κ_m occurs after a few minutes' heating at 450° C. The susceptibility subsequently falls until after the last test it has a comparatively low value. This then is the value of κ_m for ferric oxide produced in this manner, assuming complete oxidation of the sulphur and pure FeS_2 initially. In the second experiment some of the native iron pyrites (rhombic) was heated in air from the start. The first batch was raised to 155° C., and the value of κ_m was practically unaltered. The second batch was heated three times, the final temperature being 350° C., and still no material increase in κ_m occurred. The third batch was heated to 400° C. and produced a large increase in susceptibility. Finally the value of κ_m was reduced until it was equal to 120×10^{-6}. In the third experiment some cubic pyrites in powder form was tested in a similar manner, and here again the critical temperature is about 400° C. The final value of κ_m is smaller than in the previous cases. In the fourth experiment some copper pyrites ($Cu_2S.Fe_2S_3$) was tested by heating for 30 minutes in a closed vessel to 650° C., the susceptibility rising to the moderate value of 480×10^{-6}. Heating the same powder in air for 5 minutes at a temperature of 290° C. produced a large increase in the value of κ_m. After heating this batch for 30 minutes at 950° C. the susceptibility showed an increase. This point was further tested by heating a fresh batch of the native mineral directly to 950° C., and maintaining it at this temperature for 30 minutes. The comparatively high value of $4\,480 \times 10^{-6}$ was then obtained, and should be compared with the value $33\,000 \times 10^{-6}$ obtained with copper ferrite. This is an interesting example of the effect of the formation of the double oxides ($2CuO.Fe_2O_3$). The last experiment was made with powdered pyrrhotite by heating it in a closed vessel for half an hour at about 650° C. Owing to the initially high value of the susceptibility, a serious increase would not be expected, nor was it obtained. The increase, however, is substantial, and a further interesting point is that the susceptibility is still a variable in respect to the magnetic force. The analysis of these compounds at the various stages of heating is still under consideration.

It will be readily understood from these experiments that a different susceptibility can be obtained by heat treatment according to fineness of division and access of air, and this is an important matter in the event of large quantities having to be dealt with.

Though not properly coming under the group of sulphides, heating tests of chrome iron ore and hornblende are recorded in Table 4. A considerable reduction in susceptibility was produced in each case. Another specimen of chrome iron ore showed a similar reduction in susceptibility on heating, whilst a third specimen showed a slight increase on heating, but its initial susceptibility was lower.

Transformation temperatures.—The transformation temperatures of natural and artificially prepared minerals and alloys have been examined by many workers, notably by Curie, Weiss, Barton and Williams, Allan, Hilpert, Wologdine, Wedekind, Honda, and Ishiwara. Published figures are given in Table 5.

Metallic salts.—The magnetic properties of salts were investigated by Faraday, followed by Wiedemann, Quincke, Meslin, S. Meyer, Curie, Chéneveau and many others.

The mass-susceptibility (κ_{m1}) of a solution of a salt in water is given by the formula:

$$\kappa_{m1} = \frac{p}{100}\kappa_m + \left(1 - \frac{p}{100}\right)\kappa_{m0}$$

where κ_{m0} is the mass-susceptibility of water; which may be taken as $-0{\cdot}75 \times 10^{-6}$, κ_m is the mass-susceptibility of the water-free salt, and p the percentage weight of salt in solution.

TABLE 5.

Material	Temp. °C.
Magnetite	535
	525
Iron phosphide (Fe_3P)	445
Pyrrhotite	320
	300
Pyrrhotite (nickeliferous)	300
Iron carbide (Fe_3C)	180
Iron and tungsten carbide ($2Fe_3C.3W_2C$)	80
Franklinite	61
Calcium ferrite	160
Potassium ferrite	150
Lead ferrite	200
Copper ferrite	280
Cobalt ferrite	280
	290
Manganese antimony alloy	320
	330
Manganese arsenide	40
	45
Manganese phosphide	18
	26
Manganese bismuthide	360
	380

As manganese sulphate (anhydrous) has proved to be a valuable standard of reference in work on low susceptibility, it will be taken as an illustrative case. The values given in Landolt-Börnstein's (1912) tables for the mass-susceptibility (κ_m) of manganese sulphate are (98, 114, 100, 85) 10^{-6} C.G.S. units at atmospheric temperature. The mean of the four values is 99×10^{-6} C.G.S. units. A solution of this salt containing 0·055 g. of manganese, or 0·151 g. of sulphate per cubic centimetre was placed in a test-tube having an internal diameter 0·867 cm., the total volume of the solution being 2·36 c. cm. After correction for the glass tube itself, it was found, with the use of the balance (Fig. 3) that the value of κ_v was $93{\cdot}6 \times 10^{-9} \times 190 = 17{\cdot}8 \times 10^{-6}$.

For this solution $p = 15{\cdot}1$, and using the mean value of κ_m (99×10^{-6}) we obtain by calculation

$\kappa_{m1} = +14{\cdot}5$ C.G.S. units. The density of the solution is 1·17, so that the volume-susceptibility κ_v is $+16{\cdot}95 \times 10^{-6}$, and this is slightly smaller than the value $17{\cdot}8 \times 10^{-6}$ obtained with the balance.

It is interesting to draw a comparison between the above susceptibility and that of ferrous sulphate. A solution was tested containing 0·056 g. of iron, or 0·152 g. of ferrous sulphate per c. cm., the total volume being 2·36 c. cm. as before. The value of κ_v given by the balance was $93{\cdot}6 \times 10^{-9} \times 150 = 14{\cdot}0 \times 10^{-6}$ C.G.S. units. The solution used had a density 1·15 and a value of $p = 15{\cdot}2$. We obtain by calculation $\kappa_m = +11{\cdot}8 \times 10^{-6}$, and $\kappa_v = +13{\cdot}59 \times 10^{-6}$, if we adopt a mean value of $\kappa_m = 82 \times 10^{-6}$ obtained by rejecting the last two entries in Landolt and Börnstein in the series (93, 75, 82, 84, 75, 37, 51) 10^{-6} C.G.S. units.

It is an interesting fact that copper has a negative susceptibility and its salts are positive, whereas magnesium is positive and its salts are negative.

Wiedemann showed as early as 1865 that paramagnetic salts when hydrated give a higher value than the anhydrous salts, although water itself is diamagnetic.

III. Practical Applications.

Magnetic Survey.

The magnetic method of surveying iron ore deposits has long been employed,* and an interesting example is that carried out by Mr. G. W. Walker, F.R.S., in 1917 in the Melton Mowbray and Irthlingborough districts.† This survey differed from similar work in which magnetite was the disturbing mineral, in that oolitic carbonate ore was under consideration. The susceptibility (κ_v) of this ore was of the order 0·0003, and the disturbing forces due to it were therefore relatively very small, thus increasing the difficulty of the work. Those interested in the magnetic survey of the British Isles are referred to the work of Rücker and Thorpe,‡ Rücker and White,§ Rücker,‖ and Walker who has designed and used a special instrument or variometer¶ for magnetic survey work.

Magnetic Separation.

So far as records go, the history of magnetic separation would appear to have commenced in 1792, when Fullerton obtained certain patent rights in England. The first important commercial undertaking was commenced in 1858 by M. Stella in the Traversella Mines in Piémont, the object being to separate magnetite from copper pyrites. The subject was taken up in 1880 in Scandinavia and later in America in connection with extensive deposits of magnetite. An early form of magnetic separator is that due to C. W. and Ll. B. Atkinson.** In 1896, at Franklin, New Jersey, U.S.A., Wetherill demonstrated the possibility of dealing with the magnetic separation of feebly magnetic materials, the minerals concerned being franklinite and zinc-blende. At the present time so far as actual bulk is concerned magnetite is of the first importance, but magnetic separation has found applications in other directions. Statistics given by Professor Henry Louis at the Inter-Allied Chemical Conference in Paris, 1919, show that in 1914 in Northern Norway about 2 200 000 tons of ore were dealt with, yielding 900 000 tons of concentrates. At Mineville,* New York State, U.S.A., about 1 200 000 tons of ore were dealt with in 1916, and in 1917 in Sweden 2 500 000 tons of ore, yielding 1 100 000 tons of concentrates, are recorded. Of course these figures do not refer to such important applications as the separation of wolfram from tinstone ores, or the separation of monasite for use in the gas mantle industry.

Before dealing with the various types of apparatus used in the separation of minerals, the susceptibilities met with in some of the more important cases may profitably be studied. Table I gives the susceptibility values of the particular mineral or minerals to be separated, together with those which are important and most likely to be met with. It must not be inferred that they are the only ones concerned as conditions vary in different localities.

(1) *Magnetite.*—The susceptibility of even low-grade magnetite which may be of the order 0·1 is still so high as to overwhelm that of any other rocks. It is largely associated with apatite, which has a small negative susceptibility and which gives rise to trouble, not from a magnetic standpoint, but because it takes the form of an impalpable powder and adheres to the magnetite most tenaciously. Its removal has to be specially dealt with by washing or by air-blast, as the case may be.

(2) *Wolfram.*—In the separation of wolfram ore from tinstone there is none too large a margin in favour of the wolfram ore. A great field is open for treatment.

(3) *Monazite.*—The deposit in which monazite is found contains amongst other minerals ilminite and zircon. This is a case of double separation, the ilminite being extracted by a weak field, and the monazite by a strong field, the zircon going forward as a non-magnetic mineral. Great care must be taken, if grinding of the ore has to be resorted to, that the zircon, which is very hard, is not rendered magnetic by minute traces of iron.

(4) *Spathic ore.*—In the separation of this ore from zinc-blende or copper pyrites, there is a fair margin in favour of the former, and there is scope for treatment.

(5) *Franklinite.*—The separation of this mineral from zinc-blende was one of the early problems solved by Wetherill.

(6) *Specular ore.*—When occurring with magnetite, as in the Norwegian schists, a double separation is involved, the magnetite being first removed by a weak field, and the hematite by an intense field.

A great deal of ingenuity has been spent upon the construction of the machinery used in magnetic separation. In the case of magnetite, the preliminary rough

* Iron Ore Resources of the World (Ore Field, Kurnuavaara), Stockholm, 1910. Nordenström. *Journal of the Iron and Steel Institute*, 1898, p. 35. Haanel: "On the Location and Examination of Magnetic Ore Deposits by Magnetometric Measurements," Ottawa, Canada, 1904.

† *Philosophical Transactions of the Royal Society*, A, 1919, vol. 219 (Appendix); *Proceedings of the Royal Society*, A, 1920, vol. 96, p. 429.

‡ *Philosophical Transactions of the Royal Society*, A, 1890, vol. 181, and vol. 188 (1896), A.

§ *Proceedings of the Royal Society*, 1898, vol. 63.

‖ Address to the British Association, Section A, 1894.

¶ *Proceedings of the Royal Society*, 1916, vol. 92, p. 313.

** See British Patent, No. 17486/1890.

* *Transactions of the American Institute of Mining Engineers*, 1916, vol. 56, p. 928.

dry process (Wenström or Gröndal) is followed by either a dry or wet fine separation. The Edison separator is an example of the dry process, and that of Gröndal is extensively used in wet separation. In the case of feebly magnetic materials, e.g. monazite, the double separators of Wetherill, the Rapid Magnetic Machine Company or Humbolt are available. Only a brief description of the different types of separators is possible here. Information on this subject can be obtained on reference to papers read by Professor Henry Louis before the Allied Chemical Conference in Paris (1919), and the West of Scotland Iron and Steel Institute (1912).

The general lay-out in the case of the separation of magnetite is substantially the same whatever type of separator is used. The ore is first crushed to a size usually about 2 inches cube. The smaller pieces or

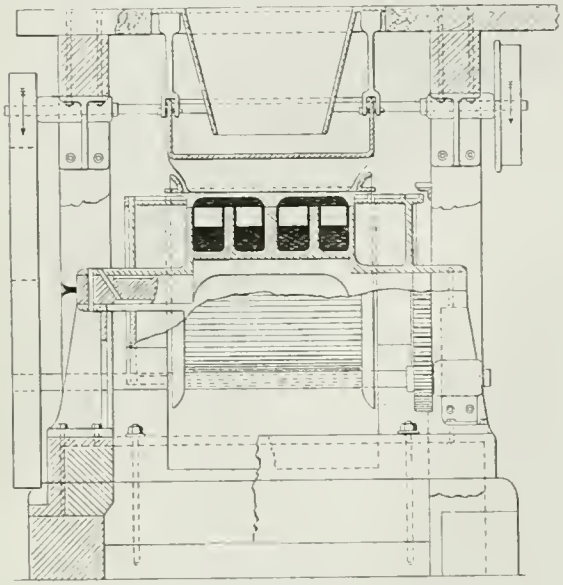

Fig. 5.—Wenström separator (sectional elevation).

"fines" up to about $\frac{1}{2}$ inch cube are screened out, and the larger pieces are passed on to a coarse separator. The Wenström machine is illustrated in Fig. 5, and may be taken as an illustrative case. It consists of a drum, revolving at about 30 r.p.m., whose length is 24 inches and diameter is 30 inches, and it is capable of dealing with about 7 tons per hour. The surface is composed of alternate longitudinal bars of wood and iron, the latter having inward radial projections which in the case of alternate bars come opposite the N and S poles of an electromagnet. These polar projections are sector-shaped and subtend an angle of about 180°, and magnetize those bars which occupy the front of the drum. When the ore is fed on to the top of the revolving drum, the magnetic pieces are attracted and carried round until the longitudinal bars become non-magnetic, when they drop down through a shoot. The non-magnetic pieces are carried round until an angle of 90° has been traversed, when they drop off under the action of gravity and pass through another shoot. Usually the waste from this separator contains about 5 per cent of magnetite and is passed on to the waste dump. The magnetic portions, together with the fines, are then stored in bins ready for the next wet crushing process, whence they pass to the first fine magnetic separator, which may be of either the wet or dry type.

The Gröndal wet machine is illustrated in Fig. 6 and is extensively used. A series of radial electromagnets are arranged so as to magnetize the front and lower portions of a horizontal rotating cyinder which consists of alternate brass and iron bars. The mixture of ground ore and water or "pulp" flows over a weir underneath the drum which attracts the magnetic portions and carries them forward, whilst the non-magnetic gangue drops down and is carried with the stream to the exhaust outlet. When nearing the limiting position as defined by the electromagnets, the magnetic particles or middlings meet a spray of water which washes them still farther and drives them on to a shoot for transference to a second separator. In this second machine the magnetic field is weaker, and thus a cleaner concentrate is produced. The drum rotates at about 100 r.p.m. and the centrifugal force acting upon the ore helps in the process of discharging it on to the shoot.

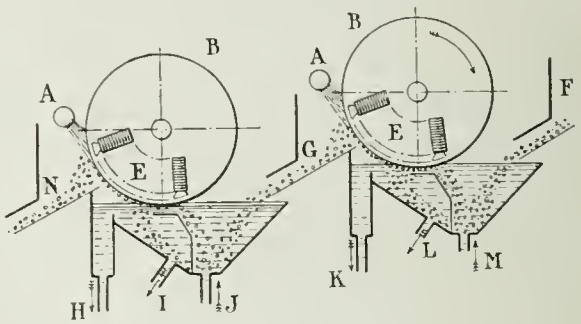

Fig. 6.—Gröndal duplex separator.

A, spray pipe.
B, rotating drum.
E, battery of magnets.
F, feed.
G, middlings.
H, K, slime and water.
I, L, tailings.
N, concentrate.
J, M, agitating water.

The drum has a width and diameter of 2 feet, and deals with about 5 tons of ore per hour.

The Ball-Norton dry separator, which is extensively used at Mineville, N.Y., U.S.A., consists of two rotating drums with electromagnets arranged radially as in the Gröndal machine, the first being more strongly magnetic than the second. The ore is carried by the first drum to the limit of the magnetic field, and the non-magnetic portions are dropped as in the Wenström machine and carried away by a shoot. The middlings from the first drum are collected by the second, and the non-magnetic portions and gangue are still further removed, the concentrate being carried forward as before. In order to free the magnetite from the very fine apatite dust the machines are subjected to an air-blast whose direction is opposed to that of the drum. This is one of the difficulties of dry separation, another being that the ore must be thoroughly dry. During this process it is not easy to regulate the temperature so that other portions of the ore, e.g. iron pyrites, are not rendered magnetic.

The Edison machine furnishes another example of

dry separation. In this the ore falls down by gravity through a series of suitable shoots and passes in turn the poles of a bank of electromagnets. The magnetic portion is attracted from the rest and thus separation is effected. The poles are $4\frac{1}{2}$ feet wide and each series of magnets can treat about 5 tons of ore per hour. As in the Ball-Norton machine, a blast of air is used to separate the fine apatite dust.

The concentrate produced by one or other of the above processes is passed on to wet fine grinders and thence to the final separators. It is then drained in settling pits or dried by other suitable means when wet separation has been used.

The separation of substances more feebly magnetic than magnetite involves the use of more powerful magnetic fields. In the case of dry separation the field must be strong enough to attract the substance in air, but in wet separation the difficulty due to the surface

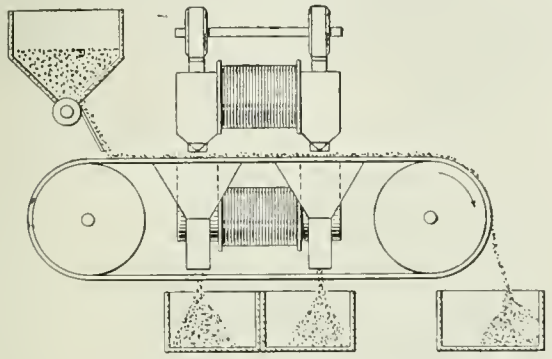

FIG. 7.—Rowand-Wetherill separator (elevation).

tension of the fluid has to be overcome. The Wetherill separator is shown in side elevation in Fig. 7. It is arranged for double separation as in the case of ilmenite, monazite and zircon, and the magnetic circuit consists of two horizontal iron cores wound with magnetizing coils and provided with pole-pieces. The lower edge of each of the upper poles is V-shaped to produce a strong magnetic field along its whole length, and an adjustment of the gap length can be made in order that the strength of the field may be varied. A horizontal belt which acts as the first ore conveyer runs on two pulleys, and its upper portion passes over the upper surfaces of the lower pole-pieces. At right angles to this belt are two others, each of which runs on three pulleys, and the lower portions pass along just under the sharp edges of the upper poles. The ore is fed from a hopper on to the first conveyer, and is carried forward towards the first pole-piece, which is sufficiently strong to attract the most highly magnetic particles. These impinge on the underside of the transverse belt and are carried by it out of the magnetic field, when they fall by gravity through a shoot into the first bin. The remaining ore passes on to the second pole-piece whose field is so adjusted as to attract the particles of intermediate susceptibility. These are removed in similar manner by the second transverse belt, and finally the least susceptible portions are carried by the conveyer to a receptacle at the extreme end. It will be seen from Table 1 that ilmenite, monazite and zircon have susceptibilities of descending order of magnitude, and these serve as an illustration. A standard machine treats about 1 ton per hour, and requires from 3 to 4 horse-power to drive it.

The dry separator supplied by the Rapid Magnetic Machine Company consists of a series of bell magnets, which rotate and throw off the attracted particles by centrifugal force. The air space between these magnets and the conveyer belt is varied after the manner already described, in order that the particles of different susceptibility can be collected in separate bins. With three bell magnets six different field strengths can be obtained during the passage of the ore, the non-magnetic portions finally dropping into a receptacle as in the Wetherill machine.

The Edison machine has been modified by the addition of sharp edged pole-pieces in order to deal with low susceptibilities.

The problem of the wet separation of feebly magnetic materials is dealt with in the Ullrich machine. A steel disc rotates on a vertical spindle at a speed of 15 to 30 r.p.m., and supports on its underside one or more wedge-shaped rings of soft iron. Each ring is capable of vertical adjustment in order to vary the strength of the magnetic field between the sharp lower edge and the opposite pole of the electromagnet. The lower stationary poles support a brass trough down which the mixture of ore and water or "pulp" flows. A continuous stream of water flows from the upper rotating disc down each side of the V-shaped rings. As each ring passes over the pole-pieces an intense field is formed, and the magnetic portions are attracted to the upper poles through a stream of water (thus avoiding surface tension). The rotating ring then carries the particles beyond the limit of the magnetic field, when they are washed off into a suitably placed shoot and so collected. The number of magnets varies from 4 to 10 according to the size of machine, and the strengths of the magnetic fields can be varied to suit the varying susceptibilities of the different minerals in the ore.

The ore to be separated in the case of very low susceptibilities cannot be dealt with before it has the degree of fineness necessary for the required purity. In the case of monazite sand the necessary degree of fineness already exists, otherwise the ore must be crushed and ground.

THE TRANSMISSION OF ELECTRIC WAVES ALONG WIRES.
A VECTOR METHOD INVOLVING ONLY ELEMENTARY MATHEMATICS.*

By A. G. Warren, B.Sc., Member.

(*Paper received 7 February*, 1921.)

Summary.

The first part of the paper embodies a determination, using elementary mathematics only, of the fundamental equations of a long transmission line. The interpretation of the equations is discussed. Subsequently, the discussion is extended to the growth of the wave system.

Introduction.

In the case of short transmission lines no difficulty is experienced in calculating the regulation with the necessary degree of accuracy. It is usually sufficient to consider the capacity as if it were concentrated at the mid-point of the line. With longer lines such a rough approximation is not justifiable. To obtain results of reasonable accuracy the line has to be considered as divided into a number of sections, each section being treated separately as a short transmission line. The arithmetical labour involved is considerable and liable to cumulative errors and possible mistakes.†

Much labour may be obviated and more exact results obtained by the direct application of the equations for the transmission of a periodic disturbance along a pair of wires. These equations are usually deduced from a pair of simultaneous partial differential equations expressing the relations obtaining in the flow of a current of any nature along the conductors.‡ For the case with which the engineer usually has to deal, the equations are needlessly general. Their complete solution is a matter of considerable difficulty, and it is rarely that one meets anything but the limited solution applicable to sinusoidal currents under stable conditions. In such cases the engineer usually applies vectors, even under circumstances of doubtful accuracy, rather than use formulæ obtained by mathematics of somewhat obscure physical significance. The vector method is probably the most graphic in describing any alternating-current phenomenon; it is certainly the method most generally understood.

The method set out below of solving the problem was evolved by the author in an attempt to discuss with students of limited mathematical knowledge the regulation of transmission lines. It is simply an extension of the approximate method in terms of vectors, the final exact solution being obtained by making the number of sections of the line indefinitely great.

In subsequent paragraphs the interpretation of the equations is considered. Finally, the equations are extended to the discussion of the growth of the wave system. The treatment is analogous to that used by Kennelly * in determining the successive jumps in voltage in free and earthed lines. The general case is discussed in the present article.

Determination of the Transmission Equations.

Symbols Employed.

E = electromotive force at any point.
I = line current at the same point.
R = resistance per unit length of line.
L = inductance per unit length of line.
C = capacity per unit length of line.
G = leakance per unit length of line.
Z = impedance per unit length of line.
Y = admittance per unit length of line.
r = resistance of a certain short length of line.
l = inductance of a certain short length of line.
c = capacity of a certain short length of line.
g = leakance of a certain short length of line.
z = impedance of a certain short length of line.
y = admittance of a certain short length of line.
f = frequency.
$\omega = 2\pi f$.
l is used for length also, but no confusion should arise in its two applications.

Let us consider a length x of the line divided into a large number, n, of equal sections (Fig. 1). For each

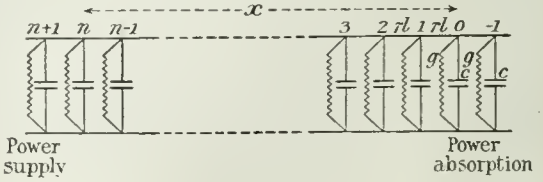

Fig. 1.

section we may imagine the capacitance and leakance concentrated; the inaccuracy of the approximation vanishes when n is made indefinitely great. In the limit it is clearly a matter of indifference whether the currents

* The Papers Committee invite written communications (with a view to publication in the *Journal* if approved by the Committee) on papers published in the *Journal* without being read at a meeting. Communications should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

† Useful practical methods of calculation of different degrees of accuracy, due respectively to Professor Fleming and Mr. McKinstry, are given in the *I.E.E. Journal*, (1) 1914, vol. 51, p. 717; and (2) 1920, Supplement to vol. 57, pt. 2, p. 92.

‡ These equations and a discussion of them will be found in Professor J. A. Fleming's "The Propagation of Electric Currents in Telephone and Telegraph Conductors," 2nd edition, p. 66.

* "The Application of Hyperbolic Functions to Electrical Engineering Problems," 2nd edition, 1916, p. 77.

are measured slightly to the right or left of the points of division, i.e. whether I_1 is the current in the element 01 or 12. For the former case the vector diagram is Fig. 2, and for the latter, Fig. 3.

For Fig. 2 we have:

$$\left.\begin{aligned} E_1 &= E_0 + zI_1 \\ I_1 &= I_0 + yE_0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

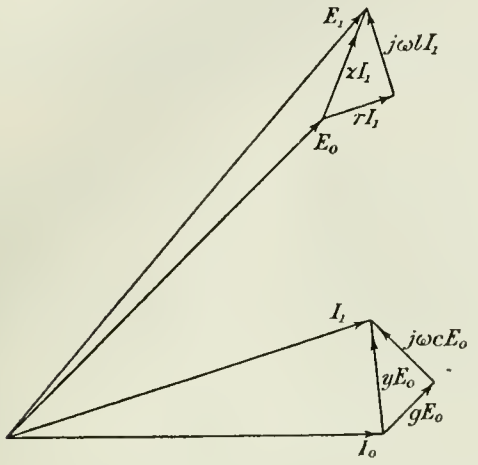

Fig. 2.

For Fig. 3 we have:

$$\left.\begin{aligned} E_1 &= E_0 + zI_0 \\ I_1 &= I_0 + yE_1 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

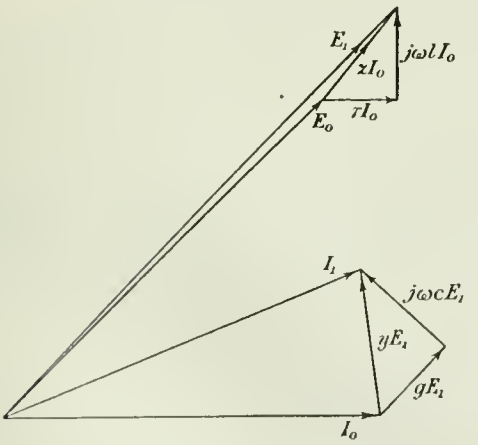

Fig. 3.

On the assumption just made we may select one value from each pair of equations, viz.

$$\left.\begin{aligned} E_1 &= E_0 + zI_0 \\ I_1 &= I_0 + yE_0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Similarly

$$\left.\begin{aligned} E_2 &= E_1 + zI_1 = E_0 + 2zI_0 + zyE_0 \\ I_2 &= I_1 + yE_1 = I_0 + 2yE_0 + zyI_0 \end{aligned}\right\}$$

and

$$\left.\begin{aligned} E_3 &= E_2 + zI_2 = E_0 + 3zI_0 + 3zyE_0 + z^2yI_0 \\ I_3 &= I_2 + yE_2 = I_0 + 3yE_0 + 3zyI_0 + zy^2E_0 \end{aligned}\right\}$$

It will be noticed that the numerical coefficients are those occurring in binomial expansions. It is readily proved by induction that, generally,

$$\left.\begin{aligned} E_n &= E_0 + nzI_0 + \frac{n(n-1)}{2!}zyE_0 \\ &\quad + \frac{n(n-1)(n-2)}{3!}z^2yI_0 + \text{. . . etc.} \\ I_n &= I_0 + nyE_0 + \frac{n(n-1)}{2!}zyI_0 \\ &\quad + \frac{n(n-1)(n-2)}{3!}zy^2E_0 + \text{. . . etc.} \end{aligned}\right\} \quad (4)$$

If we simplify one of these expressions, the simplification of the other can be written down from symmetry. Dealing with the former we have:

$$\begin{aligned} E_n &= E_0\left\{1 + \frac{n(n-1)}{2!}zy \right. \\ &\quad \left. + \frac{n(n-1)(n-2)(n-3)}{4!}z^2y^2 + \text{. . . etc.}\right\} \\ &+ \sqrt{\frac{z}{y}}I_0\left\{n(zy)^{1/2} \right. \\ &\quad \left. + \frac{n(n-1)(n-2)}{3!}(zy)^{3/2} + \text{. . . etc.}\right\} \quad (5) \end{aligned}$$

$$\text{or} \quad E_n = \left\{\frac{[1+\sqrt{(zy)}]^n + [1-\sqrt{(zy)}]^n}{2}\right\} + \sqrt{\frac{z}{y}}I_0\left\{\frac{[1+\sqrt{(zy)}]^n - [1-\sqrt{(zy)}]^n}{2}\right\} \quad . \quad (6)$$

But $\sqrt{(zy)} = \frac{x}{n}\sqrt{(ZY)}$. Making this substitution, and remembering that $\mathrm{Lt}_{n=\infty}(1 + p/n)^n = e^p$, we have:

$$\left.\begin{aligned} E &= E_0\left(\frac{e^{x\sqrt{(ZY)}} + e^{-x\sqrt{(ZY)}}}{2}\right) \\ &\quad + \sqrt{\frac{Z}{Y}}I_0\left(\frac{e^{x\sqrt{(ZY)}} - e^{-x\sqrt{(ZY)}}}{2}\right) \\ &\text{and similarly} \\ I &= I_0\left(\frac{e^{x\sqrt{(ZY)}} + e^{-x\sqrt{(ZY)}}}{2}\right) \\ &\quad + \sqrt{\frac{Y}{Z}}E_0\left(\frac{e^{x\sqrt{(ZY)}} - e^{-x\sqrt{(ZY)}}}{2}\right) \end{aligned}\right\} \quad . \quad (7)$$

$\sqrt{(ZY)}$ is called the propagation constant P of the line, and $\sqrt{(Z/Y)}$ the initial sending-end impedance Z_0. Making these substitutions we obtain:

$$\left.\begin{aligned} E &= \tfrac{1}{2}E_0(e^{Px} + e^{-Px}) + \tfrac{1}{2}I_0Z_0(e^{Px} - e^{-Px}) \\ I &= \tfrac{1}{2}I_0(e^{Px} + e^{-Px}) + \tfrac{1}{2}(E_0/Z_0)(e^{Px} - e^{-Px}) \end{aligned}\right\} \quad . \quad (8)$$

Equations (8) are often written in the form

$$\left.\begin{aligned} E &= E_0\cosh Px + I_0Z_0\sinh Px \\ I &= I_0\cosh Px + (E_0/Z_0)\sinh Px \end{aligned}\right\} \quad . \quad . \quad (9)$$

If x is measured in the direction of power transmission the equations become

$$\left.\begin{aligned} E &= E_0\cosh Px - I_0Z_0\sinh Px \\ I &= I_0\cosh Px - (E_0/Z_0)\sinh Px \end{aligned}\right\} \quad . \quad . \quad (10)$$

If an impedance $\bar{Z}$ is connected at the receiving end of a line of length l, the voltage E and the current I at the generator are given by

$$\left.\begin{aligned} E &= E_0 (\cosh Pl + (Z_0/\bar{Z}) \sinh Pl) \\ I &= I_0 (\cosh Pl + (\bar{Z}/Z_0) \sinh Pl) \end{aligned}\right\} \quad . \quad (11)$$

Interpretation of the Equations.

The physical meaning of the equations is made more obvious by substituting for P the sum of its real and imaginary parts, $\beta + j\alpha$. We find that

$$\left.\begin{aligned} \beta^2 &= \tfrac{1}{2}\{\sqrt{[(R^2 + \omega^2L^2)(G^2 + \omega^2C^2)]} + (GR - \omega^2LC)\} \\ \alpha^2 &= \tfrac{1}{2}\{\sqrt{[(R^2 + \omega^2L^2)(G^2 + \omega^2C^2)]} - (GR - \omega^2LC)\} \end{aligned}\right\} \quad . \quad (12)$$

β is called the attenuation factor, and α the wave-length constant. Substituting β and α in (8), we get

$$\left.\begin{aligned} E &= \tfrac{1}{2}(E_0 + I_0Z_0)e^{\beta x}(\cos \alpha x + j \sin \alpha x) \\ &\quad + \tfrac{1}{2}(E_0 - I_0Z_0)e^{-\beta x}(\cos \alpha x - j \sin \alpha x) \\ I &= \tfrac{1}{2}[I_0 + (E_0/Z_0)]e^{\beta x}(\cos \alpha x + j \sin \alpha x) \\ &\quad + \tfrac{1}{2}[I_0 - (E_0/Z_0)]e^{-\beta x}(\cos \alpha x - j \sin \alpha x) \end{aligned}\right\} \quad . \quad (13)$$

Consider one of these expressions, say that for E and, for the moment, only the first half of the expression. $\tfrac{1}{2}(E_0 + I_0Z_0)$ is a voltage vector; $e^{\beta x}$ is a numerical multiplier, which increases the voltage in the ratio e^{β} per unit length of line; $(\cos \alpha x + j \sin \alpha x)$ is an operator which advances the phase of the voltage vector by an angle α per unit distance. Or, viewing the line in the other direction, that is to say in the direction that power is being transmitted, the first component of the voltage at any point is decreasing in amplitude in the ratio $e^{-\beta}$, and receding in phase by an angle α, per unit distance. Exactly this state of affairs could be brought about by a wave travelling along the line, in the direction of power propagation, at a speed ω/α, the wave diminishing in amplitude in the ratio $e^{-\beta}$ per unit distance travelled. The wave-length is thus defined by α, and the attenuation by β.

The second term in the expression for E represents an exactly similar wave travelling in the opposite direction at the same speed, decreasing and changing in phase in the same manner. We thus see that the voltage at any point is consistent with the view that two pressure-waves are moving along it, attenuating as they travel. The first term in the expression for E is the resultant of an infinitely large number of forward wave-trains; the second is the resultant of all reflected wave-trains. The components will be considered later.

Clearly, the expression for I is made up in an exactly similar manner.

Infinitely Long Line.

Measuring x in the direction of power transmission, equations (10) may be written:

$$\left.\begin{aligned} E &= \tfrac{1}{2}(E_0 + I_0Z_0)e^{-Px} + \tfrac{1}{2}(E_0 - I_0Z_0)e^{Px} \\ I &= \tfrac{1}{2}[I_0 + (E_0/Z_0)]e^{-Px} + \tfrac{1}{2}[I_0 - (E_0/Z_0)]e^{Px} \end{aligned}\right\} \quad . \quad (14)$$

If $x = \infty$, $I = 0$; whence $I_0 = E_0/Z_0$. That is to say, Z_0 (the initial sending-end impedance) is the effective impedance of an infinitely long line, or of any line before reflection occurs.

Substituting, we get

$$\left.\begin{aligned} E &= E_0e^{-Px} \\ I &= I_0e^{-Px} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (15)$$

or

$$\left.\begin{aligned} E &= E_0e^{-\beta x}(\cos \alpha x - j \sin \alpha x) \\ I &= I_0e^{-\beta x}(\cos \alpha x - j \sin \alpha x) \end{aligned}\right\} \quad . \quad . \quad (16)$$

That is, in an infinite line, or in a finite line before reflection occurs, as the wave travels forwards at a velocity ω/α it attenuates in the ratio $e^{-\beta}$ per unit distance.

Change in the Constants of the Line.

Suppose that after a certain distance the line is joined to another of different sending-end impedance. The sending-end impedances of the two lines, and other constants, will be distinguished by the suffixes 1 and 2. At the junction, the forward wave (E, I) will give rise to a transmitted wave (E_t, I_t) in the second line, and a reflected wave (E_r, I_r) in the first line. Taking the positive direction in all cases as that of power transmission, we have:

$$\left.\begin{aligned} E + E_r &= E_t \\ I + I_r &= I_t \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$E/I = Z_1, \quad E_r/I_r = -Z_1, \quad \text{and} \quad E_t/I_t = Z_2$$

whence

$$\left.\begin{aligned} E_r &= \frac{Z_2 - Z_1}{Z_1 + Z_2}E \\ I_r &= \frac{Z_1 - Z_2}{Z_1 + Z_2}I \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (18)$$

and

$$\left.\begin{aligned} E_t &= \frac{2Z_2}{Z_1 + Z_2}E \\ I_t &= \frac{2Z_1}{Z_1 + Z_2}I \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (19)$$

The expressions for the reflected wave are of particular interest. They are proportional to the difference between the impedances of the two lines. If the direction of power transmission is reversed, the reflected wave becomes opposite in phase. It is well known that exactly the same occurs in the case of light waves.

Let us consider three particular cases:—

(1) If $Z_1 = Z_2$ there is no reflection.

(2) If the second line does not exist; i.e. if the first line is open at the receiving end, $Z_2 = \infty$,

$$E_r = E, \; I_r = -I, \; E_t = 2E, \text{ and } I_t = 0 \quad . \quad (20)$$

The zero current at the open end is composed of the current arriving, together with a reflected wave exactly equal and opposite to it. The voltage jumps to double the value.

(3) If the first line is short-circuited, $Z_2 = 0$,

$$E_r = -E, \; I_r = I, \; E_t = 0, \text{ and } I_t = 2I \quad . \quad (21)$$

This case is complementary to the last. The current is doubled at the short-circuit, and the voltage across the short-circuit is wiped out by an exactly equal and opposite reflected voltage.

Final Sending-end Impedance.

It has been seen that in the case of an infinitely long line, or for a finite line before reflection occurs, the effective impedance is Z_0, i.e. the initial sending-end impedance. By the time the current is fully established, the finite line acts as if it had a very different impedance, depending upon the nature of the load at the receiving end. Two cases of particular interest are: (*a*) when the line is open at the far end, and (*b*) when it is short-circuited at the far end.

Consider first the open line. If we put $x = l$ and $I_0 = 0$ in equation (9), we obtain

$$\left.\begin{aligned} E &= E_0 \cosh Pl \\ I &= (E_0/Z_0) \sinh Pl \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (22)$$

whence the effective impedance, or final sending-end impedance, is

$$E/I = Z_0 \coth Pl \quad . \quad . \quad . \quad . \quad (23)$$

For the short-circuited line we have $E_0 = 0$, giving

$$\left.\begin{aligned} E &= I_0 Z_0 \sinh Pl \\ I &= I_0 \cosh Pl \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (24)$$

whence the final sending-end impedance is

$$E/I = Z_0 \tanh Pl \quad . \quad . \quad . \quad . \quad (25)$$

For very long lines $\coth Pl = \tanh Pl = 1$,* and the final sending-end impedance is Z_0.

Growth of Current in a Line.

The final conditions in a circuit are not attained by steady growth. Each reflection brings about a finite jump in the current and voltage, successive jumps diminishing in magnitude.

We will consider the case of a line loaded at the far end. For the normal load impedance we will substitute a fictitious open-circuited line of propagation constant P_2, initial sending-end impedance Z_2, and length l_2, such that $Z_2 \coth P_2 l_2$ is equal to the actual impedance Z of the load (Fig. 4).

The switching-in of the generator starts a current wave travelling down the line, the wave attenuating as it travels. By the time it has reached B let the current be i. It gives rise to a reflected wave i_r and a transmitted wave i_t. The transmitted wave suffers attenuation and phase displacement during its travel from B to C, whence it is reflected to B, suffering further attenuation and phase displacement. At B it gives rise to two waves, ai_t and bi_t. The wave bi_t repeats on its own scale the process of the wave i_t. The return waves may be summed:

$$i'_1 = i_r + ai_t + abi_t + ab^2 i_t + \text{etc.} \quad . \quad . \quad (26)$$

These suffer attenuation and phase displacement on their passage from B to A, whence they are reflected, giving rise to a secondary train of waves, whose attenuated and phase-displaced value at B may be denoted by ci, a definite fraction of i.

* This is equally true for complex quantities and wholly real quantities.

If I is the final current, measured at B, we have

$$I = I_1 + I_2 + I_3 + \ldots, \text{etc.} = I_1 + cI_1 + c^2 I_1 + \ldots, \text{etc.} = I_1/(1 - c) \quad . \quad (27)$$

From (15), (18) and (19) we have

$$i_r = \frac{Z_1 - Z_2}{Z_1 + Z_2} i; \quad i_t = \frac{2Z_1}{Z_1 + Z_2} i; \quad i_1 = i_t e^{-P_2 l_2}; \quad i_2 = -i_1;$$

$$i_3 = i_2 e^{-P_2 l_2}; \quad ai_t = \frac{2Z_2}{Z_1 + Z_2} i_3; \quad bi_t = \frac{Z_2 - Z_1}{Z_2 + Z_1} i_3$$

whence

$$\left.\begin{aligned} a &= -\frac{2Z_2}{Z_1 + Z_2} e^{-2P_2 l_2} \\ b &= \frac{Z_1 - Z_2}{Z_1 + Z_2} e^{-2P_2 l_2} \end{aligned}\right\} \quad . \quad . \quad . \quad (28)$$

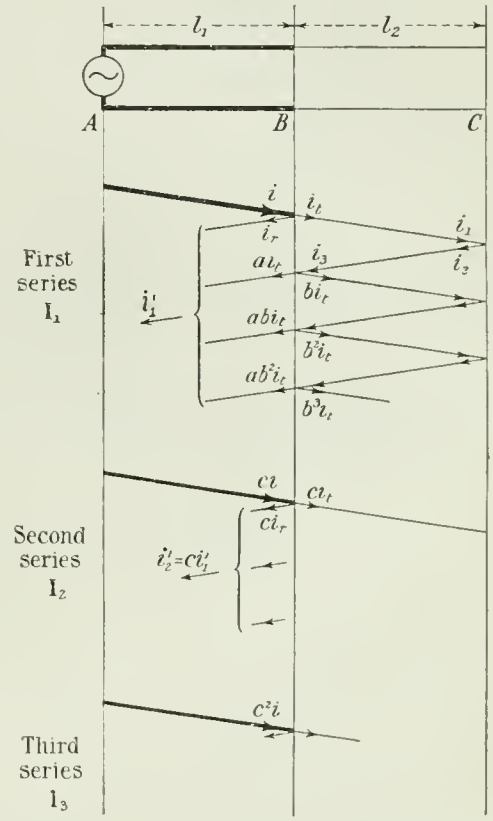

Fig. 4.

But $I_1 = i + i'_1$ which, from (26), gives us

$$I = i + i_r + ai_t/(1 - b) \quad . \quad . \quad . \quad (29)$$

Substituting, we obtain

$$i'_1 = \frac{(Z_1 - Z_2) - (Z_1 + Z_2)e^{-2P_2 l_2}}{(Z_1 + Z_2) - (Z_1 - Z_2)e^{-2P_2 l_2}} i$$

or

$$i'_1 = \frac{Z_1 - Z_2 \coth P_2 l_2}{Z_1 + Z_2 \coth P_2 l_2} i = \frac{Z_1 - Z}{Z_1 + Z} i \quad . \quad . \quad (30)$$

The return waves i'_1, after attenuation and phase displacement of $e^{-2P_1 l_1}$, give us

$$ci = i'_1 e^{-2P_1 l_1} \quad . \quad . \quad . \quad . \quad . \quad (31)$$

whence $$c = \frac{Z_1 - \bar{Z}}{Z_1 + \bar{Z}} e^{-2P_1 l_1} \quad . \quad . \quad . \quad . \quad . \quad (32)$$

The sum of all the forward trains at B is

$$I_{Bf} = i + ci + ci^2 + \ldots, \text{ etc.} = i/(1 - c) \quad . \quad (33)$$

The sum of the backward trains is

$$I_{Bb} = i_1' + i_2' + i_3' + \text{etc.} = i_1'/(1 - c) \quad . \quad (34)$$

whence $$I_{Bf} = \frac{Z_1 + \bar{Z}}{Z_1 + \bar{Z} - (Z_1 - \bar{Z})e^{-2P_1 l_1}} i \quad . \quad . \quad (35)$$

$$= \frac{(Z_1 + \bar{Z})e^{P_1 l_1}}{2Z_1 \sinh P_1 l_1 + 2\bar{Z} \cosh P_1 l_1} i \quad . \quad (36)$$

$$I_{Bb} = \frac{Z_1 - \bar{Z}}{(Z_1 + \bar{Z}) - (Z_1 - \bar{Z})e^{-2P_1 l_1}} i \quad . \quad . \quad (37)$$

$$= \frac{(Z_1 - \bar{Z})e^{P_1 l_1}}{2\bar{Z}_1 \sinh P_1 l_1 + 2Z \cosh P_1 l_1} i \quad . \quad (38)$$

The total current at B is

$$I_B = \frac{Z_1 e^{P_1 l_1}}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} i \quad . \quad . \quad (39)$$

To get the total current at any distance x from the generator the forward trains must be multiplied by $e^{P_1(l_1 - x)}$ and the backward trains by $e^{-P_1(l_1 - x)}$, whence we obtain the current at any point x

$$I_x = \frac{(Z_1 + \bar{Z})e^{2P_1 l_1 - l_1 x} + (Z_1 - \bar{Z})e^{P_1 x}}{2Z_1 \sinh P_1 l_1 + 2\bar{Z} \cosh P_1 l_1} i \quad . \quad (40)$$

If E_0 is the generator voltage, we have

$$i = \frac{E_0}{Z_1} e^{-P_1 l_1} \quad . \quad . \quad . \quad . \quad . \quad (41)$$

Substituting this value of i in (40) we get:

$$I_x = \frac{(Z_1 + \bar{Z})e^{P_1(l_1 - x)} + (Z_1 - \bar{Z})e^{-P_1(l_1 - x)}}{2Z_1 \sinh P_1 l_1 + 2\bar{Z} \cosh P_1 l_1} \cdot \frac{E_0}{Z_1} \quad (42)$$

or $$\frac{Z_1 \cosh P_1(l_1 - x) + \bar{Z} \sinh P_1(l_1 - x)}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} \cdot \frac{E_0}{Z_1} \quad (43)$$

whence $$I_0 = \frac{Z_1 \cosh P_1 l_1 + \bar{Z} \sinh P_1 l_1}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} \cdot \frac{E_0}{\bar{Z}_1} \quad . \quad (44)$$

$$I_B = \frac{E_0}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} \quad . \quad . \quad (45)$$

The voltage trains may be deduced in an exactly similar manner. A simpler method is, however, available. For the forward trains $E = Z_1 I$, and for the backward trains $E = -Z_1 I$. Hence it is readily seen that at any point distant x from the generator

$$E_x = \frac{(Z_1 + \bar{Z})e^{P_1(l_1 - x)} - (Z_1 - \bar{Z})e^{-P_1(l_1 - x)}}{2Z_1 \sinh P_1 l_1 + 2\bar{Z} \cosh P_1 l_1} E_0 \quad (46)$$

or $$E_x = \frac{Z_1 \sinh P_1(l_1 - x) + \bar{Z} \cosh P_1(l_1 - x)}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} E_0 \quad (47)$$

whence $$E_B = \frac{\bar{Z} E_0}{Z_1 \sinh P_1 l_1 + \bar{Z} \cosh P_1 l_1} \quad . \quad . \quad . \quad (48)$$

Determining the values of E_0 and I_0 in terms of E_B and I_B we obtain

$$\left.\begin{aligned} E_0 &= E_B(\cosh P_1 l_1 + \frac{Z_1}{\bar{Z}} \sinh P_1 l_1) \\ I_0 &= I_B(\cosh P_1 l_1 + \frac{\bar{Z}}{Z_1} \sinh P_1 l_1) \end{aligned}\right\} \quad . \quad (49)$$

Equations (49) are, *mutatis mutandis*, the same as (11).

THE MEAN ERROR OF AN ELECTRICITY METER.*

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Summary.

Questions concerning the accuracy of electricity meters in actual service have reference to the error of registration over a period, rather than the speed error at any particular value of the power in the circuit. There will be a certain load at which the speed error is the same as the error of registration over the period in question, and the purpose of the author is to investigate a method of ascertaining this load. From a consideration of the sources of error in electricity meters it appears that, in meters in which a constant frictional torque is the only disturbing factor, the error of registration over a period is the speed error at the average load over that period. In meters having in addition a disturbing torque proportional to the square of the speed, the error over a period will be slightly slower than the error at the average speed. A discussion of the effect of varying power factor leads to the result that the error of registration due to this cause is equal to that determined by a speed test at an average power factor deduced from the readings of an energy meter and a meter integrating the wattless component of the power in the circuit.

In agreements or regulations relating to the supply of electricity, a clause is frequently found to the effect that the electricity meter measuring the supply shall be deemed correct if the mean error does not exceed $2\frac{1}{2}$ per cent. A stipulation of this kind is ambiguous in that the expression "mean error" is not defined. In the case of a meter with a nearly linear characteristic, in which the error was one of calibration only, the percentage error over the whole working range would be sensibly constant, and any question as to the interpretation of the phrase "mean error" would not arise. In actual practice, however, nearly all electricity meters have a more or less imperfect characteristic, and, moreover, whilst in service they develop opposite tendencies to inaccuracy, the one towards slowness, due to the accumulation of friction, being most marked at low loads; the other, towards over-registration occasioned by the weakening of the brake magnets, appearing as a change in calibration and operating at all loads alike. In such a case a meter may exceed the limits of inaccuracy at certain portions of the working range, while being within such limits at others. In these circumstances the interpretation of the expression "mean inaccuracy" is of considerable importance, as upon this may depend the decision as to whether a meter is to be deemed inaccurate, such classification further fixing the question of the amendment of the consumer's account.

The expression "mean inaccuracy" can be interpreted in many ways. It is sometimes taken as the average of the errors determined at the customary loads of one-twentieth, one-quarter, one-half and full load. This method is on the face of it unsatisfactory, and obviously the weighted mean of the results of these four tests would be preferable. A further interpretation would be the true average inaccuracy as determined from the error curve of the meter. Still another meaning to attach to the phrase "mean inaccuracy" is the error at the average load of the consumer.

The correct interpretation of the phrase "mean inaccuracy" is suggested by the fact that, when the accuracy of a consumer's account is in question, the error actually required is the difference between the true energy and that registered on the dials of the meter. Revolution tests of a motor meter are concerned with the accuracy of its indications of the power in the circuit as given by the angular speed of the rotor, and, excepting the case of a uniform load, do not necessarily enable the inaccuracy of registration over a period to be properly determined.

If W_m be the watts indicated by the speed of the meter, W the true watts, η the error of the meter expressed as a fraction equal to one-hundredth of the percentage error:

$$W_m = W(1 - \eta)$$

If E_m and E be the metered and true energy respectively

$$E_m = E - \int_0^T W\eta dt$$

and the fractional error of registration will be

$$\frac{\int_0^T W\eta dt}{E}$$

The value of this expression will obviously depend upon the form of the function expressing the relationship between η and W, or, in other words, upon the shape of the error curve.

It is easy to deduce from theoretical principles the approximate relationship between the error and the load, which will apply to most types of motor meters. These approximate expressions, while not exactly explaining the forms of the error curves of meters as determined by experiment, are, nevertheless, sufficiently accurate to shed considerable light on the question of the error of registration over an extended period.

The simplest meters to consider from the point of view of their theoretical inaccuracy are the direct-current watt-hour dynamometer meter, and the commutator ampere-hour meter. In such meters the dis-

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turbing influence is due almost entirely to solid friction, which is approximately constant at all speeds. This being so, it follows that the percentage error is inversely proportional to the load. This error is the percentage deviation of the meter from the speed at which it would rotate if free from the disturbing influence of friction. The counting train of the meter is so adjusted that at the full load of the meter, when the influence of friction is relatively small, the meter is registering fast, thus tending to a better average accuracy. The fractional error of such a meter can therefore be put in the form

$$\eta = A - K/W$$

where A is the deviation from theoretical accuracy at which the registration of the meter dials is correct.

The actual error of registration of such a meter over a period will then be

$$A\int_0^T W dt - K\int_0^T dt = AE - KT$$

The error of registration expressed as a fraction of the total energy metered, is then

$$A - \frac{KT}{E} = A - \frac{K}{W_a},$$

W_a being the average load. The overall error of registration is then equal to the error of the meter at the average load as determined by the customary revolution test.

The true average of the errors between one-twentieth and full load of the meters as determined by testing is, W_1 being full load,

$$\begin{aligned} A - \frac{20}{19} \times \frac{K}{W_1}\int_{\frac{W_1}{20}}^{W_1} \frac{dW}{W} \\ = A - \frac{20}{19} \times \frac{K}{W_1}\log_e 20 \\ = A - \frac{3\cdot 16K}{W_1} \end{aligned}$$

The average error is then the error at $\frac{1}{3\cdot 16}$th of full load. Similarly, the average error of the meter between one-tenth and full load is the error at $0\cdot 41 W_1$.

The average of the customary four tests at one-twentieth, one-quarter, one-half and full load is

$$\tfrac{1}{4}\left\{4A + \frac{K}{W_1}(20 + 4 + 2 + 1)\right\} = A + K \times \frac{27}{4} \times W_1$$

or the error at $\frac{4}{27}$ths or $0\cdot 15$ of full load.

The weighted mean of the four tests is, ignoring the term A which obviously does not affect the final result:

$$\frac{K}{W_1}\left(\frac{20 + 4\times 5 + 2\times 10 + 20}{1 + 5 + 10 + 20}\right) = \frac{K}{0\cdot 45 W_1}$$

or the error at $0\cdot 45$ of full load.

The above calculations demonstrate very clearly the importance of attaching a definite meaning to the expression "mean error," and also show that the error of registration over an extended period is the error of the average load over this period for motors of the types considered. This result applies closely to meters in which solid friction is the only source of error. The average load, in the case of large power consumers, can be found with fair accuracy by dividing the recorded energy by the total hours during which the works have been running. It is obvious that the period during which there is no current passing through the meter is not to be taken into account in calculating the average load.

Meters of the direct-current mercury motor and alternating-current induction types suffer from disturbing causes in addition to solid friction. The mercury-motor meter is affected by fluid friction, the effect of which is approximately proportional to the square of the speed. The induction-motor meter suffers in accuracy by the increase of the braking torque with increased load, owing to the braking effect of the driving fluxes. The increase of the braking torque will be due to the current flux and, on the assumption that this increase in flux is small compared with the main braking flux, the increase in the braking torque will be proportional to the product of the speed and the current. Since the speed in meters of both types is nearly proportional to the load, the disturbing effect in each of these cases is approximately proportional to the square of the current. Assuming a nearly constant power factor for the alternating-current meter, the effects under consideration will in each case be proportional to the square of the load, and the fractional error proportional to the load. According to this argument, the fractional error of both the mercury-motor meter and the induction meter can be represented by an expression of the form $\eta = A - K/W - mW$, the first two terms having the same significance as in the case of the commutator-type meter already considered, the third representing the increase in braking torque due either to fluid friction or to increase in braking flux. This expression gives an error curve of a shape sufficiently similar to those empirically determined to enable approximate calculations regarding the error of registration over extended periods to be made.

$$\text{The actual error} = AW - K - mW^2$$

The error of registration

$$= A\int_0^T W dt - K\int_0^T dt - m\int_0^T W^2 dt$$

The fractional error of registration

$$= A - \frac{K}{W_a} - \frac{m}{E}\int_0^T W^2 dt$$

$$= A - \frac{K}{W_a} - m\frac{W_2^2}{W_a^2}W_a$$

where W_2 is the R.M.S. value of W for the period O to T.

If g be the form factor of the curve representing

the variation of W and t, the fractional error can be put as

$$A - \frac{K}{W_a} - mg^2 W_a$$

From this expression it appears that the error of registration is greater than the error as determined by testing at the average load, the portion of the error due to the increase of retarding torque at the higher loads being multiplied by a number equal to the square of the form factor of the load curve.

The effect of form factor on the error of registration of a mercury meter can best be appreciated by a

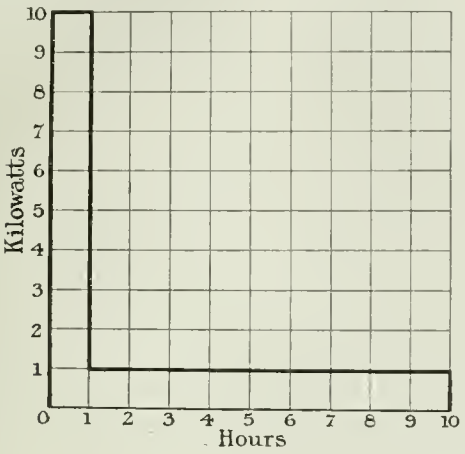

FIG. 1.

numerical example based on a simple load curve as shown in Fig. 1.

The curve gives the following data:

Average load .. .. 1 900 watts
R.M.S. load .. .. 3 300 watts
Form factor (g) = 1·73, g^2 = 3·0 approximately

If the percentage error of the meter be given by the expression

$$-\left(\frac{5\,000}{W} + \frac{W}{5\,000}\right)$$

the error curve is that shown in Fig. 2.

Energy during first hour	= 10 units.
Percentage error	= $2\frac{1}{2}$.
Error of registration	= 0·25 units.
Energy during succeeding 9 hours	= 9 units.
Percentage error	= 5·2.
Error of registration	= 0·468.
Total energy metered	= 19 units.
Total error	= 0·718.
Overall percentage error	= 3·78.

Percentage error at average load $= \frac{5}{1\cdot9} + \frac{1\cdot9}{5} = 3\cdot01.$

The meter therefore has an error of registration for the period considered 0·8 per cent slower than the error at the average load.

The same result may be deduced from the expression for the error of registration deduced above

$$\text{Percentage error} = -\left(\frac{5\,000}{W_a} + g^2\frac{W_a}{5\,000}\right) = 3\cdot78$$

It is easy to show that the average of the errors at one-twentieth, one-quarter, one-half and full load as determined by testing is given by the expression

$$-\left(A + K\frac{6\cdot7}{W_1} + m\frac{W_1}{2\cdot2}\right)$$

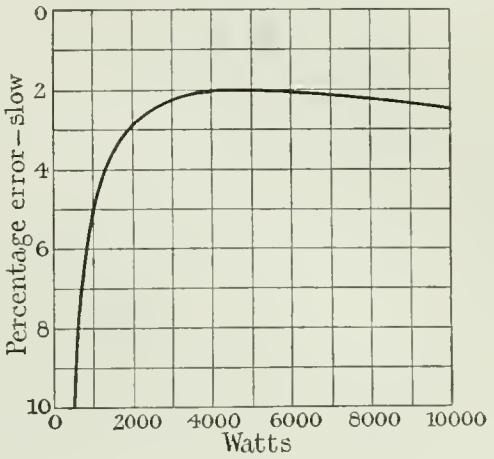

FIG. 2.

While the true average error of the meter between one-twentieth and full load is

$$-\left(A + K\frac{3\cdot16}{W_1} + m\frac{W}{1\cdot9}\right)$$

the loads to which these two averages correspond obviously depend upon the ratio of K and m.

In the case of alternating-current meters, the error will vary not only with the load but also with the power factor, and it is interesting to calculate the influence of the variation of power factor upon the total error of integration over an extended period. In an induction meter the torque is given very nearly by

$$\begin{aligned} F &= aIV \sin(\alpha - \phi) \\ &= aIV(\sin\alpha\cos\phi - \cos\alpha\sin\phi) \\ &= a(W\sin\alpha - IV\sin\phi\cos\alpha) \end{aligned}$$

If the angle α be 90°, the meter will revolve at the same speed for a given load, whatever be the power factor, within the limits of its rated capacity. If the quadrature adjustment be faulty, the difference in torque for a given load W at unity power factor, and $\cos\phi$ will be

$$aIV \sin\phi\cos\alpha$$

This last expression can be taken as being the error due to faulty quadrature adjustment, and the fractional error will be

$$\frac{aIV\sin\phi\cos\alpha}{aIV\cos\phi\sin\alpha} = \tan\phi\cot\alpha$$

The error of registration over a period will be

$$\cot \alpha \int_0^T IV \sin \phi dt$$

The quantity $\int_0^T N \sin \phi dt$ is the time integral of the wattless component of the apparent power, and, denoting this quantity by X, the fractional error of registration is equal to

$$\cot \alpha \frac{X}{E} = \cot \alpha \tan \phi_a$$

where $\tan \phi_a$ is the ratio of the readings of the wattless component and the energy meter.

The power factor error is therefore equal to that at a power factor $\cos \phi_a = E/\sqrt{(E^2 + X^2)}$.

The above investigation leads to the conclusion that, with meters in which solid friction is the only source of error, the error of registration over a period is equal to the error as determined by testing at the average load over this period. In the case of meters where there is a further source of error approximately proportional to the square of the load, the error of registration will generally be greater than that determined by testing at the average load, a greater error in this case meaning that the meter will have registered slower than the test at the average load would indicate. The difference between the error of registration in this case and the error at the average load depends upon the manner of the variation of the load, and in certain circumstances the difference may be appreciable. The influence of variable power factor is given by the result that the error due to this cause is the same as the error determined by testing at the average power factor, deduced from the readings of the energy and wattless component meters.

Since the number of meters in which solid friction is the only source of error is relatively a fairly small fraction of the total number of meters in use, at all events in this country, the definition of the "mean inaccuracy" as being the inaccuracy at the average load is not generally strictly legitimate. The departure from exactitude is sufficiently small to justify the use of so simple and easily applied a definition, which accordingly might well be incorporated into agreements for power supply in the form that the "mean inaccuracy of the meter shall be taken to be the inaccuracy as determined by testing at the average load at the average power factor for the period in dispute, the former being calculated from the energy consumption and the number of hours during which power has been taken from the supply mains, the latter being based upon the readings of the energy and wattless component meters." This definition tacitly makes use of the indication of the meter whose accuracy is in dispute, for the determination of the average load and power factor. It is, however, clear that since the error will usually be a relatively small percentage, the corresponding error in the calculation of the average load and average power factor will have an effect of negligible magnitude on the result determined by testing.

DISCUSSION ON "THE APPLICATION OF THE ELECTRIC LOCOMOTIVE TO MAIN-LINE TRACTION ON RAILWAYS." *

WESTERN CENTRE, AT BRISTOL, 7 FEBRUARY, 1921.

Professor F. Bacon : I should like to know why a low centre of gravity leads to severe wear on curves. Is there not a possibility of substantial saving in the matter of rail wear on straight portions of the track, due to the reduced slip and improved adhesion resulting from the uniform character of the driving torque and the absence of only partially balanced reciprocating masses ?

Mr. W. Nairn : The author has much to say for the life and performance of the direct-current traction motor, and I should like to know at what pressure the motors referred to were operating, whether 600, 1 200, or 1 500 volts. Are commutator micas to-day undercut, and, if so, are the gaps filled in with compound or are they left open ? Dealing with the question of regenerative braking, the author mentions the special case of the Chicago, Milwaukee and St. Paul railway. This line, however, is a very special case, as on one of the electrified portions the train starts on the top of the Rocky Mountains and runs downhill for over 200 miles towards the Pacific. One can understand regenerative braking being a success in such a case, but I do not think the extra expense and complication would be justified in this country. The extra equipment on the St. Paul locomotives is a serious item. When regenerating, the motor fields are excited from separate direct-current generators driven from the locomotive axle, and two of these generators of 50 kW capacity each were installed on each locomotive. The fields of these exciters are further separately excited from either a battery or a motor-generator, and are controlled by a motor-driven rheostat. I feel that such complications would not be justified in this country. With regard to the question of axle failures, I agree with the author that the proper method of fixing the gear wheel would appear to be to bolt it direct to the road wheel, and I would further suggest the fixing of gears to both road wheels and the replacement of the 250-h.p. motor with two 125-h.p. motors connected permanently in series, one armature being geared to each road wheel. This arrangement would give only 750 volts across each armature instead of 1 500 volts, and I understand that the 750-volt motor is not so liable to flash over as the 1 500-volt motor.

Mr. I. A. D. Pedler : How does the author account for the 70 per cent load factor ? Is it because passenger trains represent a very small percentage of the total train load ?

Mr. A. M. Willis : Last year I attended a demonstration of shunting by the somewhat unsatisfactory and perhaps inefficient battery locomotive. I was assured by the engineer that the saving was more than 50 per cent, as it was possible to arrange the trains and place them on a special piece of track set out for the purpose, so that the main-line engine did not enter the actual goods siding. I suggest that this might be a solution of the difficulty in connection with the third rail, or overhead network on sidings. It is difficult to see why breakdowns should be expected more in sidings than on the main line, but where overhead wires are used there is an unfortunate tendency to replace the soft aluminium collecting strip by materials of a much harder nature, such as copper or even steel. This must lead to excessive wear on the trolley wire and eventual breakages of the overhead line. With reference to the author's comments in connection with pantograph current-collectors, in most systems pantographs are controlled by compressed air ; the pantograph is normally down and is raised by the driver, who allows the air to flow from the air chamber to the pantograph cylinder. It should not be difficult for him to carry out this operation when he sees a tunnel or obstruction ahead. I should like to have the author's opinion as to the possibility of trouble in connection with the motors, due to the constant making and breaking of current on the third-rail system, as it appears that in this country, where large towns are very close, the number of crossings, points, junctions, etc., would necessitate the frequent sectionalization of the third rail.

Mr. T. Hood : I should be glad if the author would explain why the use of alternating-current single-phase traction motors has been discarded. The advantages of alternating current for long transmission has been, on the Continent, the factor determining its use for railway electrification, and motors of the single-phase type, designed by Messrs. Winter and Eichberg and described some time ago in the *Electrotechnische Zeitschrift*, appear to have been satisfactory. It is true that when the electrification of the Underground Railways was projected the advantages of the alternating and direct-current systems formed the subject of an inquiry. Finally, the alternating-current system advocated by Messrs. Ganz, and the direct-current system advocated by the Americans, were left in the field and the latter system was adopted. The single-phase system was tried by the North-Eastern Railway and has apparently been rejected, and I should be glad if the author would state the reason. In view of the difficulties which the author has had in connection with rotary converters, I should be glad to know if he has had any experience with the static mercury-type rectifiers now being sold by a Swiss firm.

Mr. H. I. Rogers : As a manufacturer of trolley

* Paper by Lieut.-Colonel H. E. O'Brien (see vol. 53, p. 858).

standards, more particularly for tramways, I am naturally in favour of overhead transmission rather than the third rail, but there are, probably, advantages in both systems. The point mentioned about the automatic raising and lowering of the pantograph bow at certain points might be overcome. In tramway work, various devices have been manufactured for raising and lowering the arm, but the speed and weight of trains make the question much more difficult on railways. As regards the question of shunting, I notice that the author thinks there would be no reduction in the number of electric locomotives as compared with the number of steam locomotives. I have always thought it was one of the great advantages of electric traction over steam traction that shunting would be very much simplified, and the much greater mileage of the electric locomotive before overhauling is necessary must surely in itself mean a saving in the number of locomotives involved in shunting.

Mr. S. B. Haslam: In 1904 and 1905, in conjunction with a leading locomotive engineer at that time, the late Mr. T. Hurry Riches, I carried out exhaustive inquiries into the question of electric traction on railways, which culminated in a paper read before the Institution of Mechanical Engineers at their Summer Meeting at Cardiff, in August 1906. I am interested to know that the difficulties of to-day are practically the same as 17 years ago, and are "railway operating" difficulties rather than "electrical." Some information as to comparative costs in those days may be of interest. The route considered for electrification was a short length of about six miles on the Taff Vale Railway through a very sparsely populated area. All possible methods of running the traffic were considered, including electric power supplied by the Company's generating station, or, alternatively, current from the South Wales Electrical Power Distribution Company. The table of comparative costs then arrived at may be of interest, especially when compared with present-day figures. The discussion on that paper brought out some very interesting facts and figures, not the least noteworthy being the remarks of Sir John Aspinall on the subject of regenerative control. Sir John said: "I believe one of the most important things which electricians could supply would be some form of perfect regenerative control. I am quite aware there are in existence at the present time several forms of regenerative control, but they are not as yet adapted to railway purposes. There is a field for somebody to work on, which will mean the saving of a great deal of money when an effective system is produced." Dealing with the paper now under discussion, there are so many points of interest that it is difficult to choose the ones to touch on. There can be no question of the advantage of electric over steam traction as regards acceleration, cleanliness and efficiency, but some of the disadvantages appear almost insuperable. There is

Details	Cost of Original System. Small Engine and Train	Secondary Battery		Third-rail System from Supply Company	Steam Cars
		Own Power Supply	From Supply Company at 1d. per Unit		
(1) Single car..	£ 1/3 engine per day 733 Carriages 1 775 £2 508	£ Car complete .. 4 000 Generating station . 1 150 £5 150	£ Car .. 4 000 Transformers 100 £4 100	£ Car .. 1 350 Preparing 6 miles of rails 7 136 £8 486	£ Car 2 100
Interest at 4 per cent ..	£ 100	£ 206	£ 164	£ 339	£ 84
Maintenance, wages, etc.	975	716	542	680	251
Total ..	£1 075	£922	£706	£1 019	£335
Cost per mile..	23·45d.	20·02d.	15·4d.	22·11d.	7·74d
(2) 2 Cars— Cost per mile	—	19·6d.	15·3d.	16·3d.	—
(3) 3 Cars— Cost per mile	—	19·4d.	15·2d.	14·1d.	—

very little doubt that the third-rail system is the only method for long distances at high speeds, but the third rail is a very great obstruction on the track, and presents enormous difficulties at sidings and stations where there are many lines, while a change-over system at these points is going to present difficulties from an operating point of view. To start with, the load gauge in this country is low enough now, and if arrangements have to be made for the locomotives to carry

collecting bows I am afraid it will mean a reduction in the already restricted height available. Furthermore, the third rail is bound to be a source of danger, not so much in towns as in the country, where there is very little to stop children and animals from straying over the line. Snow and rain also present difficulties. I should be glad to hear how the goods traffic was tackled in the case of the Newport and Shildon line. I understand that the main-line traffic is worked by an electric locomotive, but it would be interesting to know how the wagons are put on at the main line, whether by steam for the electric locomotive to pick up, or whether the electric locomotive goes into the siding and collects them as in present-day practice. On page 861 the author refers to the torsional stress on the axles, and gives the life of an axle as about 170 000 miles. This is, of course, considerably less than the life of the axle of a steam locomotive, and it occurs to me that this may be due to the method of construction with the gear pressed and keyed on to the axle, and it may account for the standard American practice of converting rotary motion to reciprocating motion before converting it to rotary motion again on driving wheels. I am inclined to think that the author has underestimated the mileage of the steam locomotive before heavy repairs are necessary, and I should put this very much nearer to 100 000 miles than the figure he has quoted.

Mr. A. C. McWhirter : The author appears to advocate the use of geared motors for all types of electric locomotives. In America the gearless motor is much in use for passenger traffic, and reports show an increase in efficiency over the geared motor from 5 per cent at 40 m.p.h. to 20 per cent at 65 m.p.h., The extreme simplicity and low maintenance cost of this type of motor (which is bi-polar), which gives a maximum number of commutator segments and a large space between brushes of opposite polarity, is a point worth serious consideration when working at high voltages. Another point in their favour is that, due to the pole-faces being flat, the armature can be lowered and readily removed without disturbing the fields, and will, in consequence, save considerable labour as compared with the geared motor. The author suggests a pressure of 1 500 volts, whereas in the United States the tendency is to aim at much higher pressures, 3 000 volts being now in general use. This is no doubt due to the long distance of transmission, which would not be the case in this country. I imagine that third-rail transmission will give considerable trouble in the open country during winter, due to frost and snow at such high voltages, and there is also a possibility of serious danger from shock. I think it would be well if the railways of this country adopted a standard system of third rail and equipment. In France, I believe, a Commission of engineering specialists has been appointed to advise the Government regarding the electrification of several of the large railways, and I suggest that the railway companies in this country should adopt a similar course.

Mr. W. A. Chamen : I am very interested in what the author says about vertical shafts and worm gearing. Personally, I have always looked upon worm gearing as being unsatisfactory for heavy continuous loads. The problem of tunnels where an overhead conductor has to be used, while the rest of the line is worked by means of a third rail, certainly seems to present a difficulty. Some years ago, on an electric railway on Southend pier, where the conductor consisted of a flat copper strip stretched on insulators between the rails, the problem of lifting the contact shoes over the running rails at points and crossings at the ends of the line was solved by fixing the contact shoes at the ends of levers keyed on to rocking shafts running transversely under the car, and fixed only a few inches above the rail level. At each outer end of these rocking shafts another lever was keyed on with a roller at the end of it. A kind of switchback path was fitted outside the rails on either side, with which these rollers engaged so as to lift the levers, turn the rocking shaft, and lift the contact shoes, dropping them again on to the copper strip after passing the obstruction. This arrangement worked very well for some years, but the speed was only about 2 m.p.h. through the crossings, and I doubt whether it would have worked satisfactorily at 60 m.p.h. In regard to the financial side of the electrification of railways, I have recently constructed an incline embankment at the power station near Treforest, of incombustible material which was sent with the coal. Rails are now about to be laid, and I think it would be a good opportunity to show an electric locomotive in operation beside the Taff Vale Railway main line. The cost, however, would be very high, and I am afraid that the work will have to be done by means of a mining-type haulage gear, which, after all, would do a great deal of work of this kind very satisfactorily and economically. The author says that alternating current at 25 periods per second is more satisfactory for large rotary converters for railway work than at 50 periods per second—a point which should be borne in mind to-day when so much is being done to kill the former periodicity. I believe the Central London Railway was originally worked by means of locomotives with armatures built direct upon the axles, and that the noise and vibration resulting caused the abandonment of these locomotives in favour of geared motors fixed on the cars themselves. Presumably the trouble was due to there being such heavy unsprung weights direct upon the axles. I remember a proposal to carry the armatures upon sleeves having a bore very much larger than the axle, but running concentrically with it, these sleeves running in bearings and having a system of springs, so that, while the armature was practically direct-coupled to the axle (that is to say, no gearing was used), the dead weight of the armature did not come directly on the axle itself. I should like to know whether this arrangement has been used to any extent. It is surely desirable to have overhead conductors at sidings. The author mentions the difficulty in case of derailment of not being able to use a jib crane without taking the over head conductors away, but would it not be possible to devise some other means of dealing with such breakdowns ? Presumably some other method has to be used in tunnels.

Mr. T. Morgans : I notice that, in Figs. 3 to 7, the

tractive resistance of a locomotive first decreases rapidly and then begins to rise as the speed increases. Is the decrease due to a diminution in the coefficient of friction ?

Mr. C. F. Waddington : One reason for the adoption of direct current is that batteries can be used for reducing the peak loads.

Lieut.-Colonel H. E. O'Brien (*in reply*) : Professor Bacon is correct in assuming that the wear on curves has nothing to do with the low centre of gravity of electric motor cars : the sole advantage of a high centre of gravity is the reduced effect of the swaying of the whole vehicle upon the rail. There is probably a reduction in rail wear due to more uniform torque, but it is hardly likely to be measurable. Regenerative braking depends upon the profile of the line for its value, which should almost always be investigated. Each case, however, would have to stand on its merits. The motors on the Lancashire and Yorkshire Railway operate perfectly on 1 200 volts, and a car has operated for some years on the Bury and Holcombe Brook line at 1 750 volts without any trouble, so that the use of motors permanently in series is unnecessary. In this connection it is advisable to shut off current while coasting over long gaps in the third rail, but even on the most complicated junctions the third rail can easily be so arranged as to make these of rare occurrence with multiple-unit trains. Trouble arises when locomotives with a comparatively short shoe-span are used : on a gradient with a heavy goods train even a short gap might result in the train pulling up on a dead section. This leads to the necessity for the use of an overhead trolley. In the atmosphere of towns like Manchester the life of the overhead work when constructed of iron or steel is excessively short. This factor, and also the large number of tunnels and the difficulty of finding sites for the trolley standards, combined with the lower efficiency and higher maintenance costs of the single-phase motor, have decided the Lancashire and Yorkshire Railway in their policy of adopting a pressure of 1 200 volts direct current.

The amount of shunting necessary will be unaltered whatever the system of traction, as this is in general a process of shuffling wagons. The problem of providing the power for shunting on an electrified line has not yet been solved ; the use of accumulator locomotives, rather than the erection of trolley wires over a network of tracks, will probably provide the solution. The accumulator locomotive can be made to give a high tractive effort with a wide range of speed. Where electric locomotives have to go into colliery sidings, either an overhead trolley will have to be used or the colliery will have to provide its own locomotive.

The motor with the armature built direct on the axle or on a sleeve has had a considerable measure of success, but it is doubtful if the complication is really less with a sleeve than with gearing.

It should again be emphasized that the two main problems in connection with main-line electrification are : First, the financial problem, which resolves itself into : (*a*) the arranging of a continuous user for locomotives and track equipment, and (*b*) the reduction in the capital cost of the locomotive by simplification and robustness of design and by standardization. Secondly, the provision of a cheap and abundant supply of electricity. In this connection it must be borne in mind that the cost of current is not a determining factor in coming to a decision as to whether the railway should be electrified, only a small proportion of the total cost of electrification being expended on the power station and transmission lines. The load factor obtainable from a railway electrification is likely to be so high, owing to the load due to the goods trains during the night, that the load thereby provided will be of the utmost value to power companies.

REPORT OF THE COUNCIL FOR PRESENTATION AT THE ANNUAL GENERAL MEETING OF 31 MAY, 1921.

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REPORT.

The Council, at the Forty-ninth Annual General Meeting of the Institution of Electrical Engineers, present to the members their Report for the year 1920–21.

The Council are glad of the opportunity of stating that much evidence has come before them of the value attached to the work of the Institution both by the membership and by other societies and bodies with whom the Institution is in touch.

The Council wish also to refer to the generous devotion by members of the Institution of their time and expert knowledge to the work of its various Committees.

(1) Royal Charter.

Active steps are being taken by the Council to present a petition to the Privy Council with a view to a Royal Charter being granted to the Institution. This action on the part of the Council will require the approval of the Institution, for which purpose a Special General Meeting will be convened at an early date.

(2) Membership of the Institution.

The changes in the membership since the 1st April, 1920, are shown in a table which will be found in Appendix A.

The continual growth of membership is emphasized by comparison with the corresponding totals for the last few years, which were as follows:—

Year	Membership	Increase or Decrease
1912	6 537	+ 194
1913	7 084	+ 547
1914	7 045	— 39
1915	6 811	— 234
1916	6 676	— 135
1917	6 613	— 63
1918	6 667	+ 54
1919	7 023	+ 356
1920	8 146	+ 1 123
1921	9 449	+ 1 303

The Council have again to express their gratification at the large increase in membership, which has continued, while the same high standard of qualifications for admission has been maintained. This increase, as was stated in last year's Report, is largely to be attributed to the activities of the Territorial Centres and Sub-Centres, and also at the fact that nearly 200 Post Office Electrical Engineers joined the Institution during the session.

(3) Associate Membership Examination.

The first Examination for Associate Membership to be held since the revival of the Examination (which

had been suspended owing to the War) will take place at the end of April (in May in the case of places abroad) at London, Chelmsford, Colchester, Manchester, Newcastle-upon-Tyne, Stoke-on-Trent, Swansea, and also in the Federated Malay States and New Zealand.

Five candidates submitted theses during the year in lieu of the examination.

(4) Honorary Member.

The Council have pleasure in recording that, as was announced at the Ordinary Meeting of the 16th December last, they had elected Professor André Blondel, of Paris, to be an Honorary Member of the Institution.

(5) Honours and Distinctions Conferred on Members.

Since the last Annual Report, the following honours have been conferred on members :—

Knighthood.

Beattie, J. C., D.Sc. (Associate Member).
*Bright, Charles, F.R.S.E. (Member).
Dawson, Philip (Member).
Henderson, James B., D.Sc. (Member).
Noble, William (Member).

C.I.E.

Pitkeathly, Lieut.-Col. J. S., C.V.O., C.B.E., D.S.O., R.E. (Associate Member).

C.B.E.

Archer, Sq.-Leader J. O., O.B.E., R.A.F. (Associate Member).

M.V.O.

Massey, W. H. (Member).

O.B.E.

Day, Major B. J., R.E. (Member).
Wilson, J. (Associate Member).

M.B.E.

Orloff, Captain E., R.E. (Associate Member).
Young, Major J., R.E. (Member).

M.C

Denton, Lieut. P. H., R.E. (T) (Student).

Médaille du Roi Albert (Belgium).

Olver, Major G. T. W., R.E. (Member).

Ordre de la Couronne, Officier (Belgium).

Norman, The Right Hon. Sir Henry, Bart., P.C. (Associate).

Ordre de la Couronne, Chevalier (Belgium).

Evans, Lieut.-Col. Ll., C.M.G., D.S.O., R.E. (Member).

Order of the Redeemer (Greece).

Hill, E. L. B. (Member).

* Conferred June, 1919; mention inadvertently omitted from last Annual Report.

Mentioned in Despatches.

Archer, Sq.-Leader J. O., C.B.E., R.A.F. (Associate Member).
Jackson, Captain H. Y. V., M.C., R.E. (Student).

(6) Deaths.

The Council have to deplore the death of 45 members of the Institution during the year.

Honorary Member.

Righi, Professor A.

Members.

Armitage, A.
Coyle, D.
Creak, Capt. E. W., C.B., F.R.S., R.N.
Edmundson, J. E.
Evans, M.
Gatehouse, T. E
Hammond, C. P.
Hartnell, W.
Moulton, The Right Hon. Lord, G.B.E., K.C.B., F.R.S.
Muirhead, Dr. A., F.R.S.
Nicholson, J. S., M.B.E.
Parkinson, C. F.
Perry, Prof. J., M.E., LL.D., D.Sc., F.R.S.
Rankin, R.
Sedgwick, Prof. F. W.

Associate Members.

Black, G. L.
Davidson, G. F.
Hales, G. E. B.
Hoey, M. M. McC.
Joly, H. L.
MacLean, D.
McDougald, L. A.
Martin, L. C.
Peers, G. R.
Robertson, H., M.C.
Woodward, W.

Graduate.

Lacey, F. P. S.

Students.

Bessant, S. J.
Clarke, R. St. J.
Coleman, D. T.
Japp, R. F.
Jones, A. E. P.
Kent, B. R.
Orr, W. J.
Treherne, C.

Associates.

Botham, A.
Butcher, S.
Huddleston, P.
Ince, F.
McDougall, P. W.
Noble, G. St.
Reynolds, F. W.
Trench, Col The Hon. W. le Poer, C.V.O., late R.E.
Webb-Watts, T.

The Council feel that particular mention should be made of the loss of such distinguished members as Professor John Perry, M.E., LL.D., D.Sc., F.R.S. (President of the Institution, 1900–1901), Professor A. Righi (elected an Honorary Member in 1917), Mr. Wilson Hartnell (Chairman of the Yorkshire Centre, 1912–1913), the Right Hon. Lord Moulton, G.B.E., K.C.B., F.R.S. (Member of Council, 1874–1883), Dr. A. Muirhead, F.R.S. (Member of Council, 1874–1875), and Mr. T. E. Gatehouse (Editor of the *Electrical Review*).

(7) War Memorial.

The question of erecting a suitable memorial to the members who fell in the War could not be dealt with during the past year owing to the absence of the Institution from its building, but will be taken up by the War Memorial Committee as soon as it is possible to select an appropriate position in the building. In the meantime, the task of collecting data for the biographical notices to appear in the War Memorial Book has been in progress and the Council desire to express their warm thanks to the many engineering firms, institutes, colleges and schools which have so readily supplied the particulars of the careers of the fallen members who were connected with them. The work has taken longer than was first anticipated and, although it is not yet possible to give an approximate date for the publication of the book, as much progress as possible in that direction has been made by preparing the blocks for the portraits which will be included.

(8) The Institution Building.

The building was vacated by the Authorities in January, but it was not possible for the work of redecoration to be put in hand until the middle of April. The Council have every hope that the return of the Institution to its own premises will be effected about the middle of May.

(9) Members' Subscriptions.

As was foreshadowed in the last Annual Report, it became necessary towards the end of last year to put proposals before the members for an increase of subscriptions, Special General Meetings for the purpose being accordingly held on the 2nd and 17th December last, when the Council's proposals were approved. No change has been made in the subscriptions of Students. The increases made were designed to be as low as would be compatible with the financial requirements of the Institution, and the result in this respect has so far been adequate. At the time this matter was under discussion the Council received great help and encouragement by suggestions for improvements in the work of the Institution.

(10) Meetings and Papers.

A statement of the total number of meetings and conferences held during the past 12 months and of papers read, lectures, etc., will be found in Appendices B and C to this Report.

(11) Kelvin Lecture.

The Twelfth Kelvin Lecture was delivered by Professor Sir William Bragg, K.B.E., F.R.S., on the 13th January, the subject being "Electrons." The lecturer dealt mainly with the part played by the electron in radiation processes and in atomic structure.

(12) Ordinary Meetings.

The Ordinary Meetings in London have continued to be held at the Institution of Civil Engineers, and the interest of the members in the papers read and the discussions has been shown by the large attendances at most of the Meetings.

(13) Territorial Centres and Sub-Centres.

The attendances reported at the Meetings held at the Territorial Centres and Sub-Centres afford similar evidence of the interest taken by members outside London in the work of the Institution.

During the Session the President paid visits to the Centres at Birmingham, Leeds, Manchester, and Newcastle, and to the Sub-Centres at Liverpool, Middlesbrough, Loughborough and Sheffield. He proposes also to visit the Western Centre at Bristol in May.

(14) Wireless Sectional Meetings.

The Wireless Section of the Institution has held three meetings, the first being occupied by the Address of the Chairman, Dr. W. H. Eccles. Three papers were read at the other two meetings. The Committee of the Section for this Session was elected in June, 1920, and consists of:—

Dr. W. H. Eccles (*Chairman*).

The President.

Major E. A. Barker, M.C.	Sir Oliver Lodge, D.Sc.
Sir Charles Bright, F.R.S.E.	Prof. E. W. Marchant, D.Sc.
Mr. R. C. Clinker.	Captain H. J. Round.
Prof. C. L. Fortescue.	Mr. A. A. C. Swinton, F.R.S.
Mr. A. Gray.	Mr. J. E. Taylor.
Prof. G. W. O. Howe, D.Sc.	Mr. L. B. Turner.
Admiral Sir H. B. Jackson, G.C.B.	Mr. C. H. Wordingham, C.B.E.

And	*Representing*
Major J. Erskine-Murray, D.Sc. ..	.. Air Ministry.
Mr. E. H. Shaughnessy, O.B.E. ..	.. Post Office.
Lt.-Col. H. Clementi Smith, D.S.O.	.. War Office.
Capt. J. K. Im Thurn, R.N., C.B.E.	.. Admiralty.

(15) Informal Meetings.

Nine meetings have been held, at which various subjects have been discussed. The attendance has steadily increased and the quality of the discussions has still further improved.

In addition to the above meetings a Smoking Concert was held at Anderton's Hotel on 28th February, 1921, at which about 130 members were present.

On the suggestion of the President a special meeting was also held at the Institution of Civil Engineers, on 24th January, 1921, the subject for discussion being "In What Way can the Institution be made More Useful to its members?" About 150 were present and many useful suggestions were put forward for the consideration of the Council.

More subjects have been offered for discussion than could be allotted to available dates, and several very promising subjects have had to be postponed to next Session.

The Informal Meetings Committee has met eight times during the year and consists of:—Messrs. F. Pooley (Chairman), J. F. Avila, W. E. Bradshaw, A. B. Eason, A. F. Harmer, G. C. Law, E. W. Moss, W. E. Warrilow and W. L. Wreford.

The Committee suffered a serious loss this year by the death of Mr. R. Rankin, its excellent first Chairman.

His regular contributions to the discussions are very much missed by those attending the Informal Meetings.

(16) Students.

The number of Students on the Register of the Institution has reached a record total of over 1 900. A very full programme of meetings, visits to works, and social functions was carried out during the Session by the six Students' Sections now in existence, viz. in London, Birmingham, Glasgow, Liverpool, Manchester, and Newcastle.

Addresses to the London Students' Section were given by the President (Mr. Ll. B. Atkinson) and Mr. C. C. Paterson. In addition, the President gave an Address to the Students' Section at Birmingham, which was formed this Session, and Mr. Paterson also gave an Address to the Students' Section at Newcastle.

(17) Premiums.

The premiums awarded by the Council for papers will be announced at the Annual General Meeting.

(18) Scholarships.

The following Scholarships have been awarded by the Council:—

David Hughes Scholarships.

(Value £50; tenable for one year.)

F. H. Cooper (Finsbury Technical College).
C. Dannatt (Armstrong College, Newcastle-upon-Tyne).
E. H. Lakey [City & Guilds (Engineering) College].
R. C. Titman (Sheffield University).

Salomons Scholarships.

(Value £50; tenable for one year.)

E. T. Donovan (Birmingham University).
J. Fowler (Leeds University).
H. S. Petch (East London College).
L. Turton (Liverpool University).

Paul Scholarship.

(Value £25; tenable for one year.)

A. G. Pearce (Finsbury Technical College).

(19) War Thanksgiving Education and Research Fund (No. 1).

The benefaction of £2 000 5 per cent War Loan referred to in the last Annual Report has now been constituted as a Trust Fund under the above title, the trustees being Sir James Devonshire, K.B.E., Mr. Roger T. Smith, and Sir John Snell.

A grant of £100 for educational purposes has been made this year by the Council, under the provisions of the Trust Deed, to—

Frederick Bernard de Steiger (Finsbury Technical College), an ex-member of the Australian Imperial Force.

(20) Coopers' Hill College War Memorial.

The Council have been asked by the Coopers' Hill War Memorial Fund Committee and have undertaken to award, in rotation with other bodies, a sum of £25 in prizes to young professional men in commemoration of the Royal Indian Engineering College, Coopers' Hill, and of its members who fell during the War.

The Council of the Institution of Civil Engineers are trustees of the Fund, the interest on which is expected to yield about £50 a year. Half of this will be awarded annually by the Institution of Civil Engineers. The other half is to be awarded by the Institution of Electrical Engineers in rotation with the Military School of Engineering, Chatham, and the School of Forestry, Oxford.

The prize (£25) will take the form of—

(*a*) A bronze medal,
(*b*) A parchment certificate, and
(*c*) A money prize.

In the case of the Institution of Electrical Engineers, the recipients will be Students, Associate Members, or Members of the Institution of less than 30 years of age at the date of submission of the paper for which the award is made. The award will be for the best paper (to be selected by the Council of the Institution) on any professional subject submitted during the period dating from the last award.

The prize may be awarded in addition to any other to which the paper would entitle the recipient.

(21) Kelvin Medal.

The first triennial award of the Kelvin Medal, on the Awarding Committee for which the Institution is represented, was made in January, 1921, to Dr. William Cawthorne Unwin, F.R.S.

(22) Conversazione.

The Annual Conversazione was held at the Natural History Museum, South Kensington, London, on the 1st July, 1920. The function was attended by over 1 300 members and guests.

(23) Annual Dinner.

The Annual Dinner was held at the Hotel Cecil, London, on the 3rd March, 1921. The number of members and guests present reached the record total of 423.

(24) Summer Meetings.

A Summer Meeting of the Institution was held at the Western Centre in July of last year and was attended by about 200 members and ladies. The visit lasted four days and the towns visited were Bristol, Cardiff, Newport (Mon.), and Swansea, where the visitors enjoyed the most generous hospitality of the local authorities and others. Great credit for the excellent arrangements is due to the then Chairman (Mr. Arthur Ellis), the Committee and Honorary Secretaries (Mr. C. T. Allan and Mr. A. J. Ostler) of the Centre.

Arrangements are being made for a similar Meeting to be held at the Scottish Centre in June next. A very satisfactory number of members have expressed their intention of taking part in that Meeting.

(25) Return Visit of Dutch Engineers.

It will be within the recollection of members that, at the invitation of the Association of Managers of Dutch Electricity Undertakings, a small deputation from the Institution visited Holland in September, 1919, and that a full Report of the visit was published in Volume 57 of the *Journal* (*Institution Notes*, No. 15, p. 34).

Arrangements are well in hand for a return visit of Dutch electrical engineers to this country to take place in May.

(26) Library.

The Council again desire to express their thanks to the Council of the Institution of Civil Engineers for their kindness and courtesy in placing the Library and Reading Rooms of that Institution at the disposal of members for the long period during which the Institution Library has been inaccessible.

This arrangement will be continued until the Institution Library has been redecorated and the books returned from storage and replaced on the shelves. It is hoped that this work will be completed by the summer.

The Council propose as an experiment when the Library again becomes available, to keep it open during the evenings, and if it is found that the use so made of the Library justifies the expense, the arrangement will be continued.

During the year, 163 books and pamphlets have been presented to the Reference Library by members and others.

It is with pleasure that the Council have to report the growing increase in the circulation of the Lending Library. During the year, 818 books were issued to 408 borrowers, the corresponding numbers in the previous year being 667 and 300 respectively. The revised edition of the Catalogue, which was issued in 1920, may be had on application to the Secretary.

(27) Museum.

The classification of the objects in the Museum which was in progress on the outbreak of War has necessarily been suspended, but this work will be resumed at the earliest possible opportunity after the Institution is reinstated in its building. The following objects have been acquired during the past year:—

List of Apparatus.	*Presented by*
A Reiss electric telephone (1876).	R. Messell, D.Sc. (bequest).
An Ayrton and Perry ergmeter.	S. G. Brown, F.R.S.
A collection of German wireless apparatus:	The Air Council.
One Telefunken receiver, type D.	
One set of aero navigation lights.	
One aerial winch.	
One aerial insulator.	
Two Poge Chemnitz generators.	
One Telefunken alternator.	
One Telefunken transmitter, quenched spark gap.	
A piece of wireless apparatus (amplifier) removed from an ex-German submarine.	The Lords Commissioners of the Admiralty.
A collection of early wireless apparatus (1897–1900) mounted in a glass case with three framed photographs illustrating the apparatus.	The Marconi Wireless Telegraph Company.
A pair of telephone receivers made in 1876.	R. Pidgeon.

(28) Electrical Appointments Board.

The work of the Electrical Appointments Board during the past year has been much restricted owing to the very limited number of vacancies reported to the Board. The Council express the hope that firms with vacancies on their staffs will assist the Board by reporting these vacancies to the Secretary of the Electrical Appointments Board at the Institution offices. There are 51 members on the Register of the Board at the present time desirous of obtaining employment.

(29) Institution "Journal."

The number of pages in the 1920 Volume was 944 as compared with 682 in 1919. In addition, the Institution published in 1920 a Supplement to the 1919 Volume consisting of 336 pages and including papers which, on account of shortage and high price of paper, had remained unpublished during the two previous Sessions.

The net cost of printing and publishing the *Journal* (including the Supplement) in 1920, after allowing for sales, was £6 739, as compared with £3 396 in 1919. The Council feel that this unavoidably high expenditure is fully justified by the necessity of maintaining the character of the *Journal* as a complete record of the Proceedings of the Institution and of the papers which it accepts for publication.

The Council have decided to publish in future in the *Journal* a Classified Index of British manufacturers engaged in the electrical and allied industries. The Council consider that, in view of the wide circulation of the *Journal*, such an Index will be of considerable value. Full particulars will shortly be published.

(30) "Science Abstracts."

The Physics volume of *Science Abstracts* for 1920 showed an increase of 80 pages over that for 1919, while the Electrical Engineering volume was increased in size by 144 pages.

The deficit on the publication for 1920 was £261, as compared with £754 in 1919. This substantial reduction is due partly to the increase in the rates of subscription and partly to the continued expansion of the sales, and was effected notwithstanding further increases during the year in the cost of paper and printing.

(31) Wiring Rules.

Reference was made in last year's Report to the comprehensive revision of the Wiring Rules which had

been put in hand. The Main Committee and its six Sub-Committees have been actively at work during the Session. Each of the technical Sub-Committees has reported, some of them several times, and the Editing Sub-Committee have nearly completed the first draft of the new Rules. It is intended, after the draft has been considered and approved by the Main Committee, to forward it to the Fire Offices and to hold a Conference with their representatives. After any necessary modifications have been made, an opportunity will be afforded for discussing the draft in London and at the Territorial Centres before the Rules are published. Following the precedent set in connection with the Ship Electrical Equipment Regulations, the British Engineering Standards Association has been asked to draw up Standard Specifications for wiring accessories which, while not being definitely made compulsory, will embody the requirements of the Rules and enable users and contractors to comply with them without question.

The Sub-Committees referred to above are the following :—

Sub-Committee.	*Chairman.*
(1) Cables, Wires and Flexibles	Mr. W. Cross.
(2) Earthing	Mr. W. R. Rawlings.
(3) Fittings	Mr. J. R. Dick.
(4) Motors, Arc Lamps, Resistances, Heating Apparatus, etc.	Mr. J. R. Cowie.
(5) Planning of Installations	Mr. S. G. C. Russell.
(6) Editing	Mr. C. H. Wordingham.

(32) Ship Electrical Equipment Regulations.

These Regulations have, with certain omissions, been adopted by Lloyd's Registry of Shipping and are now in force for all ships built under their survey throughout the world. Very few questions have arisen in connection with the Regulations and it has only been necessary for the Standing Sub-Committee to meet three times. The Sub-Committee have on two occasions met representatives from the Electrical Trades Union to discuss the question of urging the Board of Trade to hold examinations and grant certificates to sea-going electricians. The Board of Trade has been approached and, though its first reply was not favourable to the proposal, the matter is still under consideration by the Committee.

(33) Power Lines Committee.

The Regulations Sub-Committee of the above Committee were originally appointed to prepare a digest of the Board of Trade Regulations and similar matters concerning Overhead Lines. The Council subsequently referred to the Sub-Committee the results of the work of the Factors of Safety Sub-Committee of the same Committee for incorporation, and, in addition, the Sub-Committee have been charged by the Council with the work of suggesting amendments to the various Board of Trade Regulations, (*a*) for securing the safety of the public, and (*b*) for ensuring a proper and sufficient supply of electrical energy, which Regulations embody those governing Overhead Lines, Earthing, etc.

The Sub-Committee meet weekly, and although the ground to be covered is extensive, it is expected that a Report will be presented before the commencement of the next Session.

(34) Electricity Supply Bill (1921).

The amending Electricity Supply Bill (No. 1), which was introduced in Parliament, as was recorded in the last Annual Report, was not further proceeded with by the Government. The provisions of a new Bill, introduced in the House on the 12th April, 1921, will receive the careful consideration of the Council. For this purpose, the Council have constituted a conference of representatives of the interested bodies who will be summoned to meet at the appropriate time.

(35) Civil Service Technical Officers.

In June last the then President (Mr. Roger T. Smith) attended a Conference of Presidents of Technical Institutions to discuss certain proposals which were being put forward by the Civil Service National Whitley Council urging the recruitment and training of non-technical civil servants for technical appointments in the Civil Service.

As a result of the Conference a letter signed by the President, jointly with the Presidents of the Institution of Civil Engineers and other Societies, was addressed to the Prime Minister deprecating the proposals.

(36) Salaries of Teachers.

In November, 1920, the Council addressed a letter to the Burnham Committee of the Board of Education urging that, should it be decided by that Committee to differentiate between technical teachers holding first-class honours degrees and those without such degrees, Corporate Membership of the Institution should be accepted as an equivalent of a first-class honours degree, which in the case of secondary teachers makes them eligible for a higher scale of salaries. The Council's recommendation is under consideration by the Committee.

(37) Finsbury Technical College.

Throughout the Session under review, the Council have been closely associated, through the President, with the efforts made to secure the continuance of the work of the Finsbury Technical College, which the City and Guilds of London had expressed their intention to close in July, 1921. In consequence of the granting of a temporary annual subsidy from the London County Council and the Board of Education, it has been decided to continue the College for the next five years.

(38) Corps Pay (Royal Engineers, etc.).

The War Office have decided that the passing of the A.M.I.E.E. examination is to be a qualification for additional pay for Officers of the Royal Engineers, Royal Army Service Corps, Royal Corps of Signals, and the Tank Corps (Technical). At the request of the War Office, arrangements were made in December last for 11 officers of the Royal Corps of Signals to sit for the A.M.I.E.E. examination in the subject of "Military Signalling (Electrical)."

(39) The Institution and Bodies on which it is Represented.

For convenience of reference, there is attached hereto (Appendix D) a diagram showing the organization of the Institution and its representation on other bodies.

(40) British Engineering Standards Association.

Considerable progress has been made during the past year in the work of industrial standardization in electrical matters. The following British Standard Specifications have been issued :—

No. 91. Electric Cable Soldering Sockets.
No. 94. Watertight Glands for Electric Cables.
No. 97. Watertight Fittings for Incandescent Electric Lamps.
No. 106. Electrically Heated Cooking Range.
No. 107. Rolled Sections for Magnet Steel.
No. 132. Steam Turbines for Electrical Plant.
No. 136. Interim List of Terms and Definitions for use in connection with Automatic Telephone Systems.
No. 139. Red Fir Wood Poles for Telegraphs and Telephones.
No. 4 E.3. Low Tension Flexible Cords and Cables (Aircraft Material).

The B.E.S.A. have undertaken the work of co-ordinating the Government Department Specifications for electrical apparatus and materials, in collaboration with the permanent Government Inter-departmental Committee set up for the purpose. The invitation to undertake this work was the outcome of a special Commission appointed by the War Office early in 1919 to investigate and report on the use of electrical power in the field. That Commission, of which Mr. C. H. Wordingham, C.B.E., Major K. Edgcumbe, and Major T. Rich were members, in their report expressed the opinion that much could be done by all Government Departments in the direction of standardization, and recommended that the various Government Departments should avail themselves of the assistance of the British Engineering Standards Association in drawing up a series of specifications. Wherever possible existing British Standards will be employed, and when the specifications so prepared are suitable they will be issued as British Standards. The work will be carried out as far as possible by the existing Committees of the Association.

A recommendation of the Wiring Rules Committee that the standardization of the dimensions of distribution boards and other accessories should be proceeded with has been adopted.

The Institution has 3 representatives on the Main Committee of the Association and 5 on its Electrical Sectional Committee.

(41) International Electrotechnical Commission.

The work of the I.E.C. during the past year has grown considerably on account of the much greater attention now being paid in most countries to the question of industrial standardization. Meetings of the I.E.C. have not been held during the past year, but a large amount of preparatory work has been carried out by correspondence for meetings which, it is hoped, will take place towards the end of this year.

(42) The British Electrical Development Association.

This Association has now been in existence for 21 months and, as the result of the recent work of its Council and Committees, it stands to-day constituted by and representative of the three principal branches of the electrical industry, its founders being the Institution of Electrical Engineers and the eight principal industrial Associations which represent the supply, manufacturing and contracting branches, viz. :—

The British Electrical and Allied Manufacturers' Association.
The Incorporated Municipal Electrical Association.
The Conference of Chief Officials of London Electric Supply Companies.
The Incorporated Association of Electric Power Companies.
The Provincial Electric Supply Committee of the United Kingdom.
The Cable Makers' Association.
The Electric Lamp Manufacturers' Association of Great Britain, Ltd.
The Electrical Contractors' Association.

The Council of the Association has recently been reconstituted and consists at present of 30 members divided between the branches and the Associations of the industry with some regard to their importance and the relative amounts they are at present subscribing towards the work of the Association. The membership is now 496, and members of the Institution interested in development work are recommended to communicate with the Director of the Association, who will be glad to give particulars of present membership, terms of subscription, and information concerning the work now in hand. The offices are at 84, Kingsway, W.C. 2.

(43) The Electrical Research Association.

The Electrical Research Committee has now become incorporated as an industrial research association, under the title of the British Electrical and Allied Industries Research Association, briefly described as the "E.R.A.," with offices at 19, Tothill Street, Westminster, S.W.

At the time when efforts were being made to establish the Association, the Institution and the B.E.A.M.A. each guaranteed £750 per annum for three years towards the expenses, and the Institution paid a first instalment of £375 under its guarantee. The Council have recently been informed by the Association that in view of the greater funds now at its disposal, funds contributed in equal parts by the electrical industry and the Research Department, the Institution has been released by the Association from its liabilities in this respect.

Under its Memorandum and Articles, the Association has taken very wide powers to foster research and the advancement of technical knowledge, and members of the Institution interested in any capacity are invited to communicate with the Director.

At present, membership of the Association is confined to manufacturers, but provision is made for enrolling other interested persons as Associate Members, and applications will shortly be invited.

The Council of the Association is constituted as follows :—

Mr. C. H. Wordingham, C.B.E. (Chairman), Mr. Ll. B. Atkinson, Dr. C. C. Garrard, Mr. J. S. Highfield, Mr. C. C. Paterson, O.B E., Mr. Roger T. Smith, Mr. C. P. Sparks, C.B.E., Mr. D. N. Dunlop, Mr. A. R. Everest, Mr. A. P. M. Fleming, C.B.E., Mr. R. S. McLeod, Mr. R. K. Morcom, C.B.E., Mr. E. S. New, Mr. C. Rodgers, Mr. W. O. Smith, Mr. A. D. Williamson,	representing this Institution. / representing the B.E.A.M.A.
Dr. W. H. Eccles, Mr. W. W. Lackie, C.B.E., Sir J. E. Petavel, K.B.E., F.R.S.	representing the Department of Scientific and Industrial Research.

The Association works in close co-operation with the British Engineering Standards Association, with the National Physical Laboratory, and with Government Departments interested.

The research programme has been extended to include important researches such as those on electric control apparatus, on corrosion of condensers, and on turbine blades and nozzles. Special consideration is being given also to developments in connection with the manufacture and use of synthetic resins.

A quarterly report of the progress of the work of the Association will appear in future in the *Journal*.

(44) Standing Joint Committee of Electricity Supply Associations on Coal.

Representatives of the bodies mentioned below have been nominated to form this Committee constituted under the auspices of the Institution to deal with matters regarding coal as far as relates to electricity supply undertakings, both company and municipal.

The Incorporated Municipal Electrical Association.
The Associated Municipal Electrical Engineers of Greater London.
The Incorporated Association of Electric Power Companies.
The Conference of Chief Officials of the London Electric Supply Companies.
The Provincial Electric Supply Committee of the United Kingdom.

In addition, the President of the Institution is *ex officio* a member of the Committee, and its Honorary Secretary is the Secretary of the Institution, to whom all communications on the subject should be addressed.

(45) The Institution of Engineers (India).

The Institution of Engineers (India), to the formation of which the Council gave its cordial support, was formally inaugurated at Calcutta by His Excellency the Viceroy of India, Lord Chelmsford, on the 23rd February last. Among the members of the Institution who were present at the meeting were Sir T. O. Callender, Mr. A. C. Coubrough, Mr. E. M. Hughman, and Mr. J. W. Meares.

(46) Society of Radiographers.

The organization of the above Society was completed early last October and members have been enrolled and meetings held. The first Council of the Society consists of :—

Name.	*Nominated by*
Medical Members.	
Dr. F. Hernaman-Johnson, M.B., Ch.B.	The Council of the British Association of Radiologists and Physiotherapists.
Dr. Robert Knox, M.B., C.M., M.R.C.S., L.R.C.P.	
Dr. S. Melville, L.S.A., M.R.C.S., L.R.C.P.	
Dr. G. Harrison Orton, M.A., B.C., M.R.C.S.	
Sir Archibald Reid, K.B.E., M.R.C.S., L.R C.P.	
Prof. Sidney Russ, D.Sc.	
Electrical Members.	
Dr. W. H. Eccles.	The Council of the Institution of Electrical Engineers.
Mr. J. S. Highfield.	
Mr. C. C. Paterson, O.B.E.	
Mr. A. A. C. Swinton, F.R.S.	
Dr. R. M. Walmsley, F.R.S.E.	
Mr. C. H. Wordingham, C.B.E.	
Radiographer Members.	
Mr. R. G. Blackall.	The Council of the B.A.R.P.
Mr. F. E. Doran.	
Mr. A. C. Forder.	
Mr. H. Henry.	The Council of the I.E.E.
Mr. H. Turner.	
Mr. G. F. Westlake.	

The Honorary Secretary is Mr. G. F. Westlake, Cancer Hospital, Fulham-road, S.W. 3.

(47) Benevolent Fund.

The Committee of Management of the Benevolent Fund of the Institution report that on the 31st December, 1920, the Capital Account of the Fund stood at £5 709 9s. 6d., and the accumulated income at £2 533 1s. 9d. The donations and subscriptions to the Fund in 1920 amounted to £820 13s. 9d.

In the course of the year 32 grants were made to 11 persons amounting to a total of £340 13s.

The following is a comparison of the number of subscriptions and the amounts received from members up to the 15th April during the present year and for the same period a year ago :—

	Number of Subscribers and Donors	Amount £ s. d.
1921.		
1 January to 15 April	884	439 17 3
1920.		
1 January to 15 April	712	£418 6 10

The Accounts of the Fund, which are separate from those of the Institution, will be found on page 359 of No. 299 of the *Journal*.

The Council have pleasure in recording that, as has already been announced, Mr. E. M. Hughman (Associate), Honorary Secretary of the Calcutta Local Centre of the Institution, has generously offered to present £250 to the Benevolent Fund, provided that 19 other subscriptions of a like amount are received by 30th June, 1921. The Committee of Management have been taking steps with a view to the desired donations being subscribed.

(48) Annual Accounts.

The Annual Accounts for the year 1920 will be found on page 354 of No. 299 of the *Journal*.

Excess of Income over Expenditure.—After making provision for contingencies as in the previous year, there is a margin to the good on the Revenue Account for 1920 of £673 7s. 1d. This amount, which has been carried to the credit of the General Fund, compares with £1 495 3s. 3d. in 1919, a decrease of £821 16s. 2d.

Mortgages.—	£ s. d.
In the Accounts for 1919 these stood at	30 198 5 5
Amount of repayment during the year	913 10 5
They now stand at	£29 284 15 0

Life Compositions Fund.—The total of the Fund on the 1st January, 1920, was £5 413 19s., and a further amount of £315 2s. was received in payment of Life Compositions during the year, a total of £5 729 1s. Out of this the sum of £52 10s. has been transferred to the General Fund, in accordance with the Articles of Association, on account of Life Compositions of members deceased during the year, leaving to the credit of the Fund £5 676 11s.

Assets.—Taking the Tothill Street property and the investments at cost, and the Institution building and lease, the library and furniture, etc., at the values standing in the books after writing off depreciation—

	£ s. d.
the Assets amount to	109 673 0 1
against Liabilities	6 976 19 3
leaving a surplus of	£102 696 0 10
which, in comparison with that of the year 1919, viz.	£97 789 10 10
shows an improvement of	£4 906 10 0

The balance of £102 696 0s. 10d. is made up as follows:—

	£ s. d.	£ s. d.
Assets.		
Properties.		
Institution Building and Tothill Street Property	92 289 3 11	
Less Mortgages ..	29 284 15 0	
		63 004 8 11
Investments, Cash, etc.		41 531 1 3
Stock of paper. Libraries and Furniture ..		5 137 9 11
		£109 673 0 1
Less *Liabilities.*		
Trust Fund Income Accounts	519 4 7	
Sundry Creditors ..	4 643 18 1	
Repairs Suspense Account	1 447 3 3	
Subscriptions received in advance ..	366 13 4	
		6 976 19 3
		£102 696 0 10

APPENDIX A.

Membership of the Institution.

The changes in the membership since the 1st April, 1920, are shown in the following table:—

	Hon. Mem.	Mem.	Assoc. Mem.	Grad.	Studt.	Assoc.	Total.	Total.
Totals at 1 April, 1920	9	1 599	4 264	632	1 157	485		8 146
Additions during the year:—								
Elected	1	29	420	173	866	9	1 498	
Reinstated ..		5	11	..	3	1	20	
Transferred to ..		90	57	53	..	..	200	
Total	1	124	488	226	869	10	1 718	
Deductions during the year:—								
Deceased	1	15	11	1	8	9	45	
Resigned ..		29	44	16	23	23	135	
Lapsed ..		..	7	6	21	1	35	
Transferred from ..		..	85	30	77	8	200	
Total	1	44	147	53	129	41	415	
Net Increase		..	..	..	..	..	..	1 303
Totals at 1 April, 1921	9	1 679	4 605	805	1 897	454		9 449

APPENDIX B.

Meetings.

The following is a list of the meetings held during the past twelve months :—

Ordinary Meetings	17	Scottish	5
Special General Meetings	2	Committees :—	
Wireless Sectional Meetings	3	Benevolent Fund	11
Informal Meetings	9	Engineers' Registration	1
Council Meetings	16	Finance	12
Territorial Centres :—		General Purposes	10
Irish	6	Informal Meetings	8
North-Eastern	10	Membership	8
North Midland	10	Model General Conditions	11
North-Western	11	Power Lines (and Sub-Committees)	28
Scottish	7	Papers	11
South Midland	9	Ship Electrical Eqnipment Standing Sub-Committee	1
Western	6	Society of Radiographers	1
Territorial Sub-Centres :—		Territorial Centres	1
Aberdeen	1	War Memorial	2
Dundee	5	Wireless Section	5
East Midland	11	Wiring Rules	8
Liverpool	8	Other Committees and various Conferences	21
Sheffield	6		
Tees-side	8	Total	331
Students' Sections :—			
London	11		
Birmingham	6		
Liverpool	4		
Manchester	7		
Newcastle	14		

APPENDIX C.

Addresses.

Author.	*Title.*
Ll. B. Atkinson, President.	Inaugural Address.
Ll. B. Atkinson, President.	Address to the Students' Sections.
J. R. Beard, Member.	Chairman's Address (North-Eastern Centre).
A. G. Bruty, Member	Chairman's Address (Irish Centre).
H. Dickinson, Member	Chairman's Address (Liverpool Sub-Centre).
W. H. Eccles, D.Sc., Member.	Chairman's Address (Wireless Section).
F. Forrest, Member.	Chairman's Address (South Midland Centre).
J. Frith, Member.	Chairman's Address (North-Western Centre).
F. S. Grogan, Member.	Chairman's Address (East Midland Sub-Centre).
C. J. Jewell, Member.	Chairman's Address (North Midland Centre).
A. J. Newman, Member.	Chairman's Address (Western Centre).
J. E. Sayers, Member.	Chairman's Address (Scottish Centre).

Lectures.

Sir W. Bragg, K.B.E., F.R.S.	"Electrons" (Twelfth Kelvin Lecture).
Prof. E. Wilson, Member.	"Magnetic Snsceptibility of Low Order" (3 lectures).

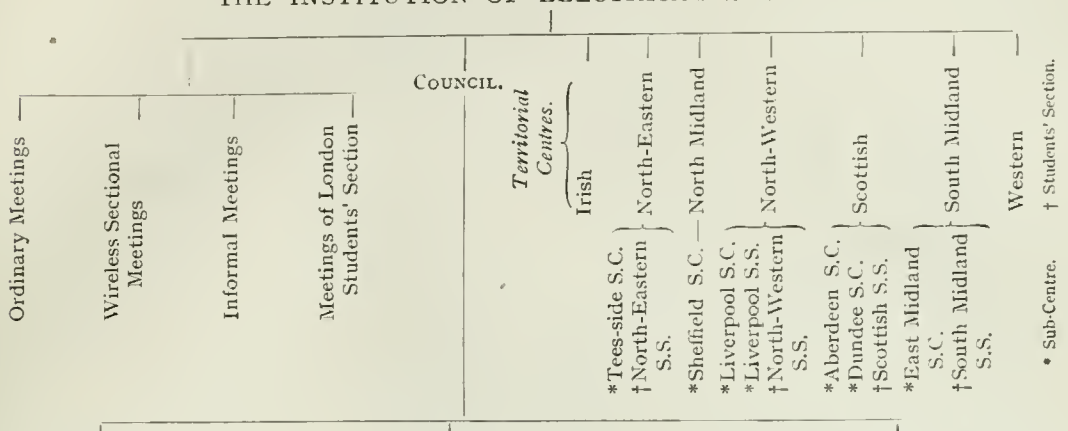
Papers Read at Meetings and Accepted for Publication in the "Journal."

K. Baumann.	"Some Recent Developments in Large Steam Turbine Practice."
J. W. Beauchamp, Member.	"Multi-part Tariffs for Domestic Electricity Supply.
J. R. Blaikie, Member.	"Electric Supply: Present Conditions and the Hopkinson Principles."
F. Creedy, Associate Member.	"Some Developments in Multi-speed Cascade Induction Motors."
W. H. Eccles, D.Sc., Member, and Miss W. A. Leyshon.	"Some Thermionic Tube Circuits for Relaying and Measuring."
E. Griffiths, D.Sc., and F. H. Schofield, B.A., B.Sc.	"Some Thermal Characteristics of Electric Ovens and Hot-Plates."
G. A. Juhlin, Member.	"Temperature Limits of Large Alternators."
R. O. Kapp, Associate Member.	"Some Economic Aspects of E.H.T. Distribution by Underground Cable."
A. B. Mallinson, Associate Member.	"Electric Driving in the Paper Mill on Heat-Economy Lines."
Sir W. Noble, Member.	"The Long-Distance Telephone System of the United Kingdom."
Lt.-Col. H. E. O'Brien, D.S.O., Member.	"The Application of Electric Locomotives to Main-Line Traction on Railways."
G. Stead, M.A.	"The Effect of Electron Emission on the Temperature of the Filament and Anode of a Thermionic Valve."
Capt. R. C. Trench, R.E., Associate Member.	"Range of Wireless Stations."
E. A. Watson, O.B.E., Associate Member.	"Magnetos for Ignition Purposes in Internal Combustion Engines."
W. B. Woodhouse, Member.	"The Distribution of Electricity."

In addition, a number of papers have been accepted for publication in the *Journal* without being read at meetings.

APPENDIX D.

THE INSTITUTION OF ELECTRICAL ENGINEERS.



Committees. (*Standing.*)

Examinations
Finance
Informal Meetings
General Purposes
Library
Membership
Overseas Information
Papers
Paul Scholarship
"Science Abstracts"
Ship Electrical Equipment
Technical Co-operation
Territorial Centres
Wireless
Wiring Rules

Sectional Committees.
- Electricity in Mines
- Electro-Chemistry and Electro-Metallurgy
- Lighting and Power
- Telegraphs and Telephones
- Traction

Committees. (*Special.*)

Dutch Return Visit
Engineers' Registration
Model General Conditions
Power Lines
Secondary Batteries Maintenance Agreement
War Memorial

Bodies on which the Institution is Represented.

Birmingham Chamber of Commerce
Bradford Public Libraries Committee
Bristol University
British Electrical and Allied Industries Research Association
British Engineering Standards Association
British National Committee of the International Electrotechnical Commission
Corrosion Committee of Institute of Metals
Darlington Board of Invention and Research
Duddell Memorial Committee of Physical Society of London
Electrical Development Association
Fuel Economy Committee of British Association
Imperial College of Science and Technology, Governing Body
Imperial Mineral Resources Bureau Conference
International Scientific Unions
International Illumination Commission, British National Illumination Committee
International Testing Association
Leeds Civic Society
Leeds Municipal Technical Library Committee
Loughborough Technical College Advisory Committee
Metalliferous Mining (Cornwall) School, Governing Body
Middlesbrough Technical College
National Committee for Physics (Royal Society)
National Committee in Radio Telegraphy (Royal Society)
National Physical Laboratory, General Board
Newcastle-on-Tyne Chamber of Commerce
Radiographers, Society of
Röntgen Society Advisory Committee for British X-Ray Industry
Royal Engineer Board
Scientific and Industrial Research Advisory Council, Engineering Committee
Scientific Societies, Conjoint Board of
Technical Societies of Canada, Joint Committee
Transport, Ministry of, Advisory Panel
War Office Committee on Engineer Organization

THE INSTITUTION OF ELECTRICAL ENGINEERS.

REVENUE ACCOUNT FOR THE YEAR ENDED 31ST DECEMBER, 1920.

Dr. EXPENDITURE. INCOME. **Cr.**

Year ended 31 Dec., 1919. £ s. d.	EXPENDITURE	£ s. d.	£ s. d.
	To Management :—		
	Salaries and Wages (including Staff Provident Scheme and War Bonus) ...	8,454 17 0	
	National Insurance	17 14 10	
	Accountants' Fees	31 10 0	
	Printing	597 9 4	
	Stationery and Office Requisites	603 1 11	
	Addressing	42 11 0	
	Postage of Correspondence and Notices	617 18 6	
	Telephone	47 17 0	
	Travelling Expenses	54 12 9	
9,125 6 5	Bank Charges	12 18 9	10,480 11 1
	„ Institution Building :—		
	Ground Rent	2,201 0 0	
	Rates and Taxes	734 2 8	
	Insurance	123 3 7	
	Reserve for Repairs	400 0 0	
	Estate Agents' Fees	73 10 0	
3,438 19 5			3,531 16 3
1,175 11 11	„ Temporary Offices (No. 1 Albemarle Street) :— Rent, Lighting, Heating, Cleaning, etc....		1,168 2 9
1,379 1 7	„ Interest on Mortgages		1,340 13 3
13 0 3	„ Furniture and Fittings (Repairs and Renewals)		25 3 0
	„ Journal :—		
	Printing	5,976 13 2	
	Postage	1,309 10 9	
	Wrappers and Envelopes	278 11 0	
		7,564 14 11	
	Less Sales	825 11 8	
3,396 4 1			6,739 3 3
60 14 4	„ Lending Library (Books, Printing, Postage, etc.) ...		94 11 0
5 2 0	„ Thompson Memorial Library :— Insurance, etc.		5 2 0
——	„ National Housing Conference		12 12 0
	„ Science Abstracts :—		
	Salaries, Abstracting, Printing, Postage, etc....	3,291 18 11	
	Less Subscriptions, Sales, and Advertisements	3,030 11 6	
754 6 2			261 7 5
	Carried Forward		£23,659 2 0

Year ended 31 Dec., 1919. £ s. d.	INCOME	£ s. d.	£ s. d.
19,532 1 4	By Subscriptions		22,319 16 6
2,311 1 0	„ Entrance Fees and Vellum Diploma Fees		1,671 13 6
321 6 0	„ Life Compositions		315 2 0
17 10 0	„ Donations and Subscriptions towards Cost of Building		11 17 6
1,387 5 0	„ Dividends and Interest		1,775 9 4
0 15 2	„ Street Lighting Specifications		0 3 7
——	„ Ship Wiring Rules		22 19 9
——	„ Examinations (Fees and Sales of Papers *less* Expenses)		15 6 3
	„ Institution Building :—		
6,240 5 0	Rent from Tenants		6,615 0 0
	„ Tothill Street Property :—		
	Rents from Tenants	923 1 11	
	Less Ground Rent, Repairs, Alterations, Rates, Taxes, etc.	365 10 11	
529 14 4			557 11 0
	Carried Forward		£33,304 19 5

REVENUE ACCOUNT—*continued.*

Dr. EXPENDITURE—*continued.*

				£ s. d.	£ s. d.
			Brought Forward	...	... 23,659 2 0
			To MEETINGS :—		
			Advance Proofs	181 18 9	
			Reporting	133 7 0	
			Honorarium to Kelvin Lecturer	25 0 0	
			Refreshments, Assistance, etc.	175 11 2	
			Hire of Rooms	148 1 0	
600	*9*	*10*			663 17 11
			„ TERRITORIAL CENTRES :—		
			Money Grants	1,729 13 7	
			Travelling Expenses	512 2 6	
1,561	*16*	*9*			2,241 16 1
140	*7*	*8*	„ PREMIUMS FOR PAPERS	... ...	192 7 4
			„ SPECIAL GRANTS :—		
			Conjoint Board of Scientific Societies ...	50 0 0	
			International Electrotechnical Commission	165 19 2	
			Society of Radiographers	70 0 0	
540	*9*	*6*			285 19 2
110	*2*	*10*	„ ANNUAL DINNER	... ...	109 2 0
858	*8*	*5*	„ CONVERSAZIONE...	... ...	492 11 5
25	*18*	*8*	„ WIRING RULES (Printing, *less* Sales) ...	... ...	5 4 10
5	*12*	*7*	„ MODEL GENERAL CONDITIONS (Printing, *less* Sales)	... ...	21 12 2
			„ RESEARCH :—		
400	*0*	*0*	Buried Cables Research	... ...	54 10 0
10	*10*	*0*	„ LEGAL EXPENSES	... ...	212 11 3
84	*5*	*9*	„ MISCELLANEOUS EXPENSES	... ...	71 14 8
					28,010 8 10
321	*6*	*0*	„ AMOUNTS TRANSFERRED TO :—		
			LIFE COMPOSITIONS FUND (*per contra*) ...	315 2 0	
277	*12*	*2*	SINKING FUND (Premiums for Redemption of Cost of Building and Lease) ...	277 12 2	
3,000	*0*	*0*	RESERVE FUND (Contingencies and Mortgage Redemption)	3,000 0 0	
			GENERAL FUND :—		
			Obligatory Repayment to Economic Life Assurance Society :—		
			Donations and Subscriptions towards Cost of Building £11 17 6		
			Contribution out of Revenue 901 12 11		
875	*18*	*0*		913 10 5	
			Expenditure on—		
58	*9*	*10*	Books and Binding for Library ...	114 18 11	
1,495	*3*	*3*	**Balance carried to General Fund** ...	673 7 1	
					5,294 10 7
					£33,304 19 5

Cr. INCOME—*continued.*

				£ s. d.
			Brought Forward	... 33,304 19 5
				£33,304 19 5

BALANCE SHEET, 31ST DECEMBER, 1920.

Dr. LIABILITIES.

	£ s. d.	£ s. d.
To Economic Life Assurance Society :—		
On Mortgage of Institution Building (1909) ...	26,000 0 0	
Since repaid	8,215 5 0	
	17,784 15 0	
On Mortgage of Tothill Street Buildings and Site (1910)	11,500 0 0	
		29,284 15 0
„ Life Compositions Fund :—		
Balance at 1st January, 1920	5,413 19 0	
Life Compositions received in 1920	315 2 0	
	5,729 1 0	
Less Life Compositions of Deceased Members transferred to General Fund	52 10 0	
		5,676 11 0
„ Kelvin Lecture Fund :—		
As per last Balance Sheet		648 13 0
„ Trust Funds Capital Accounts :—		
Salomons Scholarship	2,126 19 3	
David Hughes Scholarship	2,000 0 0	
Paul Scholarship	500 0 0	
Wilde Benevolent Fund	2,586 19 2	
War Thanksgiving Education and Research Fund (No. 1)	1,700 0 0	
		8,913 18 5
„ Trust Funds Income Accounts :—		
Balances unexpended :—		
Salomons Scholarship	163 6 8	
David Hughes Scholarship	140 0 4	
Paul Scholarship	18 15 0	
Wilde Benevolent Fund	97 2 7	
War Thanksgiving Education and Research Fund (No. 1)	100 0 0	
		519 4 7
„ Sundry Creditors		4,643 18 1
„ Subscriptions received in advance		366 13 4
„ Repairs Suspense Account :—		
Balance (as per last Balance Sheet)	1,095 17 8	
Amount set aside in 1920	400 0 0	
	1,495 17 8	
Less Expenditure on Repairs in 1920	48 14 5	
		1,447 3 3
Carried Forward		51,500 16 8

Cr. ASSETS.

	£ s. d.	£ s. d.
By Institution Building and Lease :—		
Cost	73,028 6 10	
Less Reserve for Depreciation, being Surrender Values of Sinking Fund Policies	3,301 1 5	
		69,727 5 5
„ Sinking Fund (Surrender Values of Policies for Redemption of Cost of Building and Lease)		3,301 1 5
„ Tothill Street Buildings and Site (at cost)		19,260 17 1
„ Life Compositions Investments (at cost)* :—		
£2,600 Natal Zululand Railways 3% Debentures	2,270 12 0	
£1,500 Lancashire and Yorkshire Railway 4% Preference Stock	1,513 10 4	
£2,000 Assam Bengal Railways 3% Stock ...	1,548 0 6	
£350 Western Australia 4% Stock	340 17 2	
		5,673 0 0
„ Kelvin Lecture Fund Investment (at cost)* :—		
£694 16s. 9d. 5% War Stock		648 13 0
„ Trust Funds Investments (at cost)* :—		
Salomons Scholarship :—		
£1,500 New South Wales 3½% Stock	1,556 5 9	
£500 Cape of Good Hope 3½% Stock	570 13 6	
		2,126 19 3
David Hughes Scholarship :—		
£2,045 Staines Reservoirs 3% Guaranteed Debenture Stock	1,998 15 0	
Cash at Post Office Savings Bank	1 5 0	
		2,000 0 0
Paul Scholarship :—		
£625 4% Funding Loan		500 0 0
Wilde Benevolent Fund :—		
£875 Great Eastern Railway Metropolitan 5% Guaranteed Stock	1,493 16 3	
£215 North Eastern Railway 4% Guaranteed Stock	250 19 9	
£100 London County 3½% Stock	101 8 6	
£250 New South Wales 4% Stock	251 6 0	
£100 3½% War Stock	94 8 8	
£100 5% War Stock	95 0 0	
£381 15s. 1d. 4% Funding Loan	300 0 0	
		2,586 19 2
War Thanksgiving Education and Research Fund (No. 1) :—		
£2,000 5% War Stock		1,700 0 0
„ Library (exclusive of the Ronalds Library and Faraday Papers, which are held in trust) :—		
As per last Balance Sheet	1,053 0 4	
Additions in 1920	114 18 11	
	1,167 19 3	
Less Depreciation (10%)	116 15 11	
		1,051 3 4
Carried Forward		108,575 18 8

BALANCE SHEET—*continued.*

Dr. LIABILITIES—*continued.*

	£ s. d.	£ s. d.
Brought Forward		51,500 16 8
To Reserve Fund (Mortgage Redemption and Contingencies) ...		14,000 0 0
„ General Fund :—		
Balance at 1st January, 1920	80,834 12 8	
Life Compositions of Deceased Members ...	52 10 0	
Obligatory Repayment to Economic Life Assurance Society	913 10 5	
Expenditure in 1920 on—		
Books and Binding for Library	114 18 11	
Balance from Revenue Account for 1920 ...	673 7 1	
	82,588 19 1	
Less Depreciation (*per contra*) :—		
Library £116 15 11		
Furniture, Fittings, and Apparatus 101 6 4		
	218 2 3	
		82,370 16 10
		£147,871 13 6

JAMES DEVONSHIRE,
Honorary Treasurer.
P. F. ROWELL,
Secretary.

Cr. ASSETS—*continued.*

	£ s. d.	£ s. d.
Brought Forward		108,575 18 8
By Thompson Memorial Library (Contribution towards purchase)		1,000 0 0
„ Furniture, Fittings, and Apparatus :—		
As per last Balance Sheet	2,026 6 10	
Less Depreciation (5 %)	101 6 4	
		1,925 0 6
„ Museum (Instruments purchased from the collection of the late Sir William Crookes)		213 12 6
„ Sundry Debtors		2,672 11 4
„ Insurance Premiums and Sundry Payments in Advance ...		398 4 6
„ Stock of Paper, Etc., for Publications		947 13 7
„ General and Reserve Funds Investments (at cost)* :—		
£400 Western Australia 4 % Stock	389 11 1	
£750 Union of South Africa 4 % Stock	742 12 0	
£750 Madras and Southern Mahratta Railway 4 % Debenture Stock	738 15 6	
£35 East Indian Railway "B" Annuity	791 5 4	
£1,500 South Australian 4% Stock	1,494 10 6	
£5,250 5 % War Stock	4,987 10 0	
£2,000 5 % National War Bonds	2,000 0 0	
£19,375 4 % Funding Loan	15,500 0 0	
£2,000 5½ % Exchequer Bonds	1,991 9 0	
		28,635 13 5
„ Cash in Hands of Territorial Centres on 30 Sept., 1920 ...		165 13 9
„ Cash :—		
At Bankers'	3,308 5 8	
At Post Office Savings Bank (David Hughes Scholarship Trust Fund Uninvested Income)	0 2 9	
In hands of Secretary	28 16 10	
		3,337 5 3
		£147,871 13 6

* The above values are subject to depreciation.

We beg to report that we have audited the Balance Sheet of The Institution of Electrical Engineers, dated 31st December, 1920, and above set forth, together with the annexed Statements of Account. We have obtained all the information and explanations we have required. In our opinion the Statements are correct, and the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Institution's affairs according to the best of our information and the explanations given to us and as shown by the books of the Institution.

ALLEN, ATTFIELD & CO., *Auditors*,
Chartered Accountants,
24, Martin Lane, Cannon Street, E.C. 4.

2nd April, 1921.

SALOMONS SCHOLARSHIP TRUST FUND (Income).

Dr.	£ s. d.	Cr.	£ s. d.
To Amount paid toScholars in 1920...	175 0 0	By Balance (as per last Account)	268 10 7
„ Balance carried to Balance Sheet	163 6 8	„ Dividends received in 1920	69 16 1
	£338 6 8		£338 6 8

DAVID HUGHES SCHOLARSHIP TRUST FUND (Income).

Dr.	£ s. d.	Cr.	£ s. d.
To Amount paid to Scholars in 1920	175 0 0	By Balance (as per last Account)	254 6 8
„ Balance carried to Balance Sheet	140 0 4	„ Dividends received in 1920	60 13 2
		„ Interest do. do.	0 0 6
	£315 0 4		£315 0 4

PAUL SCHOLARSHIP FUND (Income).

Dr.	£ s. d.	Cr.	£ s. d.
To Amount paid to Scholar in 1920	25 0 0	By Balance (as per last Account)	18 15 0
Balance carried to Balance Sheet	18 15 0	„ Dividends received in 1920	25 0 0
	£43 15 0		£43 15 0

WILDE BENEVOLENT TRUST FUND (Income).

Dr.	£ s. d.	Cr.	£ s. d.
To Balance carried to Balance Sheet	97 2 7	By Balance (as per last Account)	7 8 4
		„ Dividends received in 1920	87 13 11
		„ Interest do. do.	2 0 4
	£97 2 7		£97 2 7

WAR THANKSGIVING EDUCATION AND RESEARCH FUND (No. 1).

Dr.	£ s. d.	Cr.	£ s. d.
To Balance carried to Balance Sheet	100 0 0	By Dividends received in 1920	100 0 0
	£100 0 0		£100 0 0

ENTERTAINMENT FUND.

Dr.	£ s. d.	Cr.	£ s. d.
To Cash at Bank, 31st December, 1920	219 3 10	By Balance from Western Centre Meeting, 1920 ...	125 0 0
		„ Balances from previous Meetings	92 14 2
		„ Interest received in 1920	1 9 8
	£219 3 10		£219 3 10

THE BENEVOLENT FUND OF THE INSTITUTION OF ELECTRICAL ENGINEERS.

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR 1920.

Dr. Expenditure.	£ s. d.	Income.	£ s. d.	Cr.
To Grants	340 13 0	By Dividends on Investments	296 14 4	
„ Printing, Stationery, Bank Charges, and Postage	5 19 8	„ Interest	12 0 8	
„ Unexpended Balance carried to Balance Sheet	782 16 1	„ Annual Subscriptions	211 18 6	
		„ Donations of £5 and over	365 11 2	
		„ Donations under £5	243 4 1	
	£1,129 8 9		£1,129 8 9	

BALANCE SHEET, 31ST DECEMBER, 1920.

Dr. Liabilities.	£ s. d.	£ s. d.	Assets.	£ s. d.	£ s. d.	Cr.
To Capital Account :—			By Investments (Capital), at cost :—			
As per last Balance Sheet	5,709 9 6		£961 7s. 7d. Cape of Good Hope 3 % Stock	950 0 0		
„ Income and Expenditure Account :—			£593 1s. 7d. New South Wales 3 % Stock	600 0 0		
As per last Balance Sheet	£1,750 5 8		£420 Great Eastern Railway 4 % Preference Stock	503 18 3		
Unexpended Balance in 1920	782 16 1		£600 North Staffordshire Railway 3 % Debenture Stock	551 0 9		
	2,533 1 9		£750 East Indian Railway 3½ % Debenture Stock	737 18 0		
„ Sundry Creditors	38 8 6		£300 London and North-Western Railway 4 % Guaranteed Stock	333 11 6		
			£500 New Zealand 3½ % Stock	486 18 6		
			£500 Canada 3½ % Stock	478 16 0		
			£1,126 6s. 3d. 5 % War Stock	1,067 6 6		
					5,709 9 6	
			„ Investments (Income), at cost :—			
			£350 New South Wales 4 % Stock	£336 18 6		
			£200 3½ % War Stock	188 17 3		
			£2,128 8s. 6d. 4 % Funding Loan	1,624 2 0		
					2,149 17 9	
			„ Sundry Debtors		11 12 9	
			„ Cash :—			
			At Bankers'	£364 3 2		
			In hand	45 16 7		
					409 19 9	
	£8,280 19 9				£8,280 19 9	

We have audited the above Balance Sheet and Income and Expenditure Account with the Books and Vouchers and certify them to be correct, and have verified the Investments with Certificates from Bankers. The Investments, which are stated at cost, are subject to depreciation.

JAS. ATTFIELD, F.C.A.
SIDNEY SHARP } *Honorary Auditors.*

18th April, 1921.

PROCEEDINGS OF THE INSTITUTION.

659TH ORDINARY MEETING, 10 FEBRUARY, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 27th January, 1921, were taken as read, and were confirmed and signed.

The President announced that the Council had elected Mr. A. A. C. Swinton, F.R.S., to be a Vice-President in place of Mr. S. L. Pearce, C.B.E., who had resigned on grounds of health.

A list of candidates for election and transfer approved by the Council for ballot was taken as read and was ordered to be suspended in the Hall.

The following donations were announced as having been received and the thanks of the meeting were accorded to the donors:—

Benevolent Fund: (see *Institution Notes*, No. 23, p. 10, Dec. 1920).

Building Fund: J. D. Dallas.

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After an introductory paper by Mr. E. Griffiths, D.Sc., and Mr. F. H. Schofield, B.A., B.Sc., entitled "Some Thermal Characteristics of Electric Ovens and Hot-Plates" (see page 361), a discussion was held on the subject of "Electric Appliances for Domestic Purposes."

The meeting terminated at 7.45 p.m.

SOME THERMAL CHARACTERISTICS OF ELECTRIC OVENS AND HOT-PLATES.

By Ezer Griffiths, D.Sc., and F. H. Schofield, B.A., B.Sc.

(From the National Physical Laboratory.)

(*Paper received* 3 *January, and read before* The Institution 10 *February, before the* North Midland Centre 22 *February, before the* South Midland Centre 23 *February, before the* North-Eastern Centre 25 *April, and before the* Scottish Centre 29 *April*, 1921.)

Summary.

The formulation of simple tests for determining the efficiency of hot-plates and cooking stoves is complicated by the fact that the appliances are employed for a variety of operations and, in attempting to devise tests, it is, of course, very desirable that the conditions should correspond as closely as possible to those prevailing in practice. The ordinary cooking utensils are not sufficiently standardized to be used for quantitative experiments, so in the tests on hot-plates metallic discs of the same order of cross-section and thermal capacity as an ordinary kettle or saucepan were used for the purpose. It is shown that the use of a thin disc supported off the plate by three short pegs is not to be recommended, since the conditions of the test are then of a different character from those in actual practice, and this operates unfairly against the hot-plates with machined surfaces, compared with the radiant types. The difficulty is surmounted by the use of a thick disc with a flat machined surface.

When specifying tests for cooking stoves, two distinct phases have to be considered. First, the heating-up period, and secondly, the cooling period when the temperature is maintained steady or allowed to fall slightly. The efficiency test of an oven should consist in the determination of (1) the number of kilowatt-hours required to bring the temperature of the interior to the maximum within a certain period; (2) the energy required to keep it at the minimum temperature necessary for cooking operations.

The British Engineering Standards Association has recently issued a Standard Specification (No. 106—1920) for electrically heated cooking ranges. Among other requirements, certain thermal tests are prescribed which are based largely on some experimental work carried out at the National Physical Laboratory in 1918 at the request of the (then) Engineering Standards Committee. A detailed report was presented at the time to the Committee, and the chairman, Mr. C. H. Wordingham, C.B.E., thought that a review of the results might usefully be published in connection with the prescribed tests, so that the reason for their selection might be understood. It is as well to emphasize that the present paper is concerned with hot-plates and cooking stoves solely from the thermal point of view, and that it does not touch on the constructional, electrical and other factors which are also dealt with in the specification.

The formulation of simple tests for determining the efficiency of hot-plates and cooking stoves is complicated by the fact that the appliances are employed for a variety of operations and that special types have been evolved for special purposes. Thus, for toasting or grilling, a high proportion of radiant energy is required, involving the use of the bare resister elements run at a red heat; for intermittent operations the lightly constructed plate or oven embodying a minimum amount of metal has obvious advantages; while for prolonged or continuous operations the heavily constructed plate or well-lagged oven is probably the most suitable.

The cooking appliances sent to the National Physical Laboratory included some 15 makes of hot-plates and eight makes of ovens, and seem to have been a representative selection of the various types.

In the experiments a temperature of 200° C. (392° F.) has been taken as the upper limit to which the appliances would be required to work. The choice is somewhat arbitrary, but it represents the point generally aimed at for roasting, baking or frying operations.

HOT-PLATES.

The more common purposes for which hot-plates are used are frying and boiling.

In the case of frying, what is desired is a plate of very small thermal capacity which will quickly raise the temperature of a light pan to a moderately high temperature for the least expenditure of energy, while for boiling large volumes of water the chief desideratum is a plate which, when working at an approximately constant temperature, will transfer to the water the largest fraction of the energy supplied.

The transfer of heat from the hot-plate to the cooking utensil may be effected mainly by the metallic conduction, where the utensil and the hot-plate have flat surfaces which are in contact, or it may take place through the agency of radiation and of conduction through the air. This difference in the mode of heat transfer corresponds to the two main types of hot-plate, namely, the plain or enclosed type and the open or radiant type. In the former a heating unit is clamped on to the underside of an iron disc the top of which is machined smooth; while in the latter the heater is generally a bare resister wire coiled and laid in a grooved disc of fireclay, which is covered by a light grid to support the cooking utensil.

In attempting to devise tests for hot-plates, it is, of course, very desirable that the conditions should correspond as closely as possible to those prevailing in practice. The ordinary cooking utensils are not sufficiently standardized to be used for quantitative tests, and it was necessary, therefore, to substitute special appliances for them. It will be seen below that metallic discs of the same order of cross-section and thermal capacity were used for the purpose, and that attempts were made to determine the effect of various

surface conditions in the heat-receiving face. Experiments were also made with special cast-iron vessels containing oil.

Experiments with Discs.

It was first suggested that the test on hot-plates might consist in observing the rate of rise in temperature of a copper disc placed over the plate, and that, in order to make the conditions definite and reproducible by eliminating uncertainties of contact, the disc should not rest directly on the plate but be supported on three short steel pegs.

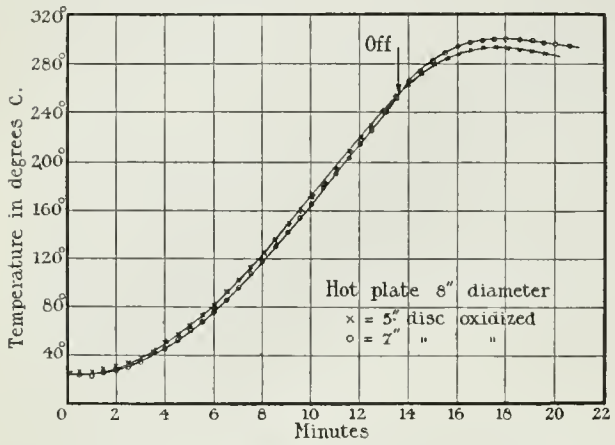

Fig. 1.—Influence of size of disc relative to that of plate.

In connection with the suggested method of test the following factors required investigation:

(1) The distribution of temperature over the disc.
(2) The influence of the size of the disc relative to that of the plate.
(3) The influence of the state of oxidation of the surface of the disc.
(4) The order of the difference produced when the disc was in good thermal contact with the hot-plate, and when separated from it by steel pegs.

Except where otherwise stated, the copper disc used was 4·8 mm. (3/16 in.) thick and 17·8 cm. (7 in.) in diameter, it was supported off the plate by steel pegs about 2 mm. (0·08 in.) long, and the surface of the disc was oxidized. The temperature distribution was studied by means of a number of thermo-couples pegged into the upper surface of the disc. The following results were obtained during the heating up of the plate.

Temperature distribution.—This was quite uniform for both the open and enclosed types of plate, except where the disc was larger than the heat-emitting area. In the latter case, as would be expected, the outer edge of the disc lagged behind the centre.

Influence of diameter of disc.—Copper discs of the same thickness but of different diameters, 13·3 cm. (5·3 in.) and 17·8 cm. (7 in.) respectively, were placed over an enclosed hot-plate of diameter 20·3 cm. (8 in.). The watt supply was approximately the same for the two experiments, and it will be observed from Fig. 1 that there is only a very slight difference between the rate of temperature-rise in the two cases. This implies that the disc only absorbs the energy emitted from the area actually covered. The uncovered area radiates the heat into space, and there is but little transverse conduction across the top of the plate.

Influence of surface oxidation.—When supported off the hot-plate by the steel pegs the copper disc is heated primarily by radiation and by air conduction through an air layer of 2 mm. thickness. Convection in such a narrow horizontal layer would probably be of negligible magnitude. Hence it would follow that the degree of oxidation of the receiving surface would exercise a considerable influence on the heat transfer by radiation.

Theory of the heat transmission for the case of a disc on pegs.—If the surface were perfectly black, i.e. absorbed all incident radiation, then the transfer of heat by *radiation* would be given by the formula:

$$H_R = \sigma(T_1^4 - T_0^4)$$

where σ is the black-body constant, and T_1 and T_0 the absolute temperatures of the surfaces respectively.

Actually, the coefficient of emission of an oxidized cast-iron plate is less than that of a black body, being about 70 per cent of the latter, while for oxidized copper the value 50 per cent would be of the right order.

Neglecting, for the moment, the effect of multiple reflections between the plates, the transfer under these conditions would be

$$H_R = 0{\cdot}35\sigma(T_1^4 - T_0^4)$$

while if the copper surface is cleaned up with emery paper its coefficient would probably be less than 10 per cent. So that the heat transfer for a given difference of temperature would only be one-fifth that obtained with the oxidized surface.

If account is taken of multiple reflections of energy between the two surfaces, it can easily be shown that the transfer between surfaces of coefficients m and n becomes, instead of mn,

$$\frac{mn}{m + n - mn}$$

Taking the last case of an oxidized iron surface with a 70 per cent coefficient and a bright copper surface with a 10 per cent coefficient, the value is 0·096 instead of 0·07. So that the effect of multiple reflections is to increase considerably the heat transfer by radiation.

Assuming the standard case of both surfaces being oxidized, the coefficients being 70 per cent and 50 per cent respectively, we can calculate the magnitudes of the radiation and air-conduction portions of the heat transfer. Take the temperatures of the copper disc and hot-plate at a given instant to be 100° C. and 200° C. respectively, then the heat transfer by radiation, in calories per sq. cm. per sec., is

$$\begin{aligned} H_R &= \frac{mn}{m + n - mn}\sigma(T_1^4 - T_0^4) \\ &= \frac{0{\cdot}35 \times 1{\cdot}28 \times 10^8}{10^{12}}(500 - 195) \\ &= 0{\cdot}0137 \text{ calorie per sq. cm. per sec.} \end{aligned}$$

With the same temperatures between the surfaces and assuming an air-gap of 2 mm. we have, by heat conduction through the air layer,

$$H_C = \frac{K(T_1 - T_0)}{d} = \frac{100 \times 0{\cdot}00005}{0{\cdot}2}$$

$= 0{\cdot}025$ calorie per sq. cm. per sec.

So that radiation is the more important factor in the heat transmission under these conditions.

In the case of the open spiral types of hot-plates the conditions are somewhat different: the spirals are running at a steady bright-red heat and the fireclay grooves are also at a fairly high temperature and, since the air-gap is considerably increased, it is probable that some

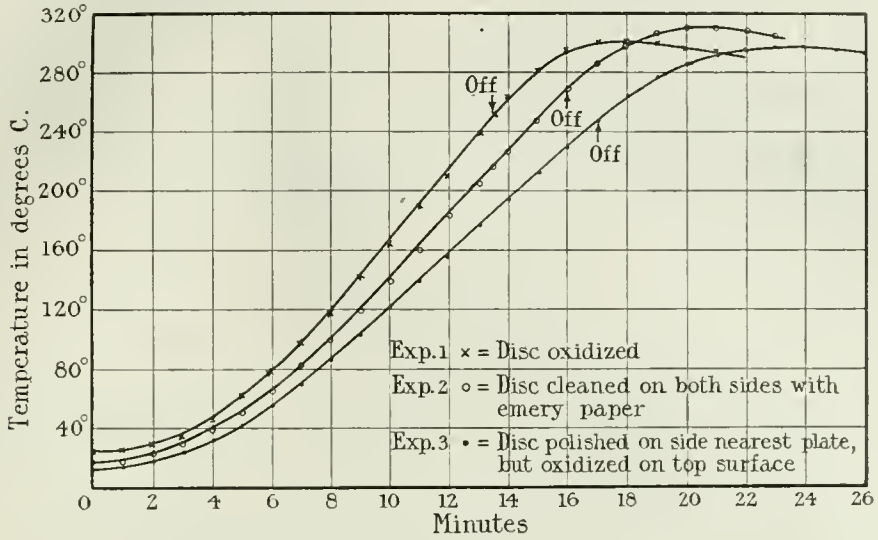

Fig. 2.—Influence of surface oxidation of copper disc.

So that for this difference of temperature the heat transmitted by thermal conduction is greater than that by pure radiation.

It is probable that after the initial stage the temperature difference considerably exceeds 100 degrees C., convection occurs. Under these circumstances it would be expected that thermal conduction would be relatively unimportant compared with radiation and convection transfer.

Some experiments were made with a closed-type

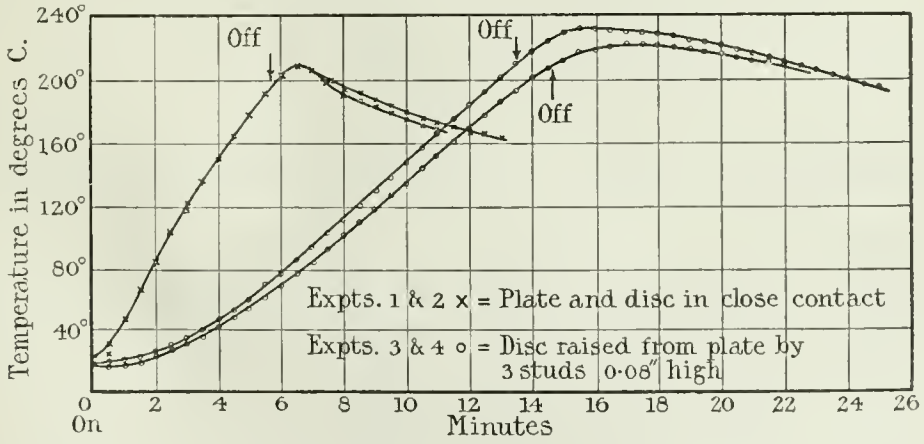

Fig. 3.—Influence of metallic contact between hot-plate and disc.

since it has been repeatedly observed that the surface of the hot-plate was at a red heat (about 600° C.) when the copper disc was in the neighbourhood of 300° C.

Calculating in the same way for a temperature difference of 300 degrees we have

$H_R = 0{\cdot}095$ calorie per sq. cm. per sec.
$H^C = 0{\cdot}075$ calorie per sq. cm. per sec.

plate to illustrate the influence of the surface condition of the receiving copper disc.

It is impossible to show the effect of polishing to the best advantage with copper, owing to the tarnishing which occurs as soon as the temperature of the disc is raised appreciably. Results obtained with different degrees of initial polish are, however, shown in Fig. 2.

The watts supplied were approximately equal in the

three experiments, being about 1 490. In the first experiment, using an oxidized disc, the temperature-rise at the end of 13 minutes was about 30 degrees C. more than for the second experiment, in which the disc was cleaned on both sides with emery paper. In the second experiment the rise was nearly 30 degrees C. more than in the third experiment, where the receiving surface was highly polished and the top surface oxidized. The effect of polishing the surface is thus to reduce considerably the rate at which the disc absorbs heat.

Effect of metal contact between hot-plate and disc.—For these experiments a rectangular hot-plate 20·3 cm. (8 in.) by 15·2 cm. (6 in.) and a copper disc 12·7 cm. (5 in.) diameter and 0·8 cm. (5/16 in.) thick were taken. It was found necessary to true-up the top of the hot-plate as well as the copper disc. The results obtained are shown in Fig. 3.

Summary of results with copper disc.—Some typical results obtained with the copper disc are set out in Table 1. In the columns of "efficiencies" is given the ratio of the heat absorbed by the disc, i.e. the heat capacity of the disc multiplied by the temperature-rise of 100 degrees or 200 degrees C., to the energy supplied to the hot-plate during the time interval taken by the disc to rise 100 degrees or 200 degrees C.

The results of these experiments bring out two important points. First, while the interposition of an air-gap by means of pegs makes the contact conditions reproducible, it has the disadvantage that the conditions are of a different character from these in actual practice, and this may operate unfairly against the hot-plates with machined surfaces compared with the radiant types.

The manufacturer of the flat-top plate assumes that

TABLE 1.—*Hot-plates.*

Copper Disc on Pegs 17·8 cm. (7 in.) diameter; 4·8 mm. (3/16 in.) thick. Weight, 1 073 g.

1 Plate		2 Time to Switching Off	3 Temp. at Switching Off	4 Mean Watts	5 Time to Rise 100 deg. C.	6 "Efficiency"	7 Time to Rise 200 deg. C.	8 "Efficiency"
		Min.	° C.		Min.	Per cent	Min.	Per cent
Open Type, A	(1) ..	8	246	1 400	4·2	12·0	7·2	13·9
	(2) ..	11	248	1 295	4·5	12·1	7·9	13·7
	(3) ..	9	247	1 085	5·4	12·1	9·3	13·9
Open Type, B	(1) ..	10	215	1 250	5·4	10·4	10·0	11·2
	(2) ..	10	211	1 250	5·6	10·0	10·2	11·1
Closed Type C, 7 in. Diameter	(1) ..	18	247	1 150	10·2	6·0	16·1	7·6
	(2) ..	18	214	993	11·1	6·4	17·9	7·9
Copper Disc 133 mm. (5·3 in.) × 4·8 mm. (3/16 in.). Weight, 596 g.								
Closed Rectangular Plate, 8 in. × 6 in.	(1) ..	6 min. (Plate in contact)	204	940	3·2	12·9	—	—
	(2) ..	6 min. (Plate in contact)	203	930	3·1	13·5	—	—
	(3) ..	14 min. (Plate on pegs)	200	910	9·0	4·8	—	—

NOTE.—The low values of the "efficiency" are accounted for by the fact that the receiving disc was of small thermal capacity, and consequently the heat absorbed by the hot-plate forms a considerable fraction of the total supply.

In the first experiment, with the copper disc in contact with the hot-plate, the surface was initially well polished. For the second experiment, it was left in contact with the hot-plate, and heated until the exposed surface was well oxidized. It will be observed that the oxidization has not affected the rate of rise of temperature, and consequently it may be presumed that the two surfaces were in sufficiently intimate contact to prevent oxidation of the receiving face of the disc.

For the third and fourth experiments the copper disc was raised with the three steel pegs, as in the case of the standard disc. The slight difference between experiments 3 and 4 is accounted for by the watts supplied not being exactly the same in the two cases, and the state of surface oxidation not being identical.

It will be observed that the time required to heat up to 200° C. was reduced from about 13 or 14 minutes to 6 minutes, by placing the surfaces in close contact. the utensils should have a flat bottom and would in practice make reasonably good thermal contact with the surface of the hot-plate. On the other hand, the maker of the open type designs his plate so as to obtain the maximum transfer of heat by radiation. The test should therefore be devised so as to take account of this essential feature of the plain type by providing the disc to be heated with a flat surface.

It is true that the thermal contact may not be equally good in all cases, but if the testing disc is flat to a higher degree of accuracy than that which is attainable for the hot-plate, the tests would discriminate in favour of the one which has the truest surface and is least liable to warp under the action of heat.

Secondly, the disc has a very small thermal capacity, being equivalent to only 0·18 pint of water.

Now, the electrical energy supply to the hot-plate is used partly for warming up the plate itself, partly

in heating the receiving disc, and partly to supply the loss by radiation, etc., so that with a disc of small heat capacity and for a given energy supply its rate of rise is largely dependent upon the thermal capacity of

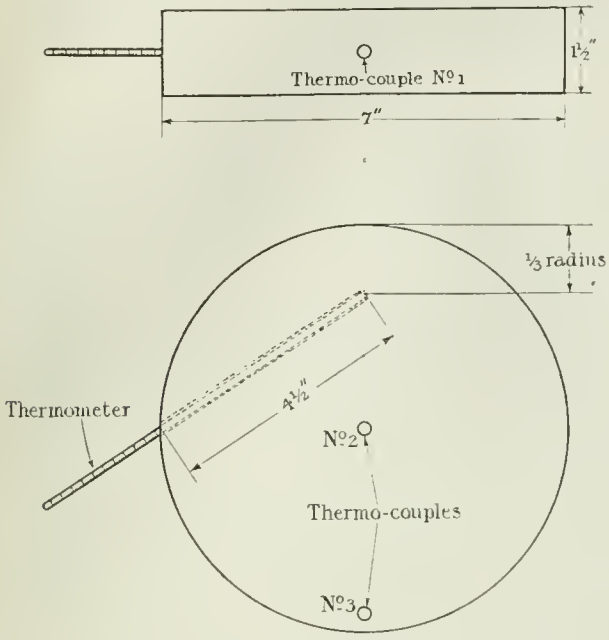

FIG. 4.—Standard testing disc.

the hot-plate. Such a test therefore gives a measure of the energy wasted in heating up the hot-plate to the working temperature, rather than its efficiency for normal working.

Experiments with thick discs.—Further experiments were made with thick discs. A plate of mild steel was used 18 cm. (7·1 in.) in diameter and 3·8 cm. (1·5 in.) thick, and one of the faces was machined on a surface grinder to an accuracy of ± 0·0005 inch. By means of thermo-couples it was ascertained that the uniformity of the temperature over a plane of the disc was quite satisfactory, and that therefore the mean temperature could be obtained by observations with a thermometer. As a mercury thermometer is probably more convenient for general use than a thermo-couple, provision was made for using the former. A hole was drilled in the plate (see Fig. 4) so that the bulb of the thermometer was at a point two-thirds along the radius from the centre. A little mercury was run into a hole to ensure good thermal contact between the thermometer and the disc, and, to prevent the mercury from running out, the stem of the thermometer was packed with asbestos thread. The heat capacity of this disc was equal to 1·6 pints of water, while a similar copper disc had a rather larger capacity. It was deemed advisable to clean the surface of the disc with polishing paste before an experiment and to rub the surface of the plate slightly with emery paper to remove rust and dirt.

Some typical results obtained with these discs are given in Table 2. As before, the "efficiencies" are taken to be the ratio of the heat absorbed by the disc to the energy supplied to the hot-plate. In the case of the efficiency for the complete run (col. 7), the ratio refers to the maximum temperature attained by the disc after the energy was switched off. This will naturally be greater than the efficiency during heating (col. 4), since the hot-plate will be at a higher temperature than the disc, when the energy is on, and, for some time after switching off, the heat will continue to flow into the disc.

Experiments with cast-iron vessels.—The experiments

TABLE 2.—*Hot-plates.*

Thick Steel Disc, 180 *mm.* × 40 *mm. Weight*, 7 413 *g.*

1 Plate	2 Time to Rise to 200° C.	3 Mean Watts	4 "Efficiency" during Heating	5 Maximum Temperature	6 Time to Reach Maximum	7 "Efficiency" for Complete Run
	Min.		Per cent	°C.	Min.	Per cent
Open Type, A (1)	24·75	1 470	29·3	214	29·0	31·3
Open Type, A (2)	25·58	1 420	30·3	215	30·0	32·4
Open Type, B	24·92	1 550	27·6	212	29·0	29·8
Closed Type (7 in.) C (1)	17·42	1 350	45·4	226	22·0	51·3
Closed Type (7 in.) C (2)	17·33	1 350	45·6	223·5	21·0	50·9
Closed Type D (1)	24·67	740	58·4	209	27·0	60·6
Closed Type D (2)	25·92	730	56·4	212	28·0	58·9
Closed Type (8 in.) E	20·92	1 450	35·1	223	29·0	39·2
Closed Type (12 in.) F	15·33	2 570	26·8	222·3	20·0	28·2
Closed Type (7 in.) Plate on Oven	23·38	1 020	44·3	216·7	28·0	46·3
Thick Copper Disc, 183 *mm.* × 42 *mm. Weight*, 9 500 *g.*						
Closed Type (7 in.) D (1)	24·50	760	59·7	204·0	28·0	61·4*
Closed Type (7 in.) D (2)	24·20	760	60·2	205·0	27·0	62·7

* The "efficiency" values with the copper disc are slightly higher than those obtained with the steel disc; this is due to the fact that the copper disc has a higher thermal capacity than the steel disc and consequently absorbs a larger fraction of the total energy supply.

described in this section were made with two cast-iron vessels 17·8 cm. (7 in.) and 20·3 cm. (8 in.) diameter respectively, supplied by the British Engineering Standards Association. The bottom surfaces had been machined up, but were not quite true.

Cotton-seed oil was used as the liquid chiefly because it had a low vapour pressure, so that the loss by evaporation would be negligible. Moreover, the oil could be raised to a temperature exceeding 200° C. It had the disadvantage, however, of being very viscous at low temperatures, so that thorough stirring by hand was impossible.

The loss by evaporation was determined and it was found that 986 g. of oil maintained at 150° C. lost 0·046 g. per minute. A similar experiment with water maintained at 90° C. showed a loss of 2·35 g. per minute.

The time/temperature curves for repeat experiments were not as concordant as those with the discs. This may be due to variable thermal contact between the bowl and the surface of the hot-plate, and to some extent to inefficient stirring.

Calculations of the efficiencies of the hot-plates have been made on the basis of the above experiments and are given in Table 3.

It is assumed that the bowl is at a uniform temperature throughout, and that the specific heat of the oil is 0·5.

TABLE 3.

Efficiency Tests with Cast-iron Bowls containing Oil.

1 Hot-plate	2 Diameter of Bowl	3 "Efficiency" for Full Run	4 "Efficiency" for first 100 deg. C. Rise	5 Temp. Interval (Full Run)
	In.	Per cent	Per cent	°C.
Open Type, B	8	33·4	29·8	15 to 146
		33·3	29·6	13 to 142
	7	31·8	27·4	16 to 148
Closed Type C	8	49·1	40·7	14 to 181
	7	45·2	35·5	15 to 179
Closed Type B_1	7	49·9	41·7	12 to 122
Closed Type D	7	53·4	42·7	17 to 134

CONCLUSIONS ON EXPERIMENTS WITH HOT-PLATES.

The experiments suggest that the following factors should be taken into consideration when framing specifications for hot-plates:

(1) The use of vessels containing water or other liquid for the purposes of testing hot-plates raises experimental difficulties arising from the necessity of stirring, the heat loss due to evaporation, etc. It seems preferable on the whole to rely on metallic discs of known thermal capacity for making the requisite tests.

(2) The disc should be of the diameter for which the hot-plate is designed.

(3) The apparent efficiency of the plate depends upon the mass of the disc, since a definite amount of energy is required to raise the temperature of the hot-plate alone. Hence it is advisable to employ a receiving disc whose thermal capacity is roughly of the same magnitude as the kettles or other utensils which will be used in practice.

(4) It seems necessary to assume that the major part of the heat transmission with the enclosed type of hot-plate is by metallic conduction, and consequently the test disc should have a plane surface.

It must be remembered that the efficiency determined with a clean disc with a true surface is the upper limit, and that utensils with enamelled surfaces, oxidized and irregular bases will give values which fall short of this limit. These defects will have much less influence with an open type of hot-plate where, in fact, a slightly oxidized surface should be advantageous. It might therefore be advisable to have one surface of the receiving disc plane polished and the other oxidized. The plane polished side might be used in contact with the enclosed types of hot-plates, while the oxidized surface might face the radiator when testing the open or radiant types.

(5) A disc test or tests might be used for defining the minimum requirements for hot-plates. Thus it might be laid down that for a certain energy consumption the hot-plate should raise the temperature of the disc to a certain maximum (say 200° C.) within a certain time, and that the energy required to maintain the disc at that temperature should not exceed a certain amount. The energy limits would, of course, be fixed relative to the effective area of the plate.

COOKING STOVES.

When specifying tests for cooking stoves, two distinct phases have to be considered. First, the heating-up period when the temperature of the oven is raised from that of the room to a certain maximum, say 200° C. (392° F.). Secondly, the cooking period when the temperature is maintained steady or allowed to fall slightly. The duration of this latter period may vary between wide limits according to circumstances. Hence the efficiency test of an oven should consist in the determination of (1) the number of electrical units required to bring the temperature of the interior to the maximum

within a certain period, (2) the energy required to keep it at the minimum temperature necessary for cooking operations.

In making experiments on these lines the measurement of energy is straightforward, but the temperature observations present some difficulty, on account of two inherent sources of error, namely, thermometric lag and non-uniformity of distribution in the oven.

for an oven taking 40 minutes to reach 200° C. the lag was only 5 degrees C.

While the light disc thermo-element left little to be desired as regards freedom from lag, it was considered that some method of obtaining the average temperature of the volume in the centre of the oven would be advantageous.

A spherical form was taken as being the most sym-

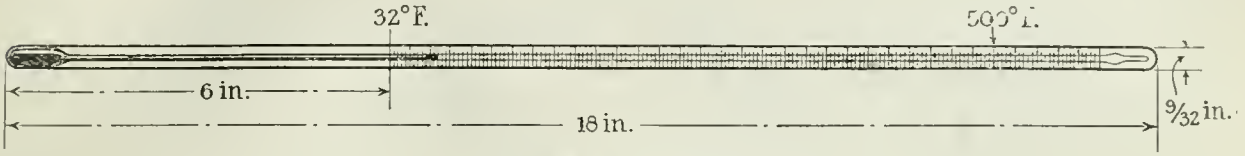

FIG. 5.—Standard testing thermometer.

Experiments on thermometric lag.—In the first instance thermo-elements of iron-constantan were employed with the hot junction formed of very thin sheets of oxidized copper-foil 5 cm. in diameter by 0·005 cm. thick. With these it was possible to measure temperatures at places inaccessible to mercury thermometers. Since the junction disc was extremely thin the thermometric lag of this form of pyrometer is practically

metrical shape for the envelope, and experiments were made using a hollow copper sphere with the two wires of the couple soldered to the top and bottom hemispheres respectively. The sphere, of course, formed the hot junction of the thermo-element, which consequently gives the average temperature of the space occupied by the envelope, assuming the copper walls to average out local irregularities by conduction. The results of

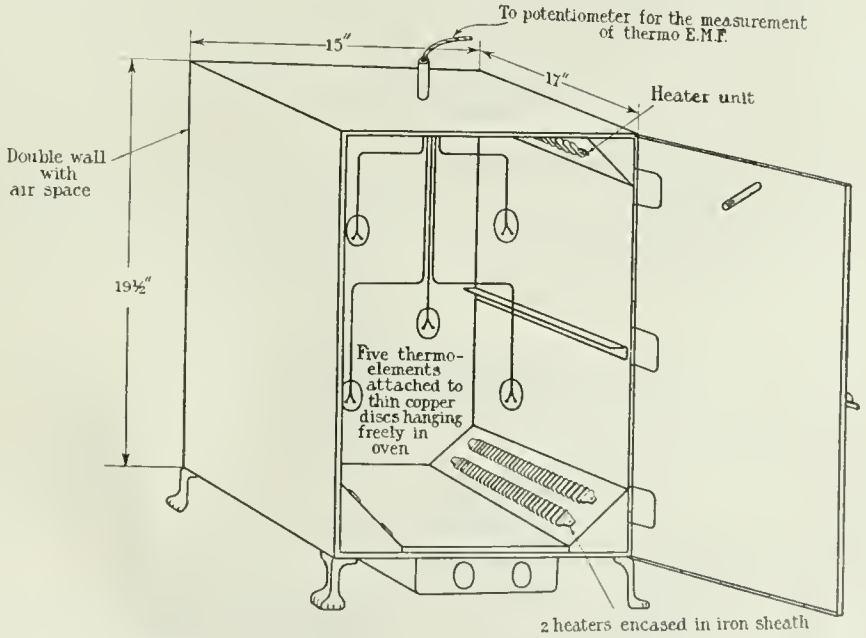
To potentiometer for the measurement of thermo E.M.F.
15″
17″
Heater unit
Double wall with air space
19½″
Five thermo-elements attached to thin copper discs hanging freely in oven
2 heaters encased in iron sheath

FIG. 6.—Relative disposition of heating elements and thermo-elements in oven J.

negligible, and the relative lag of mercury thermometers could be obtained by comparison. Thus, during the heating up of an oven simultaneous observations were taken with a mercury thermometer and a thermo-element whose hot junction was placed close to the bulb. The amount of lag depends, of course, on the rate of rise of temperature. In the case of an oven which took 12 minutes to reach a temperature of 200° C. from cold, the mercury thermometer lagged about 20 degrees C. behind the thermo-element, and

a comparison with a disc thermo-element showed that the sphere lags behind the disc by about 25 degrees C. in the case of an oven taking about 16 minutes to reach a temperature of 200° C. This lag could be reduced by making the sphere of thinner metal; the one used in the test weighed 99 g. and was 4½ inches in diameter. The indications of the spherical form are independent of the orientation, whereas with disc couples the indications are necessarily dependent to some extent on the angle which the faces of the receiving

discs make with the plane of the heating elements, owing to direct radiation.

For works' use the thermo-element is not so convenient as a mercury thermometer, since its adoption involves

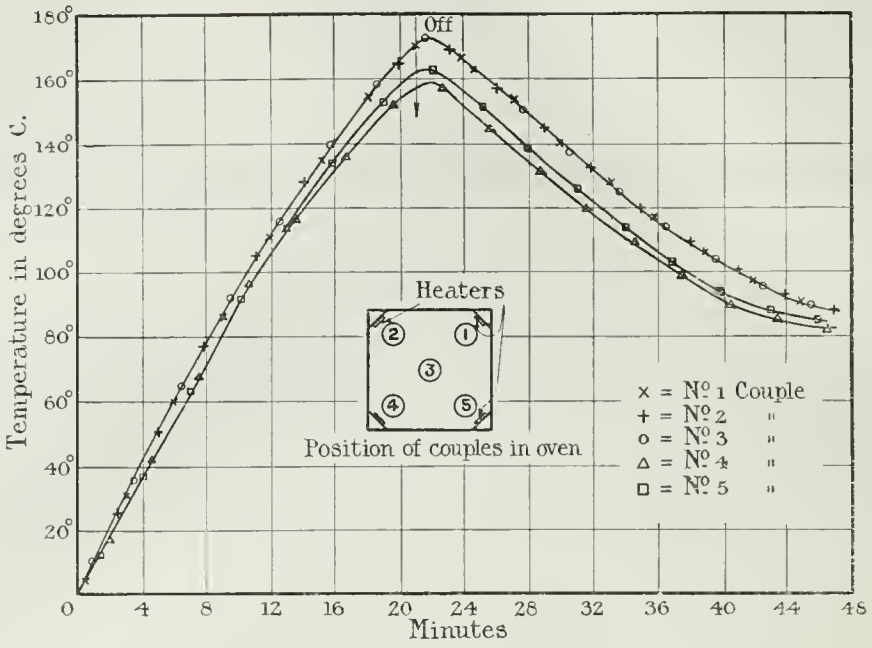

FIG. 7.—Distribution of temperature in oven J.

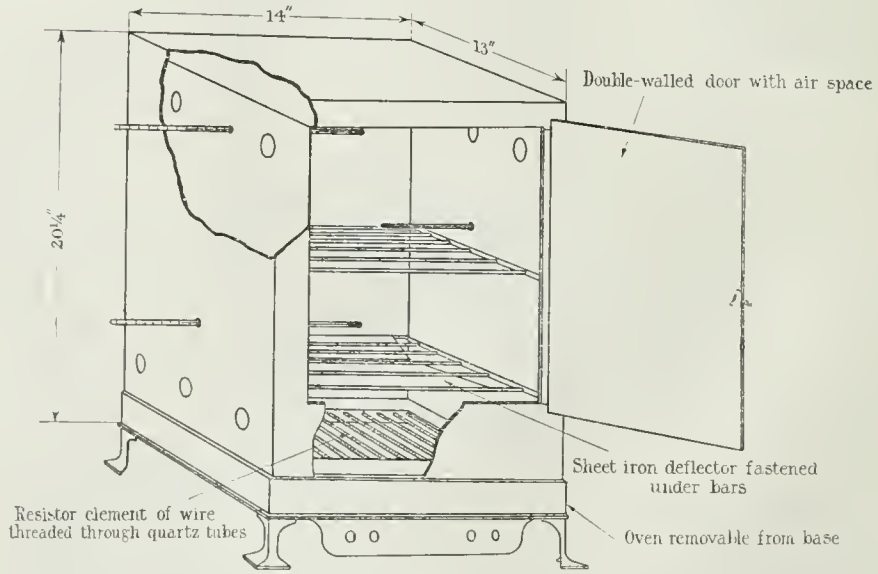

FIG. 8.—Relative disposition of heating elements and thermometers in oven P.

degrees. They were of the nitrogen-filled type. The relative dimensions of the thermometers specified will be seen from Fig. 5.

It was important to know whether changes of the use of a potentiometer and a sensitive galvanometer. Further experiments were accordingly made with mercury thermometers. Special thermometers were constructed, with the zero of the scale distant 6 inches from the bulb, which permitted of an immersion of about 5 inches into the oven. The range of the thermometers was from 0° to 250° C. (32° to 482° F.) divided into dimensions in the mercury thermometers employed had an appreciable influence on the results when the temperature was rising rapidly. It was found, however, that the differences obtained for the various sized thermometers commercially obtainable were almost negligible compared with the lack of uniformity in the oven. However, to ensure reproducibility the size of the

thermometer is specified, and it may be concluded if the specifications concerning the dimensions of the thermometer are adhered to, within the practical limits of thermometer construction, no appreciable error will be introduced into the observations by employing different thermometers.

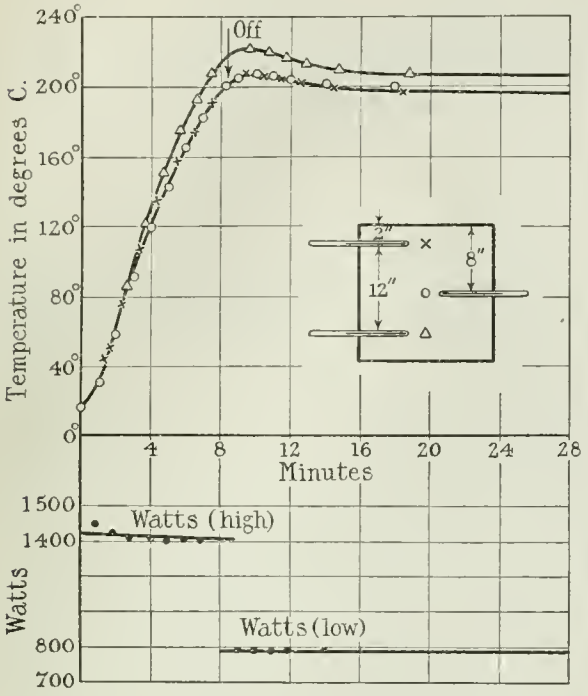

FIG. 9.—Distribution of temperature in oven P.

Distribution of temperature.—The distribution of temperature differs considerably in the different types of ovens according to the distribution of the heating units.

Examples of the temperature distribution in three different ovens are given in Figs. 6 to 11.

In the case of oven J the temperature observations were taken during the period of heating up and with the current switched off, and in the case of ovens O and P during the heating-up period, and during the period of steady temperature with the heaters at low heat.

It will be seen that no general rules can be laid down for the distribution of temperature and that it would be impracticable to attempt an integration to obtain the true mean temperature. It will probably suffice for comparative tests to take the readings given by a single thermometer projecting into the centre of the oven.

TESTS OF OVENS.

The procedure in making a test was to run the oven at "full heat" to the temperature desired and then to reduce to the medium or low heat by means of the switches provided with the oven. This corresponds more closely to what occurs in practice than the determination of the energy necessary for a series of equilibrium temperatures. The data obtained are summarized in Table 4, which shows clearly the different characteristics of the lagged and air-space insulation types of ovens. The lagged ovens, usually of heavy construction, require from 1 to 1½ units to raise the temperature to 200° C., but are very efficient when working at a steady temperature if account is taken of the cubic capacity in relation to watt supply.

Most of the ovens tend to remain at a very steady temperature when switched on to medium heat, ovens K, L and N being good examples. Oven M is rather an

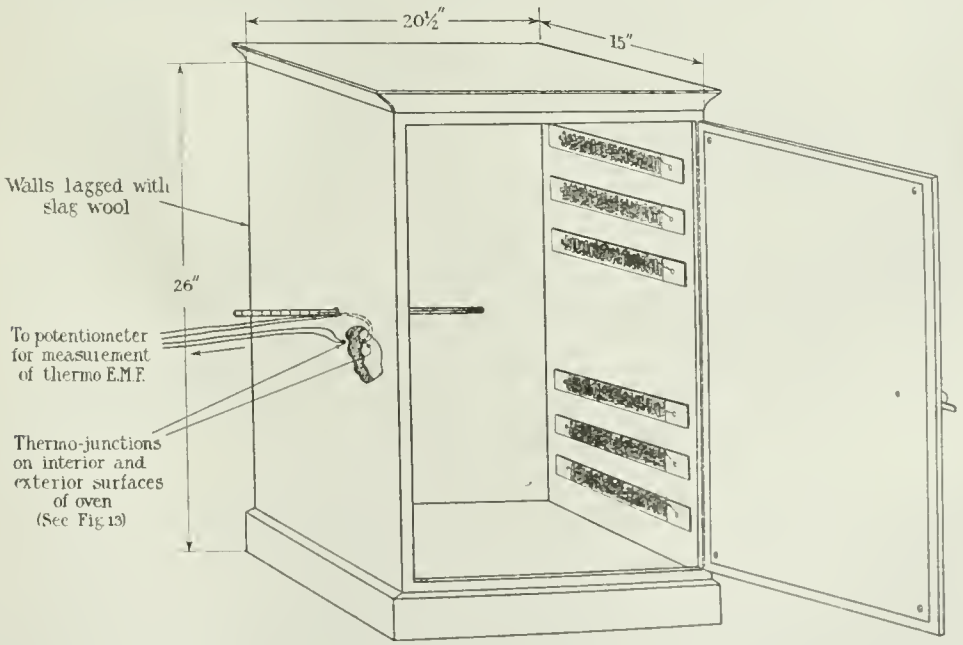
20½"
15"
Walls lagged with slag wool
26"
To potentiometer for measurement of thermo E.M.F.
Thermo-junctions on interior and exterior surfaces of oven (See Fig 13)

FIG. 10.—Relative disposition of heating elements and thermo-elements in oven O.

exception, as it shows a steady rise, while ovens O and J fall in temperature. In the case of oven P the bottom is the hottest region. That this should be so is clear from the construction, since the heater is contained in a tray beneath the oven. Owing to the diversity of type it is impossible to make any deduction as to the relation between the size of the oven and the watts required per cubic foot.

a period of half an hour from switching down to medium heat.

(4) The method of stating efficiencies in relation to cubical contents.

NOTE ON THERMAL INSULATION OF OVENS.

The thermal efficiency of an oven is largely a question of providing suitable insulation, and it is thought,

TABLE 4.

Summary of Data on Ovens.

1		2		3	4	5	6	7		8	9
Type of Oven		Time taken to reach 205° C. from Switching On		Average Watts	Total kWh to Raise to 200° C.	Approx. Cubic Capacity	kWh per c. ft.	Temp. after following Time from Switching Down		Watts for Temp. under Col. 7	Watts per c. ft. on Medium Heat
								15 min.	30 min.		
		Min.	Sec.	Watts	kWh	m³	kWh	° C.	° C.	Watts	Watts
Ovens of Light Construction and Air-gap Insulation	K	21	21 top heaters	570	0·203	0·58	0·350	199	199	360	621
		14	10 bottom heaters	785	0·185	0·58	0·319	205	204	360	621
	P	8	18	1 410	0·195	0·88	0·222	198	195	790	898
	J	24	12	1 210	0·488	1·91	0·256	197	188	655	343
	H	36	50	1 790	1·100	1·88	0·585	170	145	550	293
	L	19	27	1 990	0·645	1·55	0·416	169	165	810	523
Ovens of Heavy Construction, Lagged	M	33	50	2 275	1·283	2·06	0·623	214	225	1 350	655
	O	43	16	2 065	1·489	2·14	0·696	200	186	820	383
	N	24	27	2 590	1·055	2·71	0·389	197	200	710	262

NOTE.—The temperatures are those of the centre of the oven in each case.

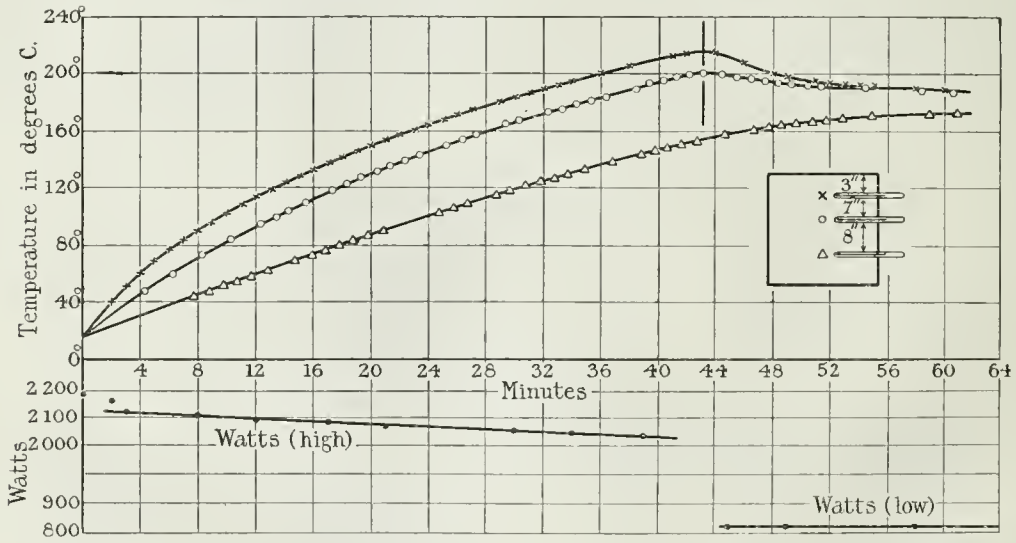

FIG. 11.—Distribution of temperature in oven O.

CONCLUSIONS.

In drawing up specifications for tests on ovens the following points require consideration:

(1) The maximum time permissible for the oven to heat up to a certain temperature (say 200° C.) from room temperature for a certain watt supply.

(2) The variation in temperature from top to bottom.

(3) The permissible variation in temperature during

therefore, that a brief note on the theory of the subject may not be out of place.

The transfer of heat from the hot air inside an oven to the outside cold air and surroundings can be considered in three stages:

(1) *Transfer of heat to the inside wall of the oven.*—The heating units will dissipate heat partly by convection and partly by radiation. Some of the latter

will generally be intercepted by objects in the oven, and will not fall directly on the walls. It is probable, therefore, that convection will account for a large proportion of the heat transfer.

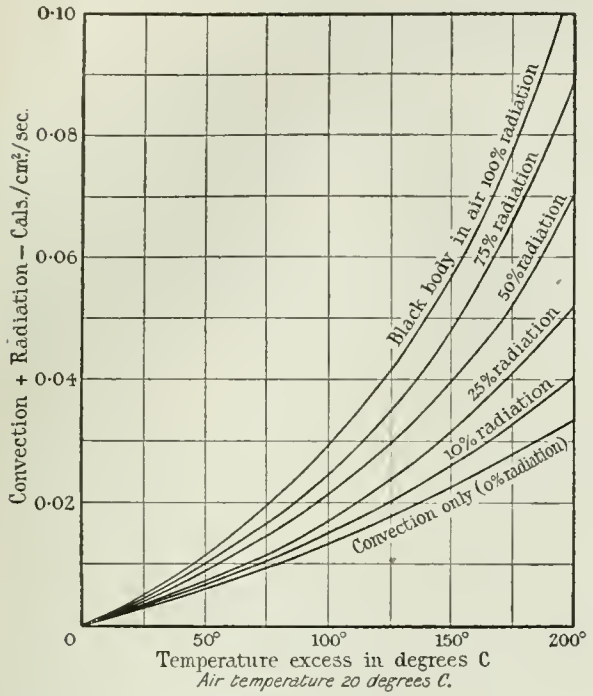

FIG. 12.—Total losses from heated surfaces in air.

(2) *Transfer of heat from the inside wall to the outside wall of the oven.*—In the case of lightly constructed ovens it is a common practice to have merely air between the walls, and the transfer will then take place by convection and radiation. In heavy ovens the space is usually packed with some insulating material through which the heat will pass by conduction.

(3) *Transfer of heat from the outside wall.*—This will take place by convection through the air and by radiation to the surroundings. Referring first to convection, the laws governing it are extremely complex and have not yet been fully investigated. Thus the heat loss from a flat surface will depend upon its orientation, and whether it is freely exposed to the air or boxed in. Taking, however, a freely exposed vertical surface as being, on the whole, most typical of oven walls, the curve connecting convection loss and temperature difference will be approximately that shown in Fig. 12 (see bottom curve).

The law governing radiation has already been referred to, and the remaining curves in Fig. 12 give the total heat loss for surfaces of various emission coefficients. Thus the curve marked 75 per cent radiation gives the sum of the radiation loss, which is 75 per cent of that of a black body, and the convection loss as shown on the bottom curve. The convection loss is, of course, the same for all these surfaces.

These curves illustrate the importance of employing surfaces of low radiating and absorbing power. Thus, for a temperature excess of 100 degrees C. above the surroundings, the heat loss of a surface with a 10 per cent coefficient will be only about one-half of that of a black body. To give some idea of the range of coefficients for common materials the following rough values may be given. Paints * of all colours and surfaces (from matt to enamel) 80 to 90 per cent, oxidized metal surfaces 50 to 90 per cent, dull metal surfaces 20 to 40 per cent, polished metal surfaces 3 to 10 per cent. It is, of course, out of the question to use highly polished metal surfaces in oven construction, but the

* A metallic paint such as aluminium paint is an exception, having a coefficient of about 50 per cent.

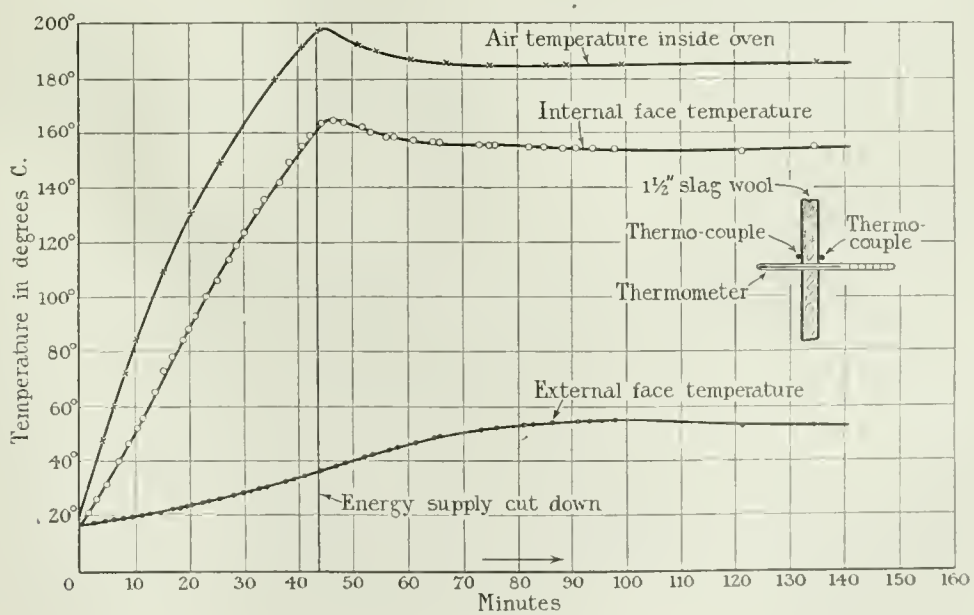

FIG. 13.—Temperature curves for air in the oven and for the interior and exterior walls.

use of clean unoxidizable surfaces often adds considerably to the efficiency. Some typical cases of oven insulation can now be taken.

Ovens with thin solid wall (generally metal).—Let t_1 and t_4 be the inner and outer air temperatures, t_2 and t_3 the temperatures of the faces of the wall, which is taken to be of thickness x and conductivity K. The rate of heat flow per sq. cm. of wall will be given by

$$H = E(t_1 - t_2) = \frac{K}{x}(t_2 - t_3) = E(t_3 - t_4)$$

If t_2 and t_3 are eliminated from these equations we get

$$H = \frac{t_1 - t_4}{2/E + x/K}$$

Now in this expression E is the rate of heat emission (by radiation and convection) for a difference in temperature between the surface and the surrounding air * of 1 degree C. Taking a clean metallic surface of emission coefficient 20 per cent, E will be of the order 0·00016 calorie per sq. cm. per sec. per degree C. Now K for most metals will be greater than 0·1 C.G.S. units and x will be a fraction of a centimetre. So that x/K will be negligible compared with $2/E$. Hence the heat loss will be governed entirely by the surface "resistance" $2/E$, and its value will be

$$H = \tfrac{1}{2}E(t_1 - t_4) = 0{\cdot}00008(t_1 - t_4)$$ calorie per sq. cm. per sec.

Oven with double metal wall with air-gap.—By the same reasoning the heat loss can be shown to be

$$H = \tfrac{1}{4}E(t_1 - t_4) = 0{\cdot}00004(t_1 - t_4)$$

or about one-half of that of the single wall. As a matter of fact it will be rather less than this, for the convection loss between two parallel faces "boxed in" at the top and bottom is considerably smaller than for a plate with free access of air and the same temperature difference.

Oven with two metal walls and insulating material between.—The heat loss in this case is given by

$$H = \frac{t_1 - t_4}{2/E + x/K}$$

E having the same meaning as before and x being the thickness of the insulating material of conductivity K. For a good material the value of K will be of the order of 0·00015 C.G.S. units.

If now $x = 2$ cm. the heat loss will be about

$$H = \tfrac{1}{4}(0{\cdot}00015)(t_1 - t_4) = 0{\cdot}00004(t_1 - t_4)$$

or the same as for the double-walled oven with air-gap.

* Fig. 12 shows that E is not a constant, but that it increases with increasing temperature difference. Also for the same temperature difference but with both temperatures higher, E will be greater. In these approximate calculations E is taken as constant and the same for inner and outer surfaces, and the value used is that for a difference of 100 degrees C.

If, however, $x = 5$ cm.

$$H = \frac{0{\cdot}00016}{7}(t_1 - t_4) = 0{\cdot}000023(t_1 - t_4)$$

or a little more than one-half of that of the double-walled oven with air-gap.

The above illustrations give some idea of the factors affecting the transference of heat through oven walls when the steady state has been reached, such as in an oven run continuously. But this is not often the case in practice. It is more common for an oven to be switched on from the cold, used for some cooking operation, and then allowed to cool again. In these circumstances the heavily lagged oven, although more efficient in continuous running, may be at a disadvantage. Its greater heat capacity will cause energy to be wasted in heating up the mass, and it will take a longer time to reach its maximum temperature. These tendencies

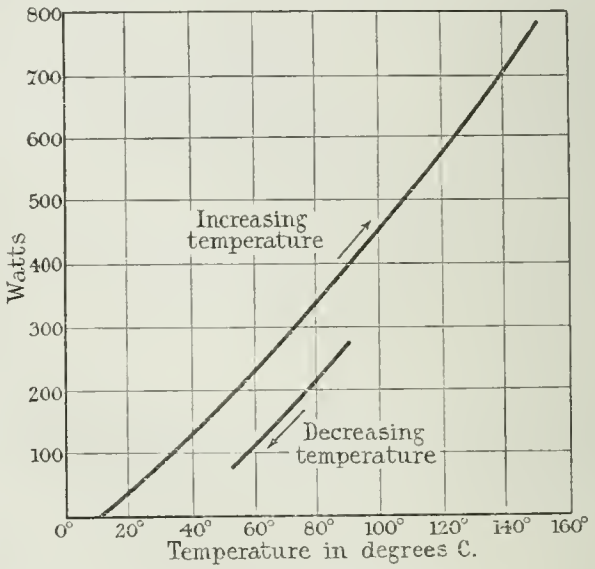

FIG. 14.—Temperature/load curves.

This oven has a lagging of slag wool, and requires a long time to attain equilibrium. In the experiments one hour was allowed to elapse before taking the temperature after setting the energy supply to a definite value.

are illustrated in Figs. 13 and 14. Fig. 13 gives the temperature curves for the air in the oven, and for the interior and exterior walls. It will be seen that, while the air and internal wall reach their maxima in less than 50 minutes, the exterior wall took over 100 minutes to reach its steady condition. This means that, for the last-mentioned period, energy is still being absorbed in heating the walls of the oven. Fig. 14 shows the curve obtained when heating up the oven and determining the watts required to keep it steady at a series of ascending temperatures, and also the curve obtained for a series of descending temperatures. The difference between the curves is accounted for by the fact that, for the ascending part of the cycle, heat is flowing into the walls when the air in the oven is kept constant, and vice versa for the descending part.

DISCUSSION ON

"ELECTRIC APPLIANCES FOR DOMESTIC PURPOSES."

BEFORE THE INSTITUTION, 10 FEBRUARY, 1921.

The discussion was opened with an introductory paper by **Messrs. E. Griffiths, D.Sc.,** and **F. H. Schofield, B.A., B.Sc.,** entitled "Some Thermal Characteristics of Electric Ovens and Hot-Plates" (see page 361).

Mr. J. W. Beauchamp : It seems to me that an investigation such as that described in the paper is of great value to designers and manufacturers. In the old days of electric cooking we generally obtained our observations from the consumers. I was very much impressed by certain points in connection with the authors' tests ; there is no doubt that to-day the difference between the efficiency of the best and the worst electric cooking apparatus is not great. There may be a difference in reliability, finish and price, but little actual difference in efficiency. The most important point is the efficiency of the operator, the uninstructed person who is going to cook with the apparatus. The authors' remarks with regard to radiant losses from hot-plates are very suggestive to me. What they mean is that to place a small pot on a big hot-plate gives rise to an excessive loss. Similar cases occur in connection with electric ovens, and it is necessary to teach the art of switching and bear in mind that generations of cooks, male and female, have been accustomed to apparatus using ordinary fires for cooking. With such apparatus an extraordinarily small percentage of the total heat of the coal finds its way into the food being cooked. If the oven is a little too hot nothing much can be done to regulate the fire or draught, so the oven door is opened, or, if the pot boils over, the pot is pulled to one side. I know from my own experience that this is what happens in electric cooking. Much trouble and effort is spent in getting a highly efficient electric cooker, and many switches are fixed, but they are not used. The great need at the present moment is for continual demonstration of these points, as we have to deal with the present generation of cooks who for years have been accustomed to work with a source of heat beyond their control, and always warmed things by putting them on the fire and cooled them by taking them off. We have to develop the switching habit. Finally, in looking at the oven commercially we must not lose sight of the fact that the use of the stove for roasting purposes represents only a very small fraction of the total cooking. Our great need is to provide an efficient means of boiling, and we shall have to develop apparatus more on the lines of the immersion heater. It seems to me that if one could produce such a heater with a stream-line or pear-shaped body which could be easily lowered into the pots through their lids, and of such a shape that it could be easily cleaned, a great advance would be made. I think that investigation along those lines would be very useful and that the development of the immersion idea would take away a great deal of work from the ordinary boiling-plate which, when it is used for boiling operations, is operating at its worst.

Mr. H. H. Berry : I think the paper does not admit of criticism ; it is really a record of facts ; but there are other facts which I should like to see added. The authors have, in my opinion, something at the back of their minds in their concluding remarks, where they say : "It is more common for an oven to be switched on from the cold, used for some cooking operation, and then allowed to cool again." It may be that they were thinking of bakers' ovens, which are in almost continuous operation, or of a method upon which I myself have been working for the last few years. My scheme approaches the whole question from an entirely different point of view, which is that we must not attempt, either in the heating of our homes or in the cooking of our food or in the heating of water required for domestic purposes, to rely upon any secondary source. When I speak of secondary sources I include gas equally with electricity. My belief is that to approach this matter on a commercial scale we shall have primarily to get the best value from the British thermal units that are to be obtained in the original fuel, so long as we depend on the original fuel for our present source of heat. Accepting that as my standard, my scheme has been to evolve a method wherein I can very economically produce in the home a furnace by means of which, burning less fuel than would be consumed in an ordinary domestic range, one can provide a standard degree of heat for the home and, using the same furnace, provide a constant supply of hot water which will vary in temperature from about 130° to 180° F., according to the draught in the furnace. We all have a knowledge of central heating as it has existed in America, in Canada and in Europe, and have been prepared to accept the average Englishman's statement when he has come back from those countries—that central heating is very efficient and economical but too hot and stuffy, and that the burnt dust affects the throat. In my opinion there are two reasons for this, one being a particularly practical and simple solution of the proposition : the other reason being psychological. The American heating engineer specifies that the plant which he installs must be capable of maintaining the temperature at a minimum of 73° F., but it generally rises to 80° or 85° F. The remedy, in my opinion, is to produce with a plant half the size an atmosphere that would have a maximum temperature of 55° to 60° F., requiring only half the usual consumption of coal during 24 hours, and that the fuel saved could be replaced by electricity to give radiant heat. I apply the same argument to cooking.

I run a tapping from my furnace and always keep the temperature of my oven at the temperature of the heating system. I never have a cold oven or cold plates to heat up, and by the use of this system I require only about two-thirds of the electricity or gas at present used to enable cooking operations to be performed in about two-thirds of the time at present taken. I should be very pleased to show this system under working conditions to anyone sufficiently interested.

Mr. W. A. Gillott: This paper gives the results of specific tests and demonstrates definite facts; one is therefore more or less forced to agree with the conclusions at which the authors have arrived. The question of how these tests were conducted, however, is entirely a different matter, and the methods employed open up a series of discussions. These tests indicate a large amount of careful work on the part of the authors. I would suggest standard names for the component parts of the appliance, such as "boiling plate" for "hot-plate," and "oven" for "cooking stove." I consider that the name of a part should imply its function. In testing the boiling plates I note the temperatures were recorded from a copper plate supported 2 mm. above it. I agree that such a test will give a comparison between different makes of plate, but would it not have been of greater value to have endeavoured to boil or heat a definite quantity of liquid in a definite time, thereby securing a more accurate efficiency in the commercial sense, and to imitate as near as possible the actual working conditions? Cooks are beginning to realize that to secure the best results from their boiling plates they must use utensils with machined bottoms. Undoubtedly the authors have some reason for employing copper and steel plates. To state that a plate has an efficiency of 12·9 per cent under the B.E.S.A. standard is quite sufficient for an engineer who understands how this efficiency is arrived at, but to the buying public it does not convey much information; they would better appreciate the statement that an 8-inch boiling plate complies with the B.E.S.A. standard and will boil a definite quantity of water in 10 minutes at full heat and, if used in compliance with certain accepted principles, the plate under ordinary working conditions should enjoy a long life. The tests on ovens are extremely interesting and demonstrate the varied results obtained by placing the heating elements in different positions in the oven, but they do not indicate the best position in which to place one's elements in order to secure the best cooking results in the domestic oven. Let us take the case of oven J. The position of these elements may produce good heating results, but they occupy valuable space and reduce considerably the effective area of the oven; judging from the figure I should imagine that about 4 inches are lost at both the top and bottom. The elements are also fitted at the bottom of oven D. Here also space is given up and, further, elements at the bottom of an oven are often a source of trouble, owing to the spilling of fat. I think that oven O embodies the correct system of heating for a domestic oven where all classes of cooking must be catered for. No reference is made in the paper to the practical utility of the oven. An oven to be useful must possess that qualification of quick heating up, as no matter how one may impress upon the cook the advisability of keeping the oven door closed, the majority of them seem to delight in continually opening it, thereby losing heat. Many engineers are of the opinion that a lagged oven must necessarily take a long time to heat up. I suggest it is merely a question of design. The company with which I am connected supply as a standard article an oven with a well-constructed interior, carefully lagged, having a capacity of 1·6 cubic feet, and attaining a temperature of 350° F. (the practical cooking temperature of an oven) in 18 minutes at an expenditure of 400 Wh; this compares very favourably with the results mentioned in the paper. Some very useful figures are given relating to the temperature between the interior and exterior walls of one oven under test. It would be interesting to know the result of a similar test on one of the unlagged or light ovens, as the figures should show the value of a lagged oven. I do not see any reference as to the particular type of heating element recommended, but I should like to give my own opinion upon this matter. For ovens, grills and open-type boiling plates the element I recommend is one with the heating wire wound in spiral form and mounted upon a fireclay former, and so fitted that in the event of a failure the spiral only is needed and not the complete element as is usually the case. It must also be so connected that the failure of one element will not prevent cooking being carried on. The fireclay portion must also stand being made red hot and plunged into cold water without being fractured. Such an element gives satisfaction under severe conditions of wear and is appreciated by both users and engineers who have the care of their maintenance. I should like to call attention to three particular points: (1) That electric cooking and heating, possessing a high diversity factor, is a desirable load for the central station, brings in a good revenue, and in the majority of cases occurs when the plant is capable of handling it. (2) To remember, when discussing cooking with a prospective client, or designing a new tariff, that the comparative cooking cost of gas and electricity is 1d. per unit against 2s. 6d. per 1 000 cubic feet. (3) The average consumption for middle-class homes, catering for all meals, is 1 unit per person per day when four and over are provided for, or from 1 to 1½ units per person per day when less than four are provided for. Above all, electrical engineers should use it in their own homes; one would then learn more about electric cooking in three months than by many debates.

Mr. Haydn T. Harrison: There are one or two points that the authors have taken for granted; for instance, that the usual square-type oven similar to a gas oven containing a certain number of shelves is always used. The oven before that was the kitchener oven, then the bread oven which is still used by bakers in the country, while a hole in the ground was probably the original oven. Many of these had a very much smaller ratio of cooling surface for their contents than the square oven, but I notice that nobody has ever taken advantage of this fact. Another noticeable fact is that ovens are nearly always made of a material which convects heat, and either air or some other heat

insulator is left in between the inner and outer casing. I have used an oven in my house for years, and it has no metal whatever in the construction of the case, and consequently has a somewhat higher efficiency than metal ovens. The figures given in connection with oxidized and bright surfaces are very valuable, but it must be remembered that at present we are passing through a transitory stage. People sometimes cook on gas or coal fires and use the same utensils for electric appliances; thus we find the bottoms of vessels not oxidized but carbonized, and I should like to know what is the effect of the carbon on the bottom of the vessels. Mr. Beauchamp brought forward an important point. Unless a certain amount of instruction is given with these new electric appliances they are likely to be wrongly used. Electrical appliances do not entirely depend, so far as value to the world is concerned, on economy alone. The special feature that requires to be brought home is the convenience rather than the economy of electricity From a propaganda point of view this is, in my opinion, a great deal more important than the fact that one type of electric oven takes 10 per cent less watts than another. Once it becomes known that establishments can be worked more easily and with less friction by means of electricity than by means of gas or coal, the question of small details of efficiency will not arise. I hope it will be possible for electric stoves to be loaned for a certain period during which people can learn by actual experience the value of electric cooking apparatus, not so much from the point of view of economy as from that of convenience. I should like to endorse Mr. Beauchamp's remarks on the art of switching. In a case which has lately been brought to my notice, a cook who had been accustomed to a coal cooker, on being put in charge of an electric cooking range, put every switch on day after day in order to heat the kitchen; the subsequent bill for electrical energy nearly resulted in the removal of all electrical cooking and heating appliances from that establishment. This could be prevented if a little instruction in the art of switching were given to consumers when they take over an installation.

Mr. W. R. Cooper: There are two types of tests on hot-plates, namely, (1) starting from cold, and (2) continuous running. Both seem desirable, more particularly the second. In the first type, a hot-plate with low heat capacity is desirable, and here the open hot-plate should be at an advantage; efficiency is not so important. Ample power is essential, as an unduly long time in bringing water to the boil is irritating to the user. In the second type of test the efficiency of the hot-plate is the most important characteristic. Although it is not directly stated, I gather that the authors' tests belong only to the first type. I am not in favour of using a metal disc on pegs. Such a method has, of course, the advantage of securing uniform conditions, but it alters these conditions very materially from those obtaining in practice. In actual cooking, a saucepan containing water is the most usual object to be heated. Consequently, the surface to be heated is maintained comparatively cool and the chief temperature gradient is at the surface of contact of the saucepan and hot-plate. A disc on pegs introduces a considerable layer of air, which is a poor thermal conductor, between the hot-plate and the body to be heated, so that a closed hot-plate must become much hotter. Radiation then plays a larger part, a closed plate being partly converted into an open hot-plate. Thus a fair comparison of closed and open hot-plates cannot be expected. If a metal disc is to be used I should prefer to have it placed directly upon the hot-plate. There is then, of course, the question of accuracy and quality of surface. Here the authors appear to have attempted conditions which are too ideal. Is it of any use to exceed the mechanical standard of the closed hot-plate? This is not likely to be true to $\pm$ 0·0005 inch. If small air-spaces exist these will result in those parts being extra hot, but the temperature will be more or less equalized by conduction. Then again, is there a serious advantage in having a polished surface for the disc? A corresponding surface will not be obtained on the hot-plate. Suppose the iron disc and hot-plate both have an oxidized surface which is rubbed clean (not a rusted surface); will the conditions so obtained vary to an extent beyond the other errors of observation? If not, these conditions would be more practical and less troublesome. I should prefer, however, to avoid a metal disc altogether, because it departs from practical conditions. The temperature of the contents of a saucepan will not much exceed 100° C., whereas that of a metal disc may be 200° C. or more, and this will affect the working of the hot-plate. Nor do I care for oil, for the same reason. Another objection to a metal disc is that the thermal capacity is low and therefore the test is short. A true efficiency test can scarcely be expected in 6 minutes. Any method probably has its objections, but I think I should prefer a saucepan containing water, the sides of the saucepan being lagged, to any metal disc. A much longer test would be obtained, and if any water is boiled off this could be measured. It would be preferable, however, to avoid boiling by having the saucepan sufficiently large, the top then being lagged in addition. I fail entirely to understand why the authors have adopted a basis for efficiency which not merely includes the thermal loss from the hot-plate but also the radiation and other losses from the heated disc. Surely the efficiency should deal only with losses from the hot-plate itself. The authors' own results, which are contradictory, appear to condemn the basis adopted. If the metal discs were lagged on top and sides a different efficiency figure would be obtained by the authors, but this would not be due to the hot-plates, and the true efficiency would be unaltered. It seems to me that efficiency tests should only be carried out on continuous tests, not on starting the hot-plate from cold. The fundamental differences between the closed and open hot-plates are often not sufficiently understood. Closed hot-plates depend only on conduction and they must necessarily cover a considerable area. Since open plates depend only on radiation there is no objection to concentrating them on to as small an area as is practicable; in fact it is an advantage. On the other hand, the rays from an open hot-plate may be readily reflected by the object to be heated so as to cause useless heating. Fig. A will make this point clear. This is more par-

ticularly the case when using bright utensils. I do not understand why open hot-plates are less efficient than the closed type in Table 3 and more efficient in Tables 1 and 2. Is this due to the lowering of efficiency by reflection? With regard to ovens, I should like to ask whether the reading of the thermometer is affected when radiant elements are used. It is, of course, preferable to obtain the temperature simply by the

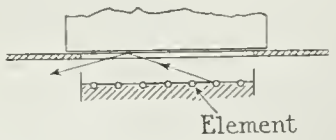

FIG. A.—Section of open hot-plate, showing loss by reflection.

reading of one thermometer, but if the design of ovens is altered this will probably lead to errors in testing. Would not the average temperature have been obtained fairly simply by the use of a vertical frame, placed centrally, carrying resistance wire, and measuring the resistance on a deflectional ohmmeter? The rapid heating up of an oven is most important. The oven which I use heats up to 350° F. (by thermometer) in 10 minutes. It is desirable, however, that the reduced input should be able to maintain the temperature, when once reached, at 250° to 300° F. without frequent attention. In Fig. 12 the authors give some very useful information. As to whether an oven should be lagged or merely provided with an air space depends upon the size and use of the oven. The loss from a purely unlagged oven is due simply to radiation and convection. In the case of a lagged oven there is,

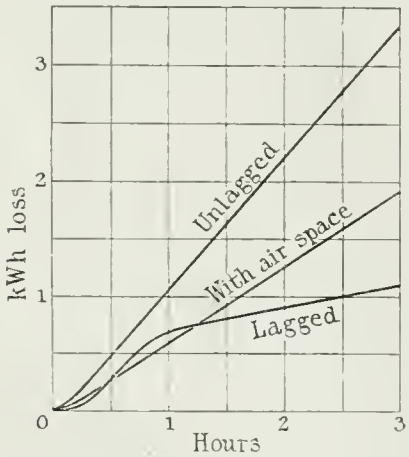

FIG. B.—A particular case of loss in lagged and unlagged ovens.

in addition, a loss due to absorption. The oven which I use is 15½ in. wide by 15½ in. deep and 23 in. high internally. The lagging is about 2 in. thick. Assuming a working temperature of 300° F., an outer temperature of 100° F. when lagged and 250° F. when unlagged, the losses may be approximately calculated. At the end of one hour from switching on, the radiation and convection loss from the lagged oven will be about 200 watts. If the oven is unlagged the total surface is smaller, but the rate of loss is about 10 times as great, so that the corresponding loss is about 1 150 watts. The water equivalent of the lagging and casing is, say, 14 lb., taking the specific heat of slag wool as 0·2, and its mean temperature-rise as 150 degrees F., so that the heat absorbed is equal to about 600 Wh. At the end of one hour, owing to the fact that the radiation and convection loss does not reach its full value at once (particularly with a lagged oven), the losses might be, say, 600 + 100 = 700 Wh for the lagged oven, and 1 050 Wh for the unlagged oven. Usually an oven is required for more nearly 2 hours than one, in which case the lagged oven shows to greater advantage, and the absorbed heat may be useful for a later operation. The difference is shown in Fig. B. Obviously no general rule can be laid down, and if air-space insulation is adopted the result is more favourable to this type than shown for the unlagged oven. Taking 1·62 as the thermal-resistance value for the air space as against 1·00 for an unlagged surface (as found by A. E. Kennelly, F. D. Everett and A. Prior) a third curve is obtained. The latter serves to emphasize the importance of the time factor. The water equivalent of the lagging should be brought down as far as possible, and if a cellular form of slag wool could be devised it might prove very advantageous in bringing down the heat-absorption loss.

Mr. P. Good: There is only one criticism I should like to offer and that is with regard to the authors' use of the term "efficiency." I am of the opinion that the term "heat absorption ratio" might be used in the paper instead of the word "efficiency." With regard to the method of testing hot-plates and ovens described by the authors, I think it might be interesting to state briefly a little of what occurred before that work was carried out. A Committee of the British Engineering Standards Association was appointed to draft a Standard Specification for electric cooking apparatus. They carefully considered the question of testing the apparatus by means of boiling a certain amount of water, and many other methods. They found difficulty in suggesting a method by which the results could be satisfactorily repeated, as has always been the case in making tests with this class of apparatus. The method finally adopted was proposed by Mr. Wordingham. As a result, this experimental work was carried out to enable the Committee to draft a few of the clauses of the Specification. There are other domestic appliances besides cooking apparatus, and I think it should be generally known that the same care and attention is given to the preparation of standards for other apparatus. There is, for example, such a thing as a British standard wall plug, but very few people take the trouble to see that they get standard wall plugs. I feel convinced that progress in the use of domestic appliances will be found to be very closely associated with the standardization of those appliances. I disagree considerably with Mr. H. H. Berry, who suggested that electric heating should be a subsidiary form of heating, but I think we shall very much modify our methods of heating. We feel cold on entering a room because our bodies are radiating heat to our

surroundings, and it may be that before long we shall have wall-papers woven with copper wire maintaining the surroundings of the room at something approaching blood heat, which will prevent us losing our heat. This seems to me a much more sensible way of keeping warm than to lose heat at the back and receive it again in front.

Professor E. W. Marchant : I should like to refer to the question of efficiency, which has been mentioned two or three times by different speakers, as it seems to me that the method the authors have adopted of testing a hot-plate by the rate at which it heats up an iron disc is rather an unfavourable method from the point of view of the hot-plate. It gives a rather lower efficiency than might otherwise be obtained. As Mr. Cooper said, the test is really one not so much of the efficiency of the heater but of the rate at which the energy is radiated away, and, in estimating the effectiveness of the hot-plate, account must be taken of the rate at which the heat is radiated from the top. I raise no objection against this method for getting relative figures, but it seems to me that the "efficiencies" arrived at are unnecessarily low. In specifying a test for obtaining comparative results, I think it would be better to have one which puts electrical appliances on a more favourable footing. For instance, I asked two of my students to make some tests of a hot-plate of the ordinary open radiant type, for which the efficiency given in the paper was about 30 per cent. (In one case, for a thin plate it was 13·9 per cent, and in another it was 29 or 30 per cent.) These two values, in themselves, are an illustration of what I mean, giving with the thin plate a very much lower apparent efficiency than with the other. An ordinary copper kettle was half filled with water, and some tests on the actual efficiencies for heating were made. When the kettle was polished on the lower surface the efficiency was 32 per cent when half full, and went up to just over 40 per cent when it was quite full. Then some lamp-black was put on the bottom of the kettle, with the result that the efficiency when the kettle was half full rose to 40 per cent and when it was quite full to 52 per cent. I think that the authors should have endeavoured to arrive at a figure which fairly represents the performance of the heating apparatus, and I hope that in any further work which they undertake they will bear that point in mind.

Mr. C. G. Nobbs : The details as to how the tests, as described in the paper, were made, are perfectly clear and I feel that they are trustworthy. With regard to the testing of the "closed" and "open coil" type of plates, in my opinion the tests should be made by heating up water with, say, three consecutive "boils" from cold, thus obtaining a good average efficiency test. In hot-plate water-boiling tests by "open" and, perhaps, "closed" types the rate of temperature-rise is, I think, covered by another factor in addition to the usual factors such as the absorbing power of the vessel, wave-length of the radiator or heater, quantity, energy, etc. I refer to a heat-insulating film that I believe develops on the under side of the absorbing vessel, due possibly to the large temperature difference of the absorber and the radiator. This film phenomenon may be of short duration. The greater temperature difference obtained with the "radiant" type plate than with the closed type plate may apparently account for the lower efficiency of the radiant or open-coil type plate. I suggest that the authors' method of testing by disc may shorten the time period of the film phenomenon, and therefore the disc test is suitable only for comparative tests of open-coil radiant-type plates. Such tests would tend to show a higher efficiency than would be obtained in everyday practice in the boiling of water. It would be highly interesting if this field could be explored so as to determine the temperature limits that should be worked upon to overcome the film phenomena and ensure better efficiency for closed and radiant-type plates. I think with Mr. Beauchamp that the future is not with the closed or open type of plate for boiling purposes, but with the immersion heater, which may be of a portable type that can be dropped into the ordinary boiling-cooking vessel, or the permanent fixed type of immersion heater for special vessels. With regard to the oven, the test might be made rather more from a practical than from a laboratory point of view, as the temperature of the air in an oven is of a very varying nature and has comparatively small heating capacity. The mere opening of the oven door causes the temperature to fall. The test should be made as practical as possible with food, meat, or its equivalent in weight, say, in the form of a water vessel; the temperature effects on this food vessel could then be noted to compare the effects of air lagging and slag-wool solid lagging. I believe that the residual heat in the food and oven fitments will have a marked temperature effect when tested in an air-lagged or lightly lagged oven as compared with the solid variety. It is also important that the large drop in temperature that occurs when meat is put into the hot oven should be avoided. The paper appears to deprecate the air-lagged oven, but I am of opinion that an oven can be constructed having an air space sufficiently deep to reduce the radiant heat losses to a practical limit for the comparatively small time the oven is in use. With a "still" air lagging of 1 inch to 2 inches, which is quite feasible, there should be a quick temperature-rise in heating up, and a smaller heat loss compared with the solid-lagged oven. There are two other schools of thought with regard to the electric ovens; manufacturers would like to get one or two standard forms of oven, but how are these to be heated? At the sides or at the top and bottom? Should the shape of the oven be shallow in height or of the tall and narrow variety? Possibly it will be necessary to divide the data required into two classes, namely, domestic ovens and ovens for large kitchens and restaurants, etc.

Professor J. T. MacGregor-Morris : I should like to know if the authors could give us the position of the curve in Fig. 3 if an enclosed type of hot-plate were used with a metal pan having the surface blackened and with one having a machined cast-iron surface. Would the rate of rise of temperature be much affected by the perfectly fitting surfaces? There are two points upon which I think more emphasis requires to be laid. The first is that more practical instruction should be

given as to how to use the electric cooker, as it is of little use to provide a long list of printed instructions if they are not read. Then it has been suggested that turned cast-iron vessels can be used with electric cooking. They are used, but do they last long? They are very often put on the fire and consequently their surfaces get blackened. Again, not a word has been said about reliability, and the question of repairs and renewals in the heating elements is certainly a very serious matter. If manufacturers can bring forward a boiling ring which can be used in the ordinary way throughout a period of, say, two years without a failure, then I should be very glad to know of it. With regard to another domestic appliance, the electric radiator is occasionally used for heating small offices, and there we have lamps each of 250 watts. I suggest it would be a good thing if a reliable lampholder could be supplied for such lamps. My experience is that the lampholder perishes after a time. The springs appear to fail owing to the holder getting too hot, with the result that the lamps will not light. These are little details to us, but they are not details to those who have to use the apparatus, and we want to get electrical appliances used by everyone, no matter whether he understands electricity or not.

Mr. E. T. Williams: I should like to see the subject of this paper dealt with a little more from the standpoint of the housewife. What does it matter to her whether the efficiency of the oven is 5 per cent or 50 per cent, as she is mostly concerned about her electricity bill and about the maid switching the oven off? Therefore an automatic switch that will switch the oven off when it reaches a certain temperature should be introduced. I feel that if we can introduce a cheap reliable thermo-switch to use in connection with electric heating we shall have advanced a long way towards making the housewife more happy in installing electrical appliances. Another small point in connection with propaganda is to put a stove, a radiator, or an electric iron in a house and let the people use it for a week free of charge. The chances are that a very large percentage of the housewives will keep it and pay for it; whereas they will not purchase appliances they do not know anything about. Mr. Berry and I are of the same opinion as to what will be the future heating in the home. To bear out this statement I may say that we at home always use an electric radiator in the various rooms in the spring and autumn instead of a fire, and it has been found invaluable, because there are certain days when it is too cold to do without some heat, though we only want it for a short time in the morning and the evening. We use an electric radiator for that purpose and it saves an enormous amount of work in the home and, incidentally, it is very much cheaper. If we as electrical engineers can only look at the housewife's standpoint we shall go a very long way towards introducing electric heating.

Mr. W. P. Adams: One important consideration does not seem to have received attention during this discussion. The same problem which proved such a serious hindrance to the adoption of electricity in the early days, more than 20 years ago, is still with us. While some means of heating water simply and economically for household purposes has to be provided, it will be difficult to get the housewife to adopt electricity for cooking. Heating water from cold to boiling point cannot be done without heavy cost by electric means, and probably no problem has so great a bearing on the development of electric cooking as this. Not only has water to be heated for cooking but also for washing up, scrubbing and cleaning, and for baths and lavatory use. Where a fire is needed for this purpose, and for warming the kitchen, the cooking range will hold its own until engineers have solved this difficulty.

Mr. A. E. Gott: When I was associated with domestic heating apparatus there was a great want of suitable resistance material. No metallic conductor existed having a resistance ratio with copper exceeding 50 or 75 to 1. The higher-resistance materials were generally unsatisfactory and none could be run red hot. I do not know that we are much better supplied to-day, and it is one of the restrictions which limits the successful construction of heating vessels to the larger sizes. Regarding efficiency, it is necessary to know the specific heat and capacity of the insulating and supporting mediums, as much loss occurs in the materials comprising these details, particularly when the utensils are used intermittently.

Mr. C. O. Bastian: The authors have pointed out what I think is the chief and perhaps only advantage of the open type of boiling plate, namely, that its efficiency is not deteriorated by defects in the cleanliness and true surface of the testing discs or cooking utensils employed. Therefore, in practical use, we should expect to find the open-type boiling plate yielding the most efficient results and, according to my experience, this expectation is realized in well-designed plates of low capacity, i.e. 750 watts maximum on a 7-in. plate. Such a plate is very little good for boiling water from cold, and we are therefore tempted to try higher loadings and immediately encounter grave difficulties. The exposed wire coils collect dust and other carbonaceous matter, become carbonized, and rapidly deteriorate at the higher temperature due to the higher loading. Even if the wire is protected from dust, etc., by means of silica tubes, the polished metal reflector, which contributes so largely to the best results, gets much too hot at the higher loading, and we have, perhaps too readily, sought to overcome this difficulty by substituting a refractory material such as fireclay. When we start baking fireclay instead of boiling water, we are again in trouble, and we have to admit that the enclosed type of hot-plate for higher capacities than 750 watts can claim some solid advantages which may well outweigh the single question of efficiency. Nevertheless, I think all will admit that the red-hot plate is the goal to make for, and that the solution will be found in the right selection and arrangement of material on the underside of the plate. I recently tested a boiling plate, measuring 7 in. × 7 in. square, loaded to 1 450 watts and, with a circular 7-in. kettle boiling on this plate, the temperature indicated by a thermo-couple resting on the reflector was 850° C. (1 562° F.). The reflector was of nickel rather heavily silver plated for the experiment, but it soon began to blacken, and I am still looking for the right solution. Another difficulty in

connection with the open-type plate is that the base should be readily removable for cleaning purposes and, even with such a base or reflector, I have seen hot-plates returned, after a few months' use in the kitchen quarters, literally clogged with dirt. It is very difficult to teach the public that heat reflectors should be kept clean. The design of boiling plates is generally admitted to be the most difficult problem in connection with the cooking equipment, and this difficulty is probably intimately associated with the question of the hot-water service. With an ample supply of hot water at 60° C. (140° F.) or thereabouts, in a thermal storage tank, from which cooking utensils would be filled, the work required from the boiling plates would be reduced by about 50 per cent. The 750-watt 7-in. plate would then become a serviceable appliance, and so would the 8-in. plate loaded to about 1 000 watts. This reduction would tend to relieve peak troubles. I do not know of any real difficulties in connection with the design of perfectly satisfactory ovens and grills, but my experience of electric cooking at home has convinced me of the importance of regarding as the first essential a sufficient supply of hot water that has not been overheated and spoilt for culinary purposes. I recognize in Fig. 8 an oven of a somewhat makeshift type, for the design of which I was responsible. Their manufacture ceased quite 8 years ago, and I should not to-day advocate heating elements in the base of the oven. I favour the view that the cooking load will have to be developed on thermal-storage lines in order to get the fullest benefit from that load, unless some epoch-making change is introduced, which will radically alter the conditions under which electric energy is produced and distributed to-day.

Mr. A. G. Warren (*communicated*): The authors' method of testing hot-plates has been somewhat criticized. It would appear to be possible to approximate more closely to the conditions of actual practice with tests as easily reproducible as those actually employed. The temperatures used were generally much above those required in actual service. A measurement of the heat absorbed by a vessel maintained at 100° C. would give results which could be more readily interpreted. The evaporation (which the authors mentioned as an objection to the use of water) might have been used as a measure of the heat absorbed. In nearly all the tests air could pass freely between the generator and the receiver of heat, but this does not occur under normal conditions. It would appear to be an easy matter to eliminate this without attempting mechanical contact, the uncertainty of which would doubtless cause serious difficulties. I am of opinion that the increased rate of heating when the disc is placed in contact with the plate, as shown in Fig. 3, is largely due to the elimination of the air passage between plate and disc. Even with the most perfect mechanical surfaces, the contact can only be unsatisfactory from the thermal point of view. There is probably considerable difference of temperature between the two surfaces, similar to the large surface differences observed in boiler plates. An idea of the nature of the contact could be obtained from simultaneous readings of the temperatures of the plate and disc. On page 362 the authors remark upon the lack of conduction across the plate in the tests made, but one would hardly expect it to take place. One might, however, look for such an effect when the disc is placed in contact with the plate. It would be interesting to know whether tests have been made upon this point. I cannot imagine that the whole of the uncovered part of the heater is wasted under conditions of normal service.

[The reply of Dr. Griffiths and Mr. Schofield to this discussion will be found on page 385.]

North Midland Centre, at Leeds, 22 February, 1921.

The discussion was opened with an introductory paper (see page 361) by **Messrs. E. Griffiths, D.Sc., and F. H. Schofield, B.A., B.Sc.**

Mr. C. E. Allsopp: The authors have told us the limits to which the experiments have been applied, and in my opinion it is unfair to criticize the paper by bringing in other matters outside the work covered by the investigations, without making it clear that the criticism is not aimed at the subject of the paper. They have also informed us that experiments were not conducted with water or fluid, because of the necessity for stirring. In my opinion, however, these tests could have been profitably carried out with stirring to approximate as near as possible to the actual process of boiling fluids. Regarding experiments carried out for temperature-rise on the thin copper plate, I fail to see why these tests could not have been carried out on the surface of the disc itself, but I appreciate that, with the heavier plate, safe deductions can be obtained from the results given. In connection with the tests carried out with the various types of ovens, my opinion is that the results obtained, owing to the fact that the tests were taken with the ovens empty, serve little or no purpose. This is borne out by the close resemblance of each of the tests carried out with a different disposition of element, and the results obtained would have been vastly different had the ovens been loaded up with materials to represent as near as possible the actual process of cooking. In connection with the hot-plate experiments carried out some time ago, I am surprised to find that with the utensil having the heavier section of metal it is possible to boil a pint of water more quickly, with the same amount of energy, than in a vessel constructed of sheet metal, and this can be accounted for by the walls of the vessel in the heavier type conducting the heat away from the points of contact with the hot-plate, and distribution taking place more rapidly than in the thinner vessel which was liable to hot spots and burning of heavy fluids. The disposition of the elements on an electric cooker has a considerable bearing on the success of the cooking operations, and a good deal of disappointment and poor results have been obtained in the past owing to manufacturers not studying this

point sufficiently. It is to be hoped that the authors will not allow this matter to rest until they have conducted further experiments and tests, from which I am sure considerable guidance and useful information can be gained. In my opinion the most successful oven is one in which the elements are placed well out of sight and harm (behind the protection of a screen made up by the shelf runners), the cooking being carried out by hot-air circulation and radiant heat from the side runners. Several ovens built on this principle have been used successfully for several years for baking bread and other operations, it being found that the low degree of radiant heat does not tend to burn the food before the cooking is completed, and that the temperature is sufficient for browning purposes. Prior to the war we, at Bradford, were seriously considering the question of hiring cookers out to consumers, and this matter is again occupying our attention. It is hoped that shortly a scheme will be put into operation which will give a success equal to that obtained by several undertakings who have already adopted the hiring system in connection with electric cookers. It is interesting to see the efforts that are being made by the manufacturers of gas cookers to rob the electric cooker of the advantage of being without fumes, and at least two firms to my knowledge are making gas cookers with the burners outside the oven chamber. These cookers are listed at £20, and from inquiries they are considerably lower in efficiency than the usual type of gas cooker with the burners inside the oven. It does not seem to me to be impossible to manufacture electric ovens which can successfully compete with gas cookers both as regards cost and upkeep, and also give better results.

Mr. H. N. Gresley: I notice that the efficiency with the copper disc is 7 per cent in the case of the closed type, and 13 per cent in the case of the open type. Further on, the efficiency of a thick steel disc varies from 60 to 30 per cent. Perhaps the authors will explain the apparent discrepancy. It is not quite clear why the efficiency is much higher with a thick disc than with a thin plate. I should like to know why the closed type of hot-plate is used at all if the open type is so much more efficient. In an ordinary hot-plate stove I imagine a frying-pan or saucepan is used. If the saucepan can be heated up by the open type, why use the closed type which is apparently not so efficient? It seems to me that, low as the efficiency of the closed type is, compared with that of the open type, it is very much lower unless the surfaces are in absolute contact, i.e. practically bedded down to one another. I think that in ordinary kitchens the care necessary to keep the kitchen utensils with their thin bottoms in that perfect state which is necessary would probably be lacking, and the pans would get knocked about, with the result that the efficiency would get extremely low. I should like the authors to give some information in regard to the heating of water. Hot water is necessary in connection with cooking, and also for washing up. I believe that heaters, as a rule, are very inefficient. An American utensil which has recently been put on the market consists of a sort of porous pot with a large number of holes extending from the top to the bottom, surrounded by an annular space between the inner and the outer walls in which a vacuum is created. The water passes through these various holes in the porcelain pot and then through a resistance of some suitable material until it comes out into direct contact with the heater. The efficiency is claimed to be 99 per cent. I do not think that ovens should be constructed with heaters along the sides or the bottom, on the ground of cleanliness. It seems to me the best form of oven, if it can be made efficient, is one in which the heating elements are hidden away behind the wall so that the whole of the interior can be thoroughly cleaned out.

Mr. A. E. Jepson: I think I can answer Mr. Gresley's question in regard to the heating of water. In 1905 I read a paper before the Manchester Students' Section of the Institution, in which I described a method of heating water by a Professor Ougrimoff, of Moscow. He obtained an efficiency of 98 per cent by having a cast-iron crucible inside a tank which was enclosed in a larger metal tank and surrounded by water. There was a carbon rod in the centre of the crucible, and an arc was stuck up between the two. In my own house everything is done electrically. There is an immersion heater in a copper tank, and it is quite successful. The heater has three heats and is controlled by a rotary-type switch. On full heat the load is 3 kW, and on the lowest about $\frac{3}{4}$ kW. Instead of fixing it in the orthodox way, i.e. at the bottom of the tank, I had it fixed half-way up; the reason being that when one wants a comparatively small quantity of water, sufficient for washing up or even for one bath, it can be obtained in half the time. The arrangement is so successful that, by switching on the top heat and leaving it on, we can get three baths of really hot water. I think that electrical engineers are very much to blame for not increasing the consumption of electricity in houses. The power station engineer is usually concerned with getting load from works, because he sees that his neighbour has got a large load from similar sources, yet there is an enormous amount of energy to be obtained from houses. The total capacity of the heating apparatus in my house is 108 amperes. The maximum which I have used is about 60 amperes. The problem of installing mains to cope with a load like this is difficult, but the load is useful. It is heaviest after the works are shut down and during week-ends, holidays, etc. Many people, including electrical engineers, say that electricity is extremely extravagant, but one can economize better with electricity than with coal; if one goes out of the house, the current can be switched off. No doubt the first cost of the apparatus is heavy, but it saves its own cost in the long run. There are one or two housing schemes in preparation at the present time, and it is suggested that the houses should be fitted with electric cooking and heating apparatus, and that chimneys should be dispensed with. My advice to the responsible authorities is to put one chimney in the house, and a kitchen fire-place. In many places, particularly around the Sheffield area, there are so many miners who get their coal for nothing, that it would be absurd to try to supply them with electricity. I am sure colliery

owners would save if they supplied free electricity instead of free coal. I have covered the copper tank in my house with felt, and stuffed the cupboard with old newspapers. The heat insulation appears to be effective, as the water in summer keeps hot for three days. The paper is an extremely useful contribution to our knowledge of electric heating, but unfortunately it does not go far enough. Fig. 1 shows only two sizes of discs, a 5-inch and a 7-inch disc put on to an 8-inch hot-plate. These figures would have been very much more interesting if an 8-inch disc were used and also one about 10 inches, because the pan used is often much larger than the hot-plate. Fig. 2 shows various treatments of the disc, but I think the most important one, the one which is most often met in practice, is the dull bottom and polished top. I was rather surprised at some tests I made on a bowl fire or pedestal heater, which had a parabolic reflector with a small heating coil. It took about half an hour to boil the water in an aluminium kettle. I then had the bottom of the kettle painted with ordinary black paint, and the water boiled in about 16 minutes. It was hard to believe that such an improvement in efficiency could be obtained. When I was testing heating apparatus 19 years ago, I tried the effect of polishing various utensils, and I found that by polishing a copper kettle there was an improvement in efficiency of 10 per cent. Another important matter to bear in mind is that, if the kettle be used for a second time immediately after being emptied, the efficiency rises to 85 or 90 per cent, for the heat put into the bottom of the kettle is used and the loss by radiation is very small. Speaking of the question of the bowl fire and radiation, it has often occurred to me that we might find some much quicker way of boiling water than that in use at present, by using parabolic reflectors to reflect the heat rays on to a small blackened surface. If the beam of rays could be narrowed, more heat would be concentrated on the kettle bottom than if the ordinary heater element or a self-contained heater in the kettle were used. It might be worth while to investigate this point. At the bottom of page 363, the authors say: "It is impossible to show the effect of polishing to the best advantage with copper, owing to the tarnishing which occurs as soon as the temperature of the disc is raised appreciably." I would suggest that aluminium might have been used, in which case the surfaces would not have been oxidized to such an extent. In actual practice most copper vessels are oxidized on the bottom. That is one of the most important points. The whole idea of these experiments was, I take it, that they might be applied commercially. Unless experiments are carried out on lines similar to those in practice the best use cannot be made of the experiments. On page 365 the authors say: "The heat capacity of this disc was equal to 1·6 pints of water." I do not think a solid body should be used in taking a test. When water is being heated in a vessel, the process is for the bottom of the vessel to get hot and conduct to the immediate surface of the water, and that is immediately carried upward by convection, and cool water comes to take its place. From what I understand of the problem, the greater the gradient that can be obtained between the temperature of the vessel and that of the water, the quicker the whole body will be heated up. In the case of a solid body such as a disc, a cooler surface is not obtained near the heater, for the temperature throughout is approximately the same, due to conduction. I quite agree with what Mr. Allsopp says about ovens. There is another important point in connection with ovens and cooking. To-day I happened to pick up Professor Ray Lankester's book, "More Science from an Easy Chair." In this he refers to the roasting of meat and so on, and says: "The scientific importance of good roasting and grilling is that a savour is thereby produced, which sets the whole gastric and digestive economy of man at work." Elsewhere he refers to the cooking of a joint and he is quite right when he says: "A joint should not be roasted by a low temperature and by convection currents only. It should be done in the old-fashioned way, by radiation." After all, the jack in front of the fire was the best way of cooking. The best way with modern arrangements is to have a red-hot radiating surface to heat the meat. First of all, this heat closes the outside pores of the meat and keeps the juices in. The question of having an even temperature is not important, as one often wants the oven for other things than meat. The meat can be arranged to be at the top, and the sweets at the bottom. The figures for efficiencies given in the paper are rather misleading. They do not refer to the efficiency of electric heating or electric cooking, but to the efficiency of the plate as a plate and as an absorbing medium, and so on. The figures relating to the ovens are extremely interesting, but one point has not been dealt with, i.e. the question of the lagged oven and the unlagged oven, and the relation of the inner surface of the oven to the whole question. I understand that if the interior of an oven is very highly polished, whether it is lagged or not, the efficiency remains almost the same, provided the outside surface is also polished. I agree with Mr. Allsopp that the results would have been far better if something had been suspended inside the oven to absorb the heat which is given off. I find that a slight percentage difference in ovens does not affect the matter, as there is such a big human element in this question. I would suggest that as an experiment the authors might try an oven with a lagging between the outer and inner surfaces and with a matt black surface inside, and a polished surface outside, and then vice versa, so as to provide some means of comparison. The results would be rather more interesting if an enamelled inner surface, such as many domestic ovens possess, could be tried. Unquestionably, electric heating will become the method of the future. It is only by installing it in our own homes and showing it to our friends, that we can get people accustomed to it. Such things as electric kettles and toasters are coming into more general use. If power supply companies can get a load of 4 000 units per annum from each house in a residential district, the diversity factor will be improved.

Mr. S. D. Jones: As to the question of the working of the oven, say as regards the cleaning, a gas oven is often used in a rough manner. Until an electric

cooker can be used somewhat in the same rough and ready manner I do not think we shall have great success. I have lately had occasion to advise on electric cooking in Batley. There was a chance of putting a fair-sized installation in one place. I got into touch with many different firms, but have not yet been able to get the installation carried out. Two facts struck me forcibly. One was that the firms did not appear to push the matter as they should have done. The other fact that struck me was the high first cost of electric cooking apparatus. If I were building a house I should be inclined to install a coke heater to heat the water, and an emergency heater to obtain the final temperatures required. This would be an extremely good method of heating the water and overcoming the difficulty. The present method is very inefficient.

Mr. J. C. B. Ingleby: I should like to suggest that the National Physical Laboratory take up the question of a better heating element. Nichrome does not stand a great enough temperature, similarly mica calcifies and punctures in a very short time after being enclosed between metal plates. While such hot-plates are new and polished they are efficient, but they oxidize very quickly and this oxidization prevents the heat from reaching the kettle. Therefore, the elements get exceedingly hot and burn out or puncture the mica very quickly. In electric ovens, I think that top and bottom elements and dull or even black heat are essential. I find that a kettle takes twice as long to boil in winter as in summer. Again, I find gas boils a kettle much more quickly and cheaply than electricity, so I keep a gas ring for that purpose. For rooms heated solely by radiators, the chimney can be blocked up with advantage; such rooms keep wonderfully clean and, although electric heating is rather costly, a lot of work is saved and possibly one is fully compensated. As regards the heating of domestic water in my house, I raised the ordinary round cylinder in the bathroom about 9 inches by means of bricks, jacketed the cylinder with asbestos and put a gas ring under it. It has proved very satisfactory, and provides ample hot water for washing purposes.

Mr. L. E. Wood: I used electricity for all domestic purposes in my house in Canada. The pressure of supply was 110–220 volts, alternating current, the connections being so arranged that, as near as possible, a balance was obtained on both sides of a core-type transformer. On the top of the oven were three elements, the loading being 660, 1 500 and 880 watts, respectively. Inside the oven were two elements, each of 880 watts' loading, and placed top and bottom. Each element consisted of two spirals of nichrome wire, each suitable for 110 volts. The spirals were laid in alternate grooves to each other in the refractory plate, so that on medium heat the heat was not confined to one half of the plate. Snap switches were used, arranged for low, medium and high, i.e. three degrees of heat. The shortest life of an element was 18 months. Cooking was commenced by using both oven elements on "high heat" until a temperature of 300° F. was reached, after which "medium heat" was used. When baking bread or pastries advantage was taken of the retained heat when ordinary cooking was finished. When renewing the elements I tried fixing them on the sides of the oven, but the results were not nearly so good as when they were placed top and bottom, pies, etc., not being properly browned. I also experimented with water heaters. As all are probably aware, Canadian houses do not have fire-places, some form of central heating being used. Each tenant provides his own cooking stove, something like a kitchen range. For heating water, one side of the grate lining of fireclay is removed, and a water front fixed in its place. This front is a casting, in reality representing a single-turn coil, and is connected in series with the circulating system and a 35-gallon tank. In the summer the stove is very little used, so an electric water heater is used in conjunction with the system, to provide a continuous supply of hot water. I experimented with one of 3 000-watt loading and three degrees of heat. On full heat the time taken to heat the 35 gallons of water to 180° F. was 3 hours; after which the low heat was sufficient to keep the water hot. Iron piping was used in its construction but, on account of complaints of the humming, brass tube was substituted. Another type was designed so that the water circulated around both sides of the element, but difficulties arose due to air locks. The Aqua water heater is really a geyser taking about 40 amperes, so that a special circuit is required for it. Heaters of this type usually consist of carbon cylinders with a small space between them through which the water passes, the latter being heated on the way. Leaving out the question of electric heating, the possibilities of domestic appliances are tremendous, and the market in this country is scarcely touched.

Mr. J. Lovell: The work done is very useful as far as it goes but, as previous speakers have pointed out, it does not go far enough. It should be very useful to the manufacturer, but at present it is not of much use to the salesman. After all, it is of no use designing and manufacturing the material unless we are able to sell it. In selling this domestic ware, not only hot-plates and ovens but fires and such lines, we meet a difficult class of customer. It is often easier to sell engineers a big plant than to sell a lady a flat iron. The authors disarm criticism by pointing out that this paper is in the nature of a trial, and by asking for suggestions as to how similar tests should be carried on and the results that are wanted. If I may make one suggestion it is that the authors should follow the lines suggested by other speakers, and at the same time start a lady at the other end, not necessarily a member of the National Physical Laboratory staff, but a housewife (and preferably a north-country woman). When—and if—they meet we shall reach perfection. We have to consider the practical point of view of the user, i.e. the housewife, and that seems very often to have been overlooked in the design of apparatus at present on the market. The apparatus is very well made and skilfully designed in a number of respects, but it has not been tried out in the way Mr. Allsopp and Mr. Jepson mentioned, by taking the articles home and allowing the housewife to try them. This is the only way to get a perfected, satisfactory article. One other point that has occurred to me is the use of the

word "efficiency" in the tables given in the paper. I think it is perhaps unfortunate and may be misinterpreted. Gas engineers may endeavour to make use of that term and these figures. A number of us have had an opportunity of inspecting the present Gas Exhibition in Leeds. I expected to see a number of novel ideas, but was, on the whole, disappointed. I saw only two irons, so their use has apparently been discarded. There were, however, many water-heating devices, and that is where we have so far not been able to show any great advantage. From Mr. Jepson's description of his water-heating scheme I believe it is on the lines of the commercial heater that we shall use finally. At the present time gas engineers hold the field when it comes to heating water supplies such as we require for domestic purposes, bath and scullery use, etc., and if a little thought and energy are put into that question I have no doubt it will enable us to carry electricity through in that field as we have done in the industrial field.

[The reply of Dr. Griffiths and Mr. Schofield to this discussion will be found on page 385.]

South Midland Centre, at Birmingham, 23 February, 1921.

The discussion was opened with an introductory paper (see page 361) by **Messrs. E. Griffiths, D.Sc., and F. H. Schofield, B.A., B.Sc.**

Mr. N. B. Rosher : I think that if the use of electric cookers and heating appliances is to become universal in the home there is much need for further research and practical experiment under working conditions on the part of designers and manufacturers. The present paper is a step in the right direction. Whilst I have faith in the ultimate triumph of electricity over other methods of heating and cooking, yet that desirable condition has by no means been attained, in fact only the fringe of the electric cooking and heating load has been touched. Taking the average town, I think it would be a very liberal estimate to put the relative proportion of electric to gas cookers in use as 1 to 500. When all allowances have been made for electricity, such as a late start in the field as compared with gas and possibly not such efficient or keen sales organization, etc., further explanation is required to account for the poor progress which has been made. One of the chief reasons, in my opinion, is the high cost of the electric cooker, which may be taken as about double the cost of a gas cooker of equal capacity. Householders will think twice, especially at the present time, before they will embark on any expenditure which can be avoided unless undeniable economy is to be obtained thereby. It has been repeatedly stated, and with a good deal of truth, that within reason the cost of energy for an electric oven is of no consequence because the loss in weight is only 10 per cent as against 20 per cent in a gas oven. Unfortunately this argument, although not entirely disbelieved by the householder of moderate or small means, is not in most cases sufficient to overcome his reluctance to pay the extra capital sum required for the electric cooker, even though the argument be reinforced by statements as to the additional advantages of cleanliness, freedom from fumes, and convenience. I believe that another reason, almost, though not quite, as important as the first, why electric cooking has not progressed faster is that supply authorities have not followed the example of the gas authorities in providing cookers on the hire or hire-purchase system. The reason for this is that probably in many cases their load has grown faster than they have been able to cope with, and therefore there has been neither time nor inclination to cultivate the domestic heating load. Assuming the domestic load to be wanted, I see little likelihood of it being obtained on a really big scale unless electric cookers in particular are supplied on hire terms. The provision of hot water for the household is, under present conditions, hopeless from the point of view of either electricity or gas. A separate coke-fired boiler is the only efficient means of providing this essential commodity. Investigations such as those carried out by the authors of this paper are of great value, but further research is required with a view to the production of electric cooking and heating appliances which shall be not only cheaper to purchase but cheaper to operate and maintain than any which are on the market at present.

Professor W. Cramp : The paper attempts to deal with two aspects of the problem and, unfortunately, in so doing makes neither issue clear. One aspect is the formulation of standard tests whereby the apparatus may be compared and judged. The efficiency of the apparatus is quite another matter. It is much to be regretted that the expression "efficiency" should have been used throughout the paper for the ratio of the heat absorbed by the proposed test block to the heat equivalent of the energy supplied to the plate, for this debits the plate with the heat energy emitted by the test block. It may or may not be agreed that a block having a heat capacity of about 2 pints of water is a fair test, or that the heat emitted from the water is sensibly the same as that from the block, but in no case is the ratio selected the real efficiency of the hot-plate, which is the ratio of the total heat energy transmitted by the plate to the selected vessel or block, divided by the energy supplied to the plate. It is a pity that scientific work which in other respects is so good should be marred by a terminological inexactitude so harmful to the electrical industry. The results obtained by the authors have an interesting bearing upon similar apparatus for gas; for it seems certain from the experiments given in the paper that gas rings constructed upon the radiant energy principle would be more efficient for boiling purposes than those of the ordinary type. The authors appear to attribute the difference between the curves shown in Fig. 2 (at least in part) to a wrong cause. They state that the difference between the second and third experiments shows that in the latter case the polished under surface reduces the rate at which heat is absorbed. This may be true, but it is at least equally true that the oxidized upper surface would increase the rate of emission of

heat by the disc, a fact apparently overlooked by the authors. Indeed, the three curves in Fig. 2 only show how impossible it is to measure "efficiency" by such means, for to oxidize the top of the test plate cannot possibly alter the efficiency of the hot-plate, and yet on the authors' assumptions it would change it considerably. Figs. 2 and 3 are exceedingly interesting when analysed. It must be remembered that three main phenomena are proceeding simultaneously, viz. the absorption of heat by the hot-plate itself, the absorption of heat by the test disc, and the loss of heat from both. If the disc were receiving heat at a uniform rate and simultaneously emitting some of it, the curve of temperature-rise and time would be of the ordinary exponential form. This is almost the case in Fig. 3, experiments 1 and 2. If, however, the plate itself is absorbing heat, the slope of the curve will be reduced, and this is obviously what is happening during the first 4 minutes in Figs. 2 and 3, experiments 3 and 4. The effect of placing the disc in close contact with the plate is to reduce the energy stored by the latter, and is not a change in its efficiency, as alleged in Tables 1 and 2, since the stored heat is partially, at least, recoverable when the current is switched off. With reference to these and other tables in the paper, I should like to press the authors to give us full details of types A, B, C, D, etc. Indeed, the absence of this information much reduces the value of the results to members of the Institution. The experiments with cast-iron bowls are similarly unsatisfactory from the scientific point of view. The hot-plate, though credited with the evaporation loss, is debited with the heat absorbed and emitted by the bowl as well as the liquid. It is not surprising, therefore, that the efficiencies are different, and in any case cast iron is a very unsuitable material for the bowl. With regard to the ovens, the authors say that it is out of the question to use highly polished metal surfaces in oven construction. I disagree with them, since one of the successful ovens on the market is made on this very principle. I would also add that another good oven, viz. that built with side channels for circulating the hot air, has been neglected in the paper. The authors' views on lagging are very interesting, but they omit to refer to the use of vacuum lagging which Mr. Frith and I have used successfully in connection with large pans. The difference between the two curves shown in Fig. 14 is also interesting, but of course this is a time effect only, due to the restoration of heat stored by the thermal capacity of the oven itself. I entirely agree with the authors as to the economies to be obtained by the use of a thermometer inserted in the oven and readable from the outside without opening the door; and I would add that, for similar reasons, the provision of a glass door or observation window is very desirable.

Mr. W. J. Line : In domestic electrical apparatus too little attention is often paid to the details and accessories. Two important factors in such articles, whether for heating or lighting, are convenience and reliability. Electricity is admittedly convenient, and has a great advantage in this direction. One has only to turn a switch, not, as is the case with gas, often to search for matches. Unfortunately, as to reliability the reputation of electricity with the public is not always so good. Much harm has been done through the ill-advised supplying of apparatus adapted for connection to ordinary existing circuits and lampholders, when neither the wiring, the lampholder, nor the adapter supplied with the portable apparatus is capable of carrying the current involved. The same spirit which, in the colleges, teaches dynamo design and assumes that any one can design regulators, starters, magnets, coils, and other accessories, is to be seen in the design of electric heaters, stoves, etc., which often are sent out to work with switches, wall plugs, connectors, and flexibles of unreliable design and insufficient capacity. Wall plugs of the pin-and-socket pattern are a common example. Contractors frequently report that these will not carry their rated current. This being so, it seems unfortunate that the British Engineering Standards Association has standardized this pattern while fresh designing intelligence requires to be brought to bear in order to evolve a more satisfactory type.

Mr. S. H. Holden : With some systems of domestic supply it is usual to employ a limit switch. This produces an objectionable intermittency of supply if the prearranged current is exceeded. It is doubtful whether this device would be suitable as a safeguard for the supply authorities in the case of cooking apparatus, and I should like to know the effect of intermittent current on the performance of a cooker.

Mr. H. Joseph : My only criticism is that the authors have departed somewhat ruthlessly from working conditions in their tests. I should like them to say why they neglected to use the method of heating lagged vessels with specially prepared contact surfaces. The loss by evaporation would appear to be a simple matter of calculation, whereas the use of discs involves an unknown loss by radiation and gives a false value for the efficiency. With regard to oven experiments, the absence of working conditions appears to leave room for further experiment. It would certainly be interesting to hear what effect the presence of an atmosphere heavily charged with aqueous vapour, as in the case when a joint of meat is being cooked, would have on the authors' results. Perhaps the authors could tell us something of this in their reply. I should have liked some information as to the effect of the position not only of the thermometer, but also of the heating elements. In regard to the use of a thermometer in cooking, this has been tried for some years in my household where electric cookers have been used, but it is found that with practice, for a given weight of meat, for instance, the time required can be anticipated exactly and the cook can carry out other duties and return to the oven just before the cooking is completed, there being no necessity to open the oven door in the interval. While electric cookers have certainly given a good deal of trouble in the past, they have been greatly improved in recent years, and experience with gas cookers has led us to discover that they are by no means free from such troubles as stopped-up burners and pipes, necessitating a hurried call on the services of the gas department.

Mr. W. Wilson (*communicated*) : The conclusions arrived at by the authors are valuable in that they demonstrate conclusively the unfitness of the hot-

plate, and of some designs of cooking stove, for their purpose. It has always seemed to me that so long as designers of domestic electrical appliances adopt a copying policy, they are fighting a losing game. As soon as such lighting apparatus as "gas arcs" made their appearance, designed to copy the arc lamp in design and even outward appearance, we realized that gas was declining as an illuminating agent; and as long as electrical engineers produce imitations of the kitchen range or the gas ring, they are admitting that their task is practically hopeless. But if domestic electrical appliances are designed from first principles, in such a way as to make use of the very valuable attributes possessed by electricity for the purpose, then I am convinced that the electric current will come rapidly into use for this class of work, to the exclusion of other methods. Now the most notable property possessed by an electric apparatus is that, whereas there is a considerable loss in the generating station in the conversion of the heat energy to electric energy, at the receiving end the electric apparatus fulfils its functions with scarcely any loss at all. For example, an electric radiator can be placed in the middle of a room where its efficiency will be 100 per cent; while an electric motor can be located anywhere and works with a loss of only about 20 per cent. This means that the objectionable phenomena attending the emission of unused heat are absent and, in consequence, electrical apparatus becomes safer, healthier, cleaner, and more convenient in every way. Thus we can take it as an axiom that, in order to be acceptable, an electric appliance must have an efficiency approaching 100 per cent. On this basis the hot-plate with its possible efficiency as low as those referred to in the paper, between 10 and 50 per cent, must be discarded. Fortunately the case is much better with self-contained utensils such as electric kettles, saucepans, frying pans, etc. Given proper design a satisfactory efficiency can be obtained; while these utensils have other important advantages which electricity renders available and which are not possessed by the hot-plate. For example, they are much simpler, easier to keep clean and to care for, and safer. I would suggest that a small household equipment should consist of three electric saucepans, two electric kettles, an electric frying pan, and a small hot-plate to be used for miscellaneous purposes, and to be employed as sparingly as possible. These would take the place of three hot-plates with separate utensils, and it would be found that the cost is very little more than the latter, while the gain is enormous. With regard to ovens, the old kitchener had to be designed in such a way that coal could be easily fed into the fire-place, and that the hob could be easily available for boiling, frying, etc.; while it had to be made of cast iron, in order to withstand the rough usage due to coal-firing and the use of heavy cooking utensils. None of these necessities exists with the electric oven; yet we find that quite a number upon the market have been designed in exactly the same way. They are made of cast iron, they are placed low down on the floor, and they have an imitation hob upon the top upon which kettles, etc., are to be heated. Again, to be acceptable the design should have been evolved without any reference to either the kitchen range or the gas stove. Full advantage should be taken of the opportunities of a much improved design offered by the use of electricity. The oven should be a self-contained unit and should be located so that the shelves, and not the top, are at a convenient height for the eye and the hand. There should be a glass door, in order that the cooking operations can be observed without the necessity of opening and the consequent periodical escape of valuable heat. The materials should be light and should conserve the heat. Finally, the current should be controlled by a thermostat in such a way that the desired temperature may be reached but not exceeded. Cooking other than roasting should be done on a convenient wooden table, the utensils being stored in a place easily accessible from this spot. Very great advantages are derivable from a cooking outfit of this description. The room can be kept cleaner, and can be decorated in any way desirable; in fact, the entire cooking process could be carried out in the dining-room if it were desired to do so. There are two criticisms which I should like to make with regard to the ovens described by the authors. One is that the tests have been carried out at no load, in that there are no vessels or materials to be baked in the oven. This must materially alter the distribution of heat and the consumption of energy. In this connection I should like to refer to a paper * by Mr. H. Gray in which the value of the oven was estimated by the actual cooking of bread. Again, it has been assumed that the temperature of the oven should be constant all over. One of the criticisms that I have heard with regard to existing patterns of electric oven, is that it is impossible to "brown" articles such as scones in them. The ordinary coal oven has the property of producing a higher temperature upon the top shelf than in the lower portions, and it is the practice when cooking many articles to transfer them during the last stage from the bottom to the top shelf in order that the browning process may be carried out. This would necessitate a special heater at the top of an electric oven, which might be used when desired. This point may not be very important, but the authors have chosen a number of representative ovens, and it seems to me that this qualification is absent from all of them.

Dr. E. Griffiths and **Mr. F. H. Schofield** (*in reply to the London, Leeds and Birmingham discussions*): Several speakers have taken exception to the use of a metallic block instead of a vessel of water in the tests on hot-plates. As Mr. Good pointed out, this question was carefully considered by the Committee of the British Engineering Standards Association, and they reached the conclusion that a metallic block affords a more definite and permanent standard than water for making comparative tests. There is no difficulty in converting the results obtained with such a block into the equivalent water value, or, if desired, making the block in the first place exactly equal in heat capacity to a pint or a quart of water. In practical work the metallic block has obvious advantages; the temperature is uniform throughout the mass,

* "Electric Heating as Applied to Cooking Apparatus," *Journal I.E.E.*, 1911, vol. 47, p. 219.

owing to the high thermal conductivity of the metal; there is no uncertainty as to the temperature of the walls above the water line, as in the case of a kettle; and there is no appreciable distortion of the surface of the block by heating.

The use of a thin disc on pegs has been criticized, and with much of what has been said the authors agree. Reference to page 364 of the paper makes it clear why this proposed method of test was abandoned in favour of a thick slab of metal with its surface in contact with the top of the hot-plate (if of the enclosed type). The results for the thin disc were included in the paper as being of scientific interest, and possibly when reading the paper at the meeting the authors did not make it quite clear that the method of using a thin disc on pegs was abandoned in favour of a heavy block.

Exception has been taken to the term "efficiency." This word was used in the narrow sense defined on page 364 and was also placed in inverted commas; possibly the term "heat absorption ratio" suggested by one of the speakers would be less objectionable. To use lagging on the surface of the test disc would complicate matters, and the same end would be attained by applying a small correction for the heat loss from the surface of the bare metal.

Mr. Cooper's suggestions are very interesting; as regards the use of a resistance thermometer, this would be somewhat expensive for works use. The surface of the metal block was made of the accuracy mentioned because it is easily attainable with a modern surface grinder, and the use of a block with a fairly accurate surface has this in its favour: it shows up to advantage the hot-plate with the truest surface.

There is no doubt that for water-boiling and similar operations the immersion heater, if of suitable design, is the most efficient type. The baths employed for the comparison of mercury thermometers at the Laboratory are equipped with immersion heaters. These are made by winding strip nichrome on mica, sewing two thin plates of mica on either side, and inserting the unit into a tightly-fitting sheath of copper. A number of such pockets project into the water from the underside of the bath.

As regards the tests on the ovens, it must be remembered that, in ordinary cooking practice, the empty oven is heated up from cold and, after the desired temperature has been attained, the material to be baked is placed in the hot oven. The tests specified determined the time and energy consumed in raising the temperature of the oven from cold and, secondly, the energy required to keep it steady at the baking temperature. The heat absorbed by the material being cooked has no bearing on the design and thermal efficiency of the oven, so it would only complicate matters to introduce this variable.

Professor Cramp's suggestion of a vacuum-walled oven is hardly practical. It is questionable whether the insulating property of the so-called vacuum lagging which he describes in connection with large pans is nearly so efficient as that of a covering of magnesia asbestos or cork. A very high vacuum is necessary, and radiation must be reduced by employing polished metallic surfaces.

In conclusion, the authors might point out that the paper deals with a series of experiments which they were asked to carry out, and it was not within the scope of the work to make a study of the methods of improving the efficiency of these appliances.

DISCUSSION ON

"A PRECISION FORM OF THE KELVIN ELECTROSTATIC VOLTMETER."*

Mr. F. Ockenden (*communicated*) : The paper is of great interest to all who have had occasion to use electrostatic instruments. They will know the feeling of confidence which the employment of this type inspires when only a reasonable degree of accuracy is required. When, however, the readings have to be comparable with those obtained on a potentiometer, bridge, or similar class of measuring device, the electrostatic voltmeter, as commonly constructed, falls very far short of requirements. It may, however, be greatly improved by careful modification of the usual design, and I find it interesting to note how closely that described by the author agrees with a similar instrument which I constructed some time ago in the laboratories of Messrs. Everett, Edgcumbe & Co., for use as a heavy-current measuring device by means of a potential transformer and shunts. As the transformer in this case had been previously designed with a ratio of 0·2 : 40, it was necessary to design the voltmeter for a full-scale (90 degree) deflection of 40 volts, as compared with the author's 150 volts. The outstanding features which this instrument has in common with the one described are :—

The use of a bi-filar suspension.

The reduction of the weight of the moving parts to a minimum, in fact 2·5 grammes.

The employment of a fairly large number of needles in the moving system, viz. 15.

On the other hand, the damping system has been designed to withstand conditions probably more severe than those met with at the National Physical Laboratory, and consequently differs considerably from that shown in the paper. As the conditions referred to consist of heavy local vibration due to machinery in the vicinity, such as would be met with in any factory, the design adopted may be of interest. A form of suspension head devised to meet a difficulty similar to that mentioned by the author, that is, to replace a broken suspension with another of precisely similar strength, may also be of interest, in that it provides means for doing this without adding to the weight of the moving system for the purpose of adjustment. A general idea of the device used is given in Fig. A. It allows for the automatic correction of error due to (*a*) variation in the elasticity of the wire, at the actual tension at which it is in use ; (*b*) any slight deviation from the vertical of the instrument, movement, or suspension head. Providing, moreover, that the inductors can be placed, or replaced, in exactly their correct relation to lamp and scale, the deflections may be fitted into the scale length with the utmost precision. The suspension consists of a single length of fine tungsten wire, sufficiently long to form both sides of the " pair," and has each end clamped in a separate clamp to the top of the movement. Unless two clamps are used it is difficult to ensure that both wires are efficiently held, and that there is no internal twist between the ends. If this condition is not complied with, the movement will not hang truly. The wire, fastened to the movement or to the hook which engages it, is looped over the pulley P, and hangs down between the jaws of the miniature vice V, and also between the ends of the pivoted arms C, C. The instrument is put into position and levelled. Since the wire is carried over the slack pulley the tension on each wire must necessarily be the same. The distance between the two wires may be adjusted by rotating the screw-head H. The cone on the end of the finely-threaded screw engages between the upper ends of the arms C, C, and the gap between the lower ends is thus caused to expand or contract to any reasonable extent with great precision. When the correct separation has been approximately found, the vice V is closed and a more accurate trial made. Provided, however, that the jaws of C,C, are in practically the same plane as those of V, but at right angles to them,

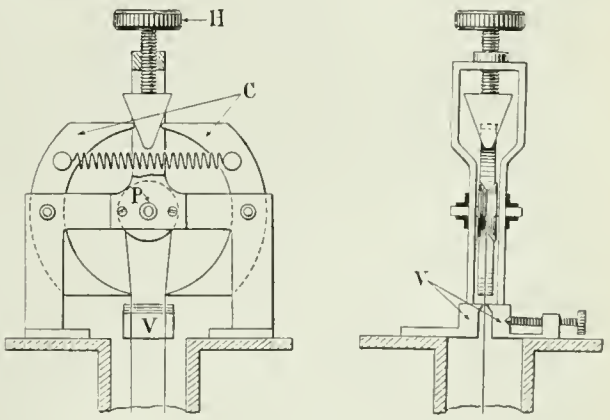

FIG. A.

the alteration in the effective length of the suspension—due to closing V—is small and may easily be allowed for. The correct disposition of the inductors, and therefore of the instrument as a whole, relative to the scale, is ensured by having a mirror firmly fixed to the case itself and adjusted to throw a spot from the lamp, or from a second fixed light-source, on to the zero. On replacing the instrument the two spots and the zero are made to coincide by rotating the instrument on its base, and the suspension head about its own axis. The disc damper described in the paper is no doubt excellent for use in a vibration-free situation, but in any factory vibration from heavy machinery in the locality is almost inevitable, and unless the damper is carefully designed this vibration will give rise to unexpected and apparently untraceable error. Provided that the damper of the usual disc or paddle type is not immersed in oil, no

* Paper by Dr. E. H. Rayner (see page 138).

reasonable amount of vibration is likely to affect it. When immersed, the lower end becomes to a certain extent clamped. The whole then tends to behave as a violin string and, owing to the presence of one or more hook joints in it, will pick up almost any low frequency applied, whether regular or irregular. I have found the working of a heavy gas engine, in a separate building about 100 yards distant, to cause a "zero shift" equal to the application of 3 or 4 volts, since, once the vibration of the movement is set up, the slightest "want of truth" in any immersed portion of the damper tends to act as a propeller and so swings the movement round. In Fig. B is shown the form of

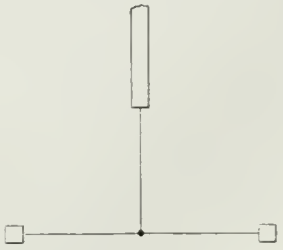

Fig B.

damper which I have found best fitted for its purpose. The paddles (of minimum area) are attached to the ends of arms sufficiently long to secure adequate damping torque, and yet exerting no vertical "clamping" effect. In the design described by the author vertical clamping is a maximum for a given radial damping torque. It may be noticed too, in the same figure, that the diameter of the damper spindle is made very small (about 0·01 inch). I have found this desirable, since if the diameter of the spindle—at the point where it breaks the surface of the oil—is appreciable, a considerable quantity of oil tends to cling to it, the pull thus exerted increasing the effective weight of the movement, and this will form an appreciable increment, seeing that the weight of the movement is as low as 2·5 grammes. This error will vary with the viscosity, and consequently the temperature of the oil. The wire spindle shown, however, successfully reduces this effect to a negligible value. The idea of using some form of Dewar vacuum flask as a container for the damping oil has occurred to me. It would obviate the variable-damping troubles experienced during very hot and very cold weather, but I have not yet tried this in practice. The following figures relative to the design referred to may be of interest :—

Max. reading = 40 volts, corresponding to 130 in. of scale length.
Radius of scale = 80 in.
Volts per inch in centre of scale = 0·15 V.
Volts per inch, average = 0·3 V.
Periodic time, undamped = 15 sec.
Total weight of moving parts = 2·5 g.
Number of needles = 15.

Dr. E. H. Rayner (*in reply*) : Mr. Ockenden's notes will be very helpful to anyone intending to install an electrostatic instrument for making precision measurements, using the word precision to mean that the instrument can be read with certainty to 1 part in 1 000 over a large part of the scale. In the ideal bi-filar suspension the elastic torsional control of the suspending wire is negligibly small in comparison with the force of gravity. The correction for variation in elasticity of the wire mentioned by Mr. Ockenden may be therefore somewhat misleading. However, in dealing with such small masses as 2 or 3 grammes, it is impossible to reduce the torsional control as low as one would wish, as suspending wires less than about 0·02 mm. (0·001 in.) become too delicate to handle, though the tensile strength may be more than sufficient when they are mounted. The screw-and-cone arrangement for adjusting the separation of the wires appears to be an excellent scheme. I have used flat surfaces of brass rather than jaws for clamping the top of the suspension from fear of pinching the wire too much and weakening it. Tungsten is so hard that it is possibly difficult to damage even fine wires by any clamps made of brass or other soft metal. The essential difference in detail as regards the suspension between Mr. Ockenden's arrangement and mine is that he provides a loose pulley at the top to equalize the tension of the two wires. After this is done they are rigidly clamped. Assuming they divide the weight equally, any possible later deformation which would cause a difference in tension between the two wires, would result in a diminished controlling force for a given deflection. In the case of a separate bridge-piece at the bottom, such as I have used, the distribution of the weight is automatic. There is probably nothing to choose between the two systems if they are used intelligently. The remarks on damping are a valuable contribution to a very important subject in connection with instruments of this nature.

THE LONG-DISTANCE TELEPHONE SYSTEM OF THE UNITED KINGDOM.

By Sir WILLIAM NOBLE, Member.

(*Paper first received* 16 *December*, 1920, *and in final form* 10 *February*, 1921; *read before* THE INSTITUTION 17 *March*, *before the* SCOTTISH CENTRE 8 *March*, *before the* NORTH-WESTERN CENTRE 22 *March*, *and before the* LIVERPOOL SUB-CENTRE 11 *April*, 1921.)

SUMMARY.

This paper describes the development of the trunk telephone system since 1905.

It deals mainly with the improvements in line plant design, especially in underground cables, which have been made in the past 10 years.

The expansion of the trunk-line system since 1905 led to congestion of the pole lines along roads, railways and canals; and the difficulty of finding new routes increased. Improvements in underground long-distance telephone cables led to a solution of this difficulty; and the new trunk-line system is to be almost entirely underground.

Some details are given of the early work of Post Office engineers in devising methods and apparatus for balancing underground cable circuits so as to render them free from mutual interference or cross-talk. This early work led up to the modern precision methods of cable balancing which were first applied to a cable laid between Leeds and Hull in 1913. Details are given of the second balanced cable, namely, that laid between London and Liverpool in 1914; and the problems created by the success of this cable are discussed.

The telephone repeater became a practical proposition when the 3-electrode thermionic amplifier was evolved; and the general introduction of telephone repeaters into long-distance lines has led to what is almost a revolution in long-distance communication schemes.

The methods of duplexing the thermionic amplifier in order to obtain both-way working are described, and details are given of the methods of measuring the impedance/frequency characteristics of telephone lines for the purpose of constructing the artificial line balances required for duplex working.

The scheme of introducing telephone repeaters into the trunk-line system is explained and the economies to be effected in line plant are discussed.

A map of the proposed main underground-cable system is given.

In submarine telephone-cable work there has been continued progress since the reading of Colonel O'Meara's paper in 1910, and some details are given of the latest developments, especially in the direction of continuously loaded cables.

The paper concludes with a reference to the introduction of high-frequency carrier-wave telephony and its application to long-distance communication.

The subject of this paper has not been brought before the Institution since 1905, when Sir John Gavey (then Mr. Gavey) in his Inaugural Address * referred to the trunk telephones. Previous to that Address, the *Journal* of the Institution contains only one paper dealing with the subject, namely, Mr. Gavey's paper on "The Telephone Trunk System in Great Britain" presented in November 1896.†

* *Journal I.E.E.*, 1906, vol. 36, p. 4.
† *Ibid*, 1896, vol. 25, p. 624.

I feel, therefore, that no apology is necessary for introducing the subject after this lapse of time, especially as it is one that has recently received considerable attention in the public Press and technical journals. In his inaugural address in 1905 Sir John Gavey referred to two fundamental problems which at that time awaited solution: first an effective telephone relay or repeater, and secondly a method of extending the range of communication in submarine cables.

The second matter was dealt with by Colonel W. A. J. O'Meara in December 1910 in his paper * read before the Institution, entitled "Submarine Cables for Long-Distance Telephone Circuits." In the interval between 1910 and the present time a solution of the first-named problem has been found, and this has led to the developments which I propose now to describe.

During the past 4 or 5 years the whole art of long-distance telephonic transmission has been almost completely revolutionized by the evolution of the three-electrode thermionic tube amplifier and its use in the telephone relay or repeater. An earlier opportunity would have been sought to describe the character of this revolution in view of its far-reaching effects on the design and manufacture of telephone plant of all kinds, but the circumstances of the Great War made such a course impossible.†

My task this evening, however, is much facilitated by the fact that since the Armistice was declared several papers dealing with the thermionic valve amplifier have been printed in the *Journal* of the Institution,‡ and the subject has received widespread attention in the technical Press. I am therefore able to assume that the theory of the thermionic valve and the methods of using it as an amplifier of telephonic currents are well known to the members of the Institution: and consequently I may deal more fully with the effects of its introduction into telephone engineering.

At the present time long-distance telephony divides along two main lines of development, namely:—

(*a*) Communication between the principal commercial centres of the country.

(*b*) Communication with remote districts, and international communication.

In the former the demand is for the provision of large numbers of circuits over distances up to 450 miles, e.g. London and Manchester, London and Glasgow; whereas in the latter the requirement is

* *Journal I.E.E.*, 1911, vol. 46, p. 309.

† B. S. COHEN and J. G. HILL: "Long Distance and Cable Telephony," *Journal of the Institution of Post Office Electrical Engineers*, vol. 9, p. 1. J. A. FLEMING: "Improvements in Long Distance Telephony," Royal Institution, March 1914.

‡ R. L. SMITH-ROSE: "The Evolution of the Thermionic Valve," *Journal I.E.E.*, 1918, vol. 56, p. 253. L. B. TURNER: "The Oscillatory Valve-Relay" *Journal I.E.E.*, 1919, Supplement to vol. 57, p. 50. B. S. GOSSLING: "Development of Thermionic Valves for Naval Uses," *Journal I.E.E.*, 1920, vol. 58, p. 670.

for a very few circuits, in most cases one or two only, over much longer distances, e.g. London to Belfast, London to Brussels, Paris and Marseilles, and in due course to Rome, Madrid, Constantinople or even Cairo.

Each line of development presents distinct problems of engineering design.

In the first decade of the present century it appeared that the ultimate possible efficiency in long-distance telephone transmission had been reached; and that it was attainable only by the employment of the heaviest practicable copper conductors, erected on aerial lines, highly insulated, and revolved in pairs or quadruples to reduce inductive disturbances to a minimum. In this country the maximum economical conductor was proved to be one weighing 600 lb. to the mile. A few circuits with 800-lb. conductors had been erected in the last decade of the 19th century, but their very small advantage in transmission efficiency as compared with 600-lb. conductors within the distances to be covered in this country, did not compensate for their much greater capital and annual costs.

On the basis of a total attenuation of $Bl = 2{\cdot}5$ (representing a ratio of about $7\frac{1}{2}$ per cent as between the received and transmitted currents, that is a loss of $92\frac{1}{2}$ per cent) as being the limit for a commercial long-distance line, the maximum length of a 600-lb. circuit was about 900 miles.

In this country, however, it was never possible to obtain the maximum efficiency of an aerial circuit on account of the necessity for short lengths of underground cables in passing through intervening town areas or across physical obstructions such as river estuaries, etc., and 60 per cent efficiency was the average obtained in practice.

Even under these conditions good commercial service was given between centres so far apart as London and Glasgow, London and Dublin, London and Paris, and London and Brussels; and the lines available for these services had a fair margin of efficiency for extension to minor telephone centres in their respective areas.

From the time of the taking over by the Post Office of the trunk telephone service in 1896 until about 1910 it was possible to accommodate on pole lines along the public roads and railways all the wires required to meet the public demand for trunk service. The following table shows the increase in the number of trunk circuits radiating from London to the more important provincial centres.

It will be realized that the pole lines carrying these circuits radiating from London must carry in addition the shorter circuits connecting towns lying on the routes.

For example, one of the London–Manchester lines passes through Northampton, Leicester, Derby and Buxton, and carries circuits serving those towns. Moreover, the requirements of the telegraph service have also to be met.

The congestion on the main aerial lines was becoming serious in 1910, and it was practically impossible to find road routes for new pole lines, especially in the approaches to London. The traffic-development studies undertaken about that time forecasted a demand for trunk lines which could not possibly be met by any ordinary system of road pole-line construction, and a scheme of direct cross-country routes of steel tower construction capable of carrying very large numbers of conductors was prepared. At this juncture, however, the results of a long series of experiments on the loading of telephone circuits with inductance coils, in accordance with the principles described by Pupin in 1900, indicated another line of development of the trunk telephone system. The earlier experiments with loading in this country were made with inductances of the air-core type, but these were replaced later by the improved iron-core types introduced by the Western Electric Company of New York.* Under the favourable climatic conditions of North America, the American Telegraph and Telephone Company had obtained an improvement factor of 2·5 on the transmission efficiency of aerial lines, but in this country the climatic conditions were much less favourable, and even if the American standard had been attainable, and the weight of copper in aerial circuits correspondingly reduced, the problem of relieving congestion of the pole lines would have still remained.† The results obtained by the loading of underground telephone cables with inductance coils were, however, more encouraging, and the attention of the Engineering Research Department of the Post Office was therefore specially directed to this line of development. The manufacture of dry-core paper-insulated lead-covered telephone cables had already reached a high stage of perfection, and the cables produced by the manufacturers of Great Britain were, and are still, the best obtainable in the world.

TRUNK CIRCUITS RADIATING FROM LONDON TO PROVINCIAL TOWNS.

	1902	1905	1908	1914–15	1920	Estimated 1925	Estimated 1930
Birmingham ..	4	7	14	32	37	60	76
Liverpool ..	8	10	14	35	37	63	80
Manchester ..	6	6	10	17	21	47	61
Leeds	2	2	9	13	14	20	24
Newcastle ..	1	2	8	10	9	14	17
Glasgow ..	4	4	4	6	6	10	12
Bristol	3	5	6	15	17	27	30
Cardiff	2	4	10	14	18	26	29
Southampton ..	1	4	6	11	11	20	25

The history of the development of the long-distance telephone cable is a record of undaunted perseverance in the solution of scientific and practical engineering problems which cropped up persistently as progress was made, and which frequently appeared to be insuperable. It will be instructive to review the various stages of progress in this development, and to recall the difficulties and the methods of overcoming them.‡

* J. G. HILL: "Telephone Transmission"; A. W MARTIN: "The Loading of Telephone Cable Circuits," *Institution of Post Office Electrical Engineers, Professional Papers.*

† J. G. HILL: "The Loading of Aerial Lines and their Electrical Constants," *Ibid.*, No. 54.

‡ J. A. FLEMING and G. B. DYKE: "The Power Factor and Conductivity of Dielectrics," *Journal I.E.E.*, 1912, vol. 49, p. 323.

The earliest underground cables used in the long-distance telephone system were of the same type as those used up to that time in the telegraph service for subterranean lines. They were made up with copper conductors weighing 40 lb. to the mile, covered with 50 lb. of gutta-percha to an overall diameter of 174 mils. For telephone purposes these covered wires were laid up in quad formation around a jute core with an outer wrapping of tarred tape, the circuits being formed on the diagonals. These cables gave circuits with the following constants (per mile of loop) :

R = 44 ohms.
C = 0·2 microfarad.
L = 1·5 millihenrys.
Insulation = 1 000 megohms.
Attenuation constant = 0·12 at 800 cycles per second.

The inefficiency of transmission of these cables restricted their use in the telephone trunk lines to the terminal sections in large cities, and to the leading-in at testing points.

Rubber-insulated conductors were also used to a very limited extent, but were found to give less satisfactory results in practice than the gutta-percha cables.

The introduction of air-space cables with paper insulation and lead sheathing in the early nineties of last century appeared to offer a solution of the difficulty in carrying telephone circuits underground, although the first practical experiments with this type of cable were somewhat discouraging. Numerous breakdowns of underground air-space paper cable sections occurred, and until the causes had been discovered and proper remedies found, maintenance engineers were very reluctant to replace their old gutta-percha cables with the air-space type.

Apart from questions of workmanship in drawing in and jointing underground cables, the comparison between the two types was narrowed down to their respective susceptibilities to damage by lightning, and in this respect the gutta-percha cables appeared to hold a distinct advantage.

Short sections of underground cables connected with long lengths of highly-insulated aerial wires form effective spark-gaps for lightning discharges unless very efficient protectors are installed at the ends of the cable sections. At the time of the introduction of air-space cables into the telephone trunk system, the standard type of lightning protector for underground sections was a small vacuum tube with platinum electrodes (Fig. 1). These tubes were mounted in teak weatherproof cases fixed on the terminal poles at the junctions of cable and aerial line sections. The tubes were exhausted to a vacuum of about 2 mm. of mercury. In spite of regulations for the frequent testing of these tubes, especially after thunderstorms, faults in underground cable sections caused by lightning were numerous : but whereas in the case of gutta-percha cables the fault would be limited to perhaps one or two circuits only, in the case of air-space cables the lead sheath was melted and sooner or later moisture entered and the whole of the circuits in the cable were stopped. The replacement of the faulty gutta-percha cable was a comparatively easy and speedy matter, whereas the repair of the air-space cable was frequently a difficult and tedious operation entailing a considerable loss of revenue-earning time on the circuits involved. Considerable experience had already been obtained in the use of air-space underground cables for short telephone circuits in city areas, and their superiority over gutta-percha cables for this class of service had been proved. Damage by lightning was unknown in the case of wholly underground circuits. The design of a more efficient lightning protector for trunk cable sections and improved workmanship in handling and jointing lead-sheathed cables soon established the air-space cable for trunk-line purposes, and at the end of the last century gutta-percha subterranean telephone cables were obsolescent, all renewals being effected with air-space cables.

The superior transmission efficiency of the air-space cable, and the fact that much heavier conductors could be used as compared with gutta-percha cables, permitted longer underground sections in the trunk routes, and so afforded a solution (although only a partial one) of the increasing difficulty in carrying heavy main routes through urban areas and into the main terminal towns : but with the extended lengths of underground sections, a fresh difficulty, which had

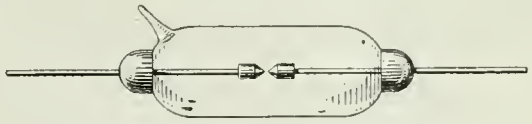

Fig. 1.—Vacuum-tube lightning protector.

not been apparent in gutta-percha cables, cropped up, namely, increased cross-talk between adjacent circuits.

In the gutta-percha cables the mean distance between loop circuits and the comparatively short lengths of underground sections were such as not to increase perceptibly the mutual disturbance between neighbouring circuits ; but the close packing of the conductors of the air-space cables and the increasing length of cable sections introduced conditions of mutual disturbance which had serious effect on the commercial efficiency of the long-distance circuits.

At the outset this limit to the use of air-space cables for long-distance telephone circuits appeared almost insuperable, but in view of the importance of a solution of this problem, special attention was given to it by Post Office engineers.

Messrs. F. Tremain and A. W. Martin between July and October 1899 made an exhaustive investigation of the electrical properties of a 93-mile section between London and Leamington of the first London–Birmingham air-space underground cable.

This investigation had for its principal object a comparison of the telephonic cross-talk between telephone circuits and inductive disturbances between telegraph circuits in the two types of construction used in this section of cable. This section was made up, broadly speaking, of 28½ miles of quadruple-core formation from Cricklewood to Eddlesboro', the remainder of the section being made up of wires twisted

in pairs. Although every effort was made by the cable manufacturers to produce symmetrical cable with the view of minimizing cross-talk it was found that speech telephonically transmitted over a loop made up of diagonal wires in a given core of the quadruple formation could be heard in a telephone joined to the other pair of wires in the same core; but between diagonal pairs of neighbouring cores, no such cross-talk was observed in the $28\frac{1}{2}$-mile section tested. This result was attributed to the distortion of the quadruple core, and it was then concluded that the defect in this type of air-space cable was too difficult, if not impossible, to remedy in the course of manufacture. Attention was then directed to the cross-talk observed between neighbouring loops in the pair-type formation, and it was found that the cross-talk was practically negligible between two circuits of 52 miles in length when these circuits did not run for more than $5\frac{1}{4}$ miles in adjacent pairs. This ratio, i.e. 10 per cent of length of adjacent wires to length of the whole circuits, was considered significant and was used as a guide later in the investigation, for arranging crosses to eliminate or reduce cross-talk disturbances.

The investigators found in their experiments with the 28 miles of quad-core cable that they could eliminate cross-talk between the two circuits carried by a quad core, by making crosses of individual wires, and pairs of wires, at 6 intermediate points in the 28-mile section. They employed 7 distinct varieties of crosses, namely, crosses A, B, and C in which the individual wires in either or both pairs are crossed; cross D, in which one pair of wires is crossed with the other pair; and crosses E, F, and G in which the pairs themselves are crossed in addition to the individual wires of one or both pairs. This systematic method of crossing exhausted the possible number of crosses which could be made in joining up the four wires of a quad core. Messrs. Tremain and Martin have pointed out that 42 possible single crosses could be made at the 6 intermediate crossing points without making a cross from one core to another. By making a total of 650 crosses, of individual wires and pairs at the intermediate crossing points, they succeeded in making this 28-mile section free from telephonic overhearing or cross-talk without necessitating any crossing of cores, a consideration which, at that time, was of some practical value. The investigation of cross-talk in the pair-type formation indicated that no appreciable cross-talk would occur when crosses of pairs were made at intermediate crossing points, to ensure that no two pairs of wires remained parallel and adjacent for more than 10 per cent of the total length of the circuit. At a subsequent date a systematic method of balancing by crossing cores wrapped with paper of different colours was adopted in order to satisfy this condition. The balance obtained by this method of crossing proved so effective that the telephone loops in the central portion of the cable were not seriously interfered with by single-wire telegraphs in the outside layer, nor vice versa. It was also observed that a key-speed telegraph circuit worked on a loop did not interfere with the telephone circuit in an adjacent loop. Even Wheatstone working on a loop did not interfere with the telephone circuit, provided one pair of wires was interposed between the two circuits. It was further reported in this experiment, that so long as the rule was observed that no two pairs of wires were parallel and adjacent for more than 10 per cent of the whole length of the cable, loops could be used indiscriminately for telephone or telegraph circuits.

It may be mentioned that before these results were obtained the investigators found it necessary to measure the electrostatic capacity of every wire in every section of the cable and, similarly, measurements of copper resistance were made to the highest accuracy obtainable with the apparatus then available, the figures being recorded to 1 in approximately 10 000 parts for capacity, and 1 in 5 000 parts for resistance.

The jointing procedure adopted as the result of the foregoing experiments fulfilled all requirements of the Post Office, until the method of improving the efficiency of telephone circuits by inserting inductance coils was adopted. The placing of loaded telephone circuits in the underground cables necessitated a reconsideration of the practice of providing telegraph and telephone circuits in the same cables, owing to the fact that loaded circuits, whilst being more efficient than unloaded ones, are at the same time more susceptible to inductive disturbance. Experiments showed that Wheatstone circuits on single wires caused serious disturbance to loaded telephone circuits in the same cable, and in some cases marked induction had been experienced on such circuits from Wheatstone circuits worked on loops. In consequence of these difficulties it became necessary for a time to provide separate cables for telegraph and telephone purposes. As the trunk underground system grew it was found uneconomical to provide separate cables for the two services, and special efforts had to be made to overcome the difficulties experienced. The method of superposing loaded telephone circuits on other telephone circuits already loaded was also found impracticable with the same degree of balance in the conductors which was found sufficient for unloaded circuits. In 1912 it was found with the usual method of cable construction that the practice of placing telegraph circuits in the same cable as loaded telephone circuits would have to be abandoned, and that phantom loaded telephone circuits could not be successfully superposed upon loaded physical telephone circuits without improving the symmetry of the conductors in underground cables or improving the balance of conductors by selective jointing.

As a consequence of the difficulties experienced, the whole question of cable design and balancing by selection of conductors for jointing was taken in hand afresh by the Research staff of the Post Office. Early in January 1913, Mr. Pollock initiated an investigation of the relation which existed between cross-talk, and wire and wire, and wire and earth capacities in quad-core and multiple twin cables when tested in short lengths, and devised apparatus for making the capacity measurements with alternating current of telephone frequency with the necessary degree of convenience and accuracy. Short lengths of cable were specially constructed with a great variety of different lays in the paired conductors, and it was shown that whilst

certain selected lays gave favourable experimental results, the necessary symmetry in the spacing of conductors could not be obtained by cable manufacturers to ensure a sufficiently good balance of the various capacities between conductors and between conductors and earth. An approximate formula was obtained for connecting the amount of cross-talk between circuits in the cable with the capacity relations of the conductors, and a systematic method was worked out by which the unbalances of capacity in one short length could be greatly reduced by selecting wires in contiguous lengths having nearly equal and opposite characteristics, so that the conductors when joined together would neutralize each other's defects. This plan only differed in degree and detail, but not in principle, from the method of balancing previously adopted by Messrs. Tremain and Martin. In order to obtain the required degree of balance for loaded and superposed circuits it was found necessary to test every drum of cable of approximately 176 yards in length, instead of sections of approximately 5 miles, and pre-select the wires for jointing at every 176 yards by inserting crosses as between core and core, in addition to crosses as between individual wires and pairs. The system of testing and selection is necessarily laborious and complex, and a number of the most able officers in the Research Section of the Post Office have contributed most valuable improvements, both to the mathematical theory and to the practical methods of cable balancing.*

The modern method of balancing underground cables which require to be loaded and to carry superposed loaded circuits was first applied to an underground cable in this country in 1913, when it became necessary to lay a composite telephone and telegraph cable between Leeds and Hull to replace the aerial wires which previously existed. This cable of approximately 52 miles in length was begun in June and handed over for traffic on the 23rd November, 1913. This cable is interesting, inasmuch as it involved the making of over 90 000 capacity tests under field conditions, and because every individual mile of the cable throughout the whole route was balanced to a very high degree of accuracy. The cable without any modification or adjustment was found suitable for working simultaneously, without cross-talk or disturbances of any kind, loaded physical telephone circuits, loaded superposed or phantom circuits, and high-speed Wheatstone automatic telegraph circuits. A description of the system of balancing employed on this cable is beyond the scope of this paper, but it is fully described in one of the technical publications of the Post Office, which will shortly be available to members of the public by purchase from His Majesty's Stationery Office. In the meantime, copies have been supplied to the Institution Library.

Opportunity might be taken at this point of publicly expressing appreciation of the work of a recently deceased member of the Post Office Engineering Research staff who made many notable contributions to the science of telephone speech transmission. I allude to the late Mr. Charles E. Hay. His work on the design of apparatus and methods for precision measurements by the alternating-current bridge was most valuable.*

The programme of construction for 1914 included the provision of a main cable-duct route from London to Liverpool via Birmingham, and the first cable on this route was commenced in that year and completed as far as Birmingham in June 1915 and extended to Liverpool in 1916.† The cable was designed to give 30 circuits between London and Liverpool, and 42 between London and Birmingham and Birmingham and Liverpool (see Appendix I).

The results obtained from this cable removed all doubts as to the future success of the underground trunk cable in relieving the congestion on the aerial lines, but it soon became evident that the very success of the first cable had opened up some new problems.

The quality of service afforded by this cable in comparison with that previously given by aerial lines led to a rapid increase in traffic between the centres connected by the cable. Not only were the individual circuits more "silent," i.e. free from the background of extraneous noise usually experienced on long aerial lines, but the whole groups of cable circuits between the respective centres were more reliable; they were unaffected by the adverse weather conditions which so frequently interrupt aerial lines in this country. Whereas a loss of 10 to 15 per cent of working time was a common experience on long aerial lines, the loss on these cable circuits was nil.

On more than one occasion during the war period, when the Post Office engineering maintenance staff had been depleted to the utmost for service with the military forces, and interruption intervals on aerial line routes were unavoidably prolonged, the London–Liverpool cable saved the telephone trunk system from serious breakdown.

This was notably the experience after the extraordinary blizzard which caused so much havoc in March 1916. I suppose that most of us have some knowledge of the effects of that storm, although the Press censors prohibited the publication of details at the time, but as the necessity for withholding facts has now passed, some description of the effect of this storm on the national communications may be of interest.

The storm swept across the country in a direction north-east to south-west, and reached a force of 75 miles an hour over a belt of nearly 100 miles in width between the Wash and the Bristol Channel. It reached the east coast about 7 o'clock on the evening of March 27th, and was accompanied by a heavy fall of snow. Within little more than two hours practically every pole line inside that belt had been destroyed, and roads, railways and canals were blocked with the poles and wires thrown across them. It was an unparalleled disaster in the record of the telegraphic and telephonic services of this country.

The main telegraph cables from London to the North saved the situation so far as that service was concerned, and the London–Liverpool telephone cable, with the spurs from Birmingham to Sheffield and Leeds, and from Liverpool to Manchester, enabled the engineers

* *Journal of the Institution of Post Office Electrical Engineers*, vol. 7, p. 41. J. G. Hill: "The Loading of Underground Telephone Cables." *Institution of Post Office Electrical Engineers, Professional Papers*, No. 68.

* C. E. Hay: "Alternating Current Measurements," *Institution of Post Office Electrical Engineers, Professional Papers*, No. 53.

† *Journal of the Institution of Post Office Electrical Engineers*, vol. 7, p. 32; and vol. 9, p. 130.

to patch up lines from London to all the important towns of the North Midlands and the North, and to keep them in telephonic communication with London at a time when such service was of vital importance.

Returning now to the consideration of the effects resulting from the success of this premier cable, it can be understood why, within two years of its completion, every available circuit had been allocated, and the question of providing a second cable arose. Three ducts had been laid along the route, this provision being in accordance with calculations of future requirements based on previous experience. It was evident that unless some new development in telephone engineering intervened, the duct route would be filled within a very short period. Moreover, the desirability of extending the underground system further north than Liverpool was apparent. The largest practicable main telephone cable has a diameter of about 2·9 inches and can contain copper conductors to a total weight of about 15 000 lb. per mile. Standard 176-yard

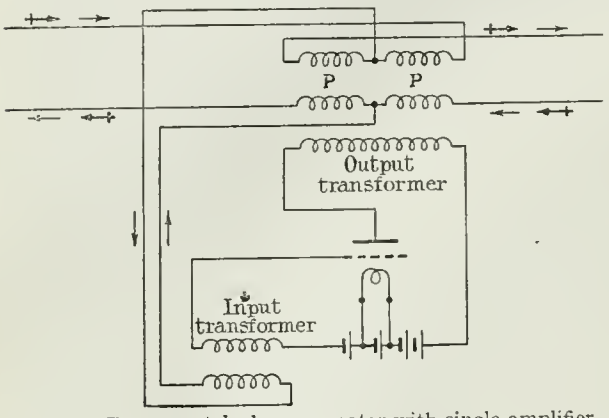

FIG. 2.—Two-way telephone repeater with single amplifier.

lengths of this cable weigh about $2\frac{1}{4}$ tons net, and about $2\frac{3}{4}$ tons gross when drummed for transport. For direct service between two centres such as London and Liverpool, requiring lines having a transmission equivalent of 18 miles of standard cable (i.e. $\beta l = 1 \cdot 926$) between terminal test-boards, such a cable will carry 25 four-wire cores or quads of conductors, providing a maximum of 75 circuits including phantoms.

For through-switching purposes between the same points, circuits having a transmission equivalent between main test-boards of 12 standard miles (i.e. $\beta l = 1 \cdot 28$) are necessary, and on this basis a full-sized cable will carry 19 quads of conductors, giving a maximum of 57 circuits including phantoms.

In order to obtain the necessary quality of transmission for direct service between centres 400 miles apart, e.g. London and Glasgow, it would be necessary to use conductors of much heavier gauge, and a full-sized cable for such a service would carry no more than 12 quads, giving a maximum of 36 circuits including phantoms, but it is not at all certain that such a cable would be a practical manufacturing proposition.

It appeared therefore that it would be necessary to increase considerably the capacity of underground duct routes in order to cope with the expansion of traffic which would follow on the extension of the underground system.

At this juncture, however, the solution of one of the fundamental problems of long-distance underground telephony, namely, the construction of an effective telephone relay or repeater, seemed possible.

The conception of a repeater which could be inserted in a telephone circuit and fulfil the same functions as a repeater in a telegraph circuit is almost as old as the telephone itself. From the earliest days of the telephone art many investigators had worked patiently on this problem, and the records of the British Post Office contain reports of long and patient trials of innumerable telephone repeater devices submitted by inventors from time to time. Prior to the introduction of the three-electrode thermionic valve, these devices were invariably of the microphonic type, in which a sensitive microphone was attached usually to the reed or diaphragm of a telephone receiver. Seeing that the energy available in a telephone circuit for operating a repeater is of the order of a few microwatts, it seems almost incredible that the inventors of microphonic repeaters should have been able to

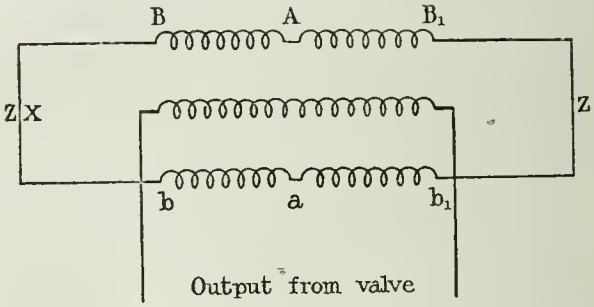

FIG. 3.—Line circuit of telephone repeater closed through two equal impedances.

design anything that would function as an amplifier of the minute currents of speech frequency, and the fact that several such devices have been actually used in commercial circuits with moderate success is sufficient testimony to the patience and ingenuity of the early workers. Chief among these were Mr. S. G. Brown in this country and Mr. H. Shreeve in America. Mr. Brown's telephone relay is well known and was described in his paper read before the Institution in 1910. In the discussion on that paper Mr. Kingsbury made an interesting reference to the work of Mr. Shreeve. In the light of our present knowledge and the possession of an almost perfect amplifier, it can be seen why the early attempts were not successful.

Our present knowledge of the laws of the electrical transmission of speech over wires is due very largely to advances in physical science which in themselves are not specifically associated with telephony and which have preceded the development of the thermionic amplifier.

The introduction of the three-electrode thermionic tube completely changed the aspect of the telephone repeater problem. The early experiments of the Post Office commencing in 1913 were conducted with "soft" valves which, while not wholly suitable for the requirements of a commercial repeater, enabled engineers to

make considerable headway in the design of repeater circuits, and emphasized the necessity for more complete measurements of the impedance characteristics of telephone lines over the whole range of speech frequencies.

The "soft" valve has now been superseded by the "hard" valve for all telephone repeater work (see Appendix II). The ideal thermionic valve for telephone repeater service should fulfil the following conditions :—

(*a*) Its general design should be such that it can be manufactured in large quantities with uniform electrical characteristics.

(*b*) Its electrical characteristics should remain constant throughout its working life.

(*c*) Small variations in filament current due to the normal voltage-drop of the filament battery during discharge should cause no appreciable change in the amplifying power of the valve.

(*d*) Its amplifying factor, in association with its input and output transformers, should be practically constant over the range of speech frequencies, and over the required range of input voltage amplitude.

(*e*) The electrodes and their supports should be sufficiently rigid to ensure that no vibration from external sources affects the performance of the valve.

(*f*) Power consumption in the filament should be as small as possible.

(*g*) Long operating life.

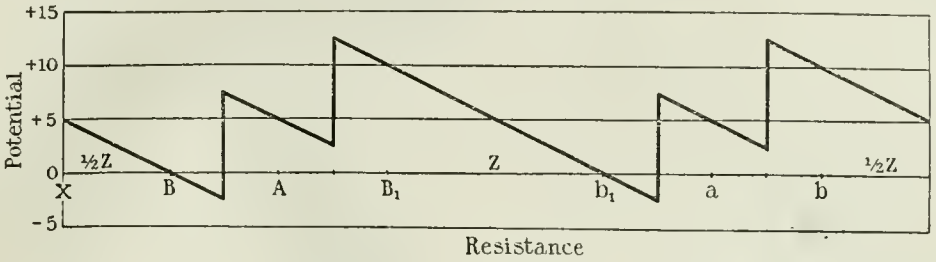

FIG. 4.—Diagram showing that A and a (Fig. 3) are equipotential points.

The thermionic valve with its associated input and output transformers has only a unilateral action as an amplifier. For use in the ordinary two-wire telephone circuit it must be duplexed in order to transmit and amplify speech currents in both directions through the circuit. The duplexing is effected on the principle of the duplex telegraph relay, by the use of differential winding on the line side of the output transformer (see Fig. 2).

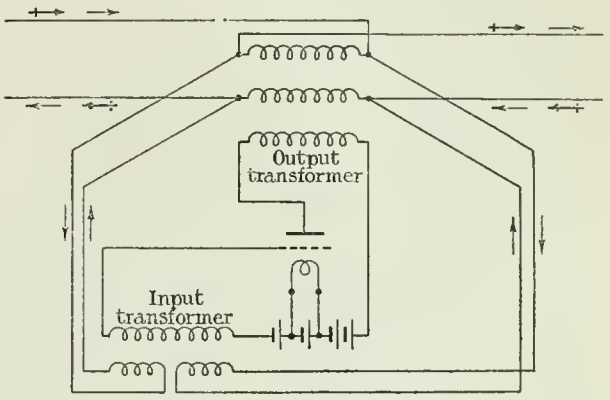

FIG. 5.—Two-way telephone repeater with single amplifier (alternative arrangement to Fig. 2).

Speech currents from the up or down lines pass

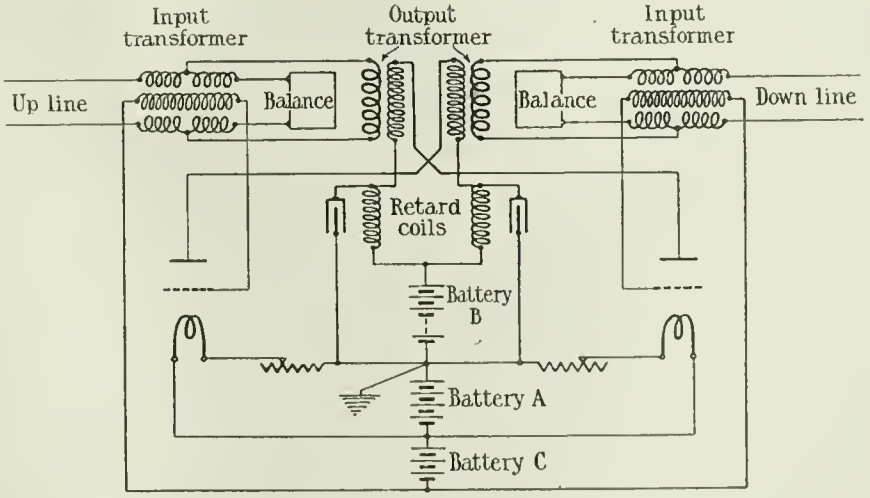

FIG. 6.—Two-way repeater with separate amplifier for Up and Down Lines.

around the line windings P, P of the output transformer in series. The primary of the input transformer being joined across the middle points of the windings is a

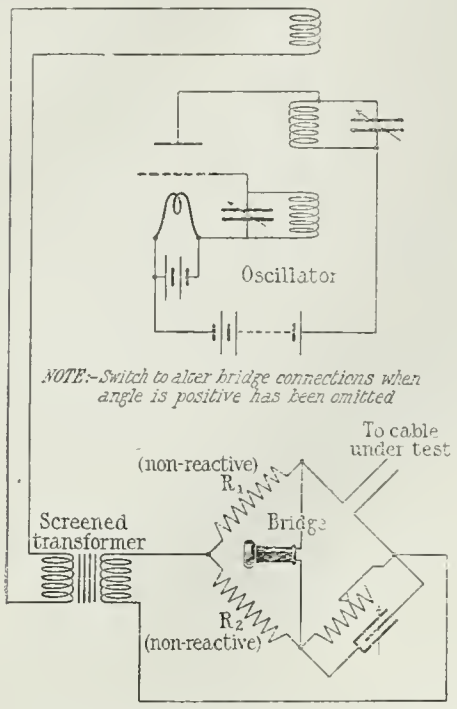

FIG. 7.—Simplified diagram of bridge and oscillator for impedance measurements.

shunt on the line circuit, and a portion of the line currents passes through it and is amplified by the valve. The amplified outgoing currents are transmitted to the up and down sides of the line in series, and if the two sides are electrically equal the potentials at the middle points of the windings P, P are equal, and no part of the amplified current is fed back into the input circuit.

If the repeater circuit be represented by Fig. 3 it will be seen that in a properly constructed transformer with an equal number of turns in all the windings AB, AB_1, ab, ab_1, the E.M.F. generated in all the windings by an alternating current flowing through the secondary will be equal. The impedances of the windings will also be equal. If the circuit be completed by the two equal impedances Z, the circuit may be represented in straight-line form as in Fig. 4, the point X being taken as the starting point.

For the sake of simplicity the E.M.F. of each winding is assumed to be concentrated at the centre of the winding. The impedances have been represented to scale. Obviously the total voltage-drop in the circuit must be equal to the total E.M.F. in the circuit. A drop of 5 volts has been assumed between the point X and the point B. There will be a corresponding drop between any two points having the same resistances between them. The total on the scale chosen will therefore be 40, and each winding must therefore produce an E.M.F. of 10 volts. It will be clear from an inspection of the voltage curve that, under the conditions given, the potential of A will be equal to that of a. This will hold good in any case if the circuit is symmetrical about these points so far as E.M.F.'s and impedances are concerned.

An alternative arrangement of the transformer windings is shown in Fig. 5. It will be noticed that if lines of dissimilar impedance be connected to the repeater shown in Fig. 2, the potentials at the mid-points of the line transformer will be unequal. Con-

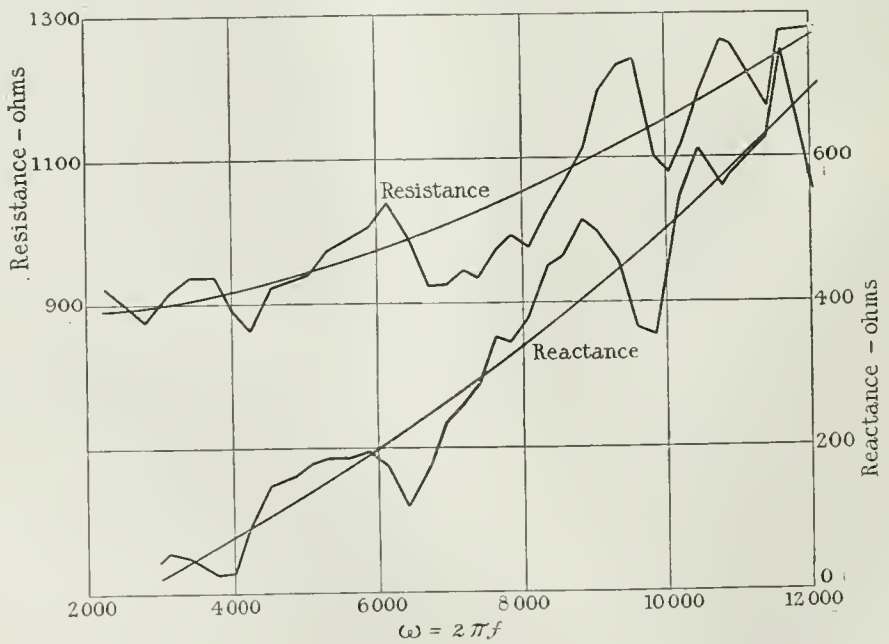

FIG. 8.—Impedance/frequency curves for 70-lb. loaded cable circuit.

sequently an out-of-balance current will flow through the input circuit and, if this current reaches a certain value, the amplifier will generate oscillations at a frequency determined by the electrical constants of the circuit. The resulting noise may be sufficient to make speech impossible between the terminals of the circuit, and in any case will reduce the transmission improvement of the repeater.

In the early days of microphone amplifiers this unbalance difficulty did not cause serious trouble, as

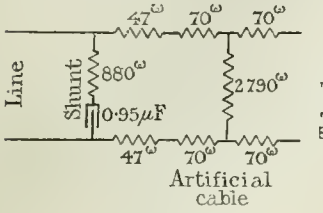

Fig. 9.

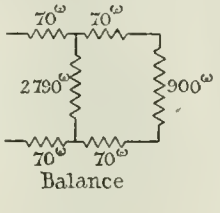

Fig. 10.

Figs. 9 and 10.—Shunt and artificial cable arrangement, and balance associated with telephone repeater on 70-lb. loaded cable.

the amplifiers were relatively insensitive; but since the introduction of the thermionic valve amplifier the unbalance difficulty has become more important. In fact it may be said to be one of the chief difficulties in modern balanced repeaters.

In practice, the use of the simple differential repeater circuit shown in Fig. 2 is limited to those cases where the repeater can be inserted at the electrical centre of a homogeneous and stable line circuit. This condition is obtainable generally only in well-constructed cables. In this country it is seldom obtainable on aerial lines owing to the variations caused by our climatic conditions.

Under the best conditions of a cable circuit the simple differential repeater may be operated to give a transmission improvement equivalent to the suppression of 16 miles of standard cable in a circuit having an equated length of 30 miles of standard cable.

It has been found generally impracticable to use the simple differential repeater for tandem working, owing to the effects of reaction between repeaters at adjacent stations; consequently its use is limited to circuits which are not liable to be switched through to other circuits containing repeaters.

This limitation led to the development of the double differential repeater shown in skeleton form in Fig. 6. In this arrangement the line circuit is divided at the repeater station; each side is terminated in a duplex repeater and balanced by an artificial line designed to represent as nearly as possible the electrical characteristics of the actual line over the range of speech frequency. In practice, absolute balance is not obtained between the actual and artificial lines at all frequencies, but the balance can be made sufficiently accurate by the use of simple apparatus to permit the operation of as many as four repeater stations in tandem, each giving a transmission improvement equivalent to the suppression of 16 miles of standard cable. The minimum residual transmission equivalent of a repeatered circuit using repeaters of this type singly or in tandem is found in practice to be about 12 miles of standard cable.

The problem of constructing artificial balancing lines for telephone repeaters has necessitated an exhaustive study of the impedance characteristics of line circuits.

It was soon found that sudden changes in the composition of a telephone circuit due, for example, to unevenly spaced loading coils, the insertion of sections of line with different conductor resistance, electrostatic capacity, or insulation resistance per mile, introduce irregularities in the impedance/frequency characteristics of the circuit of such a character as to make it practically impossible to obtain an absolute balance between the two sides of the repeater circuit. One of the objects of the improvement in line construction, therefore, is to obtain a homogeneous circuit having impedance/frequency characteristics free from irregularities. This condition is obtained by so designing the circuit that the resistance, capacity, leakance and inductance shall be uniformly distributed throughout the length of the circuit. For satisfactory transmission of speech the circuit should also be practically "distortionless." A distortionless circuit—(*a*), transmits all the important speech waves with equal attenuation; (*b*), maintains the phase relationship between the various frequencies; and (*c*), its impedance is constant over the important frequency range. An artificial balancing line for such a cable would be a plain resistance whose value could be predetermined.

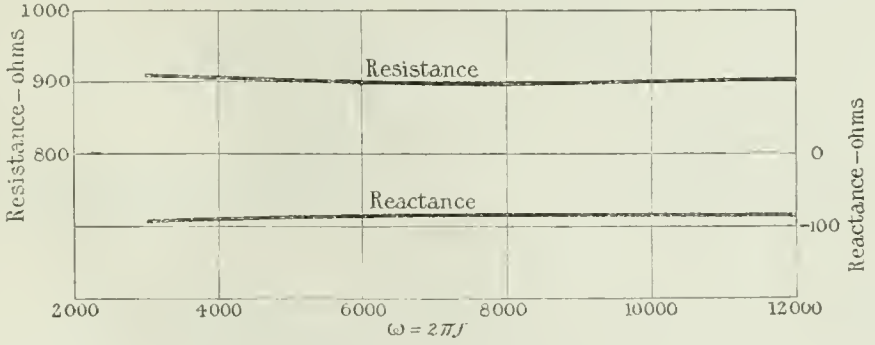

Fig. 11.—Modification of mean curves of Fig. 8 as result of shunt and artificial cable extension shown in Fig. 9.

Hitherto these conditions have not been obtained in practice, especially condition (*c*), and at present it is not practicable to predict accurately the impedance/frequency characteristic of any circuit. It is therefore necessary to measure the impedance of each line, if maximum efficiency is to be obtained from a repeater, to enable the balance to be calculated.

The impedance characteristics of line circuits are

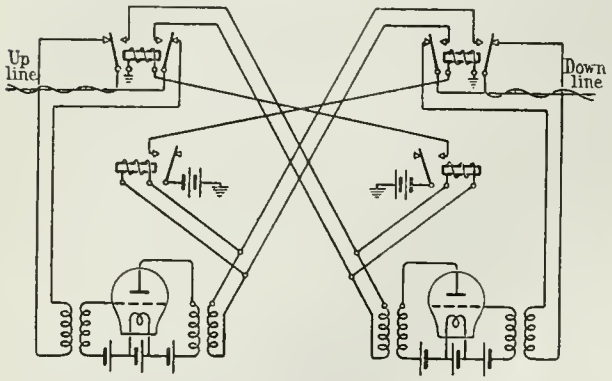

FIG. 12 (*a*).—Telephone repeater with switching relays (Schematic).

obtained from alternating-current bridge measurements made at frequencies ranging from 350 to 2 000 cycles per second. A thermionic valve arranged as an oscillation generator is used as a source of power for the bridge, which is shown diagrammatically in Fig. 7.

Fig. 8 shows the results of measurements on a loop of 70-lb. conductors loaded with 135-mH coils at intervals of 2·6 miles. It will be seen that the mean curves for both the resistance and reactance components rise with frequency. This is due to the loading coil nearest to the office from which the measurements were made being too near to that office, and also to the effective resistance of the loading increasing appreciably with frequency. The irregularities in the curves are due to various causes, such as unequal spacing of loading coils and variations between the inductances of the coils along the circuit. This cable was not designed for repeater working, and in more recent cables special care has been taken to keep the inductance of the loading coils within narrower limits and to maintain accurate spacing. It may be mentioned here that an improved type of loading coil has been introduced by the Western Electric Company, which it is expected will be much less liable to vary in inductance with time, and the effective resistance of which varies much less with frequency.

One method of treating the cable of which the impedance curves have been illustrated is shown in Fig. 9. A condenser in series with a resistance is placed as a shunt across the line, and this produces a modification

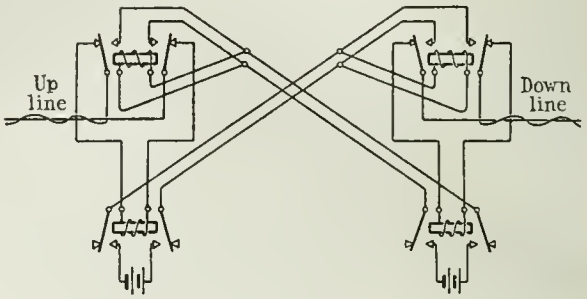

FIG. 12 (*b*).—Telegraph repeater simplex (Schematic).

in the mean curves, as shown in Fig. 11. It will be seen that they are practically horizontal. As the cable in this case is electrically short, an artificial cable is used to extend it, as shown in Fig. 10, and the balance used is shown in the same figure.

The necessity for extending the cable is due to the variations in impedance which result when a short line, that is, a line less than about 15 standard miles in length, is connected to lines or apparatus of differing impedances at the distant end. Such variations upset the balance of the repeater. If the line is sufficiently long, however, such connections do not sufficiently affect the balance to interfere with the working of the repeater.

As repeaters will usually be required to operate in tandem on long circuits, it is necessary that they shall be so designed that reflections are reduced to a minimum, otherwise irregularities will be introduced in

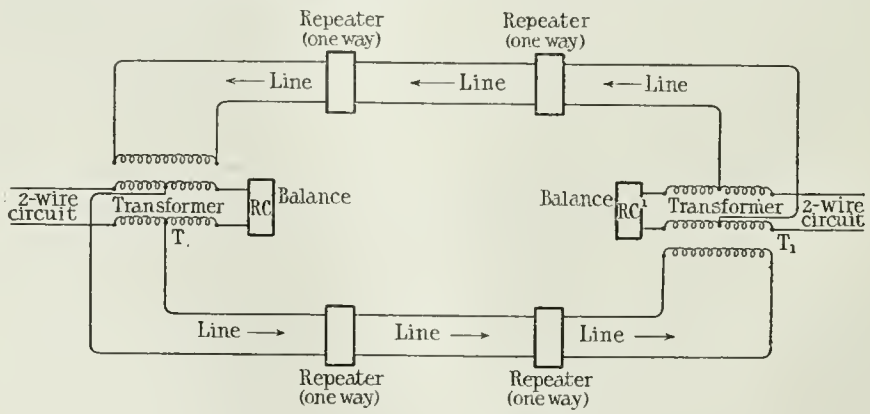

FIG. 13.—Four-wire repeater system.

the impedance/frequency characteristics of the line, as measured from a distant repeater station, and, in consequence of the out-of-balance thus produced at the latter station, the possible amplification will be reduced.

The difficulties in operating circuits using differential repeaters in tandem have induced investigations of other systems for obtaining two-way facilities.

Two such systems will be discussed. The first is an adaptation of the principle of the simplex telegraph repeater arranged for up and down working.

The line is divided at the repeater station, and the up and down sides respectively are led through the tongues and spacing contacts of an electromagnetic switch to the input circuit of an amplifier. The output circuit of each amplifier is cross-connected to the marking contacts of the opposite electromagnetic switch. The electromagnetic switches correspond to the automatic switches of the telegraph repeater, and are similarly controlled by the amplified outgoing currents. The principle of the system is illustrated in schematic form in Fig. 12. This principle has also been applied to wireless telephony.

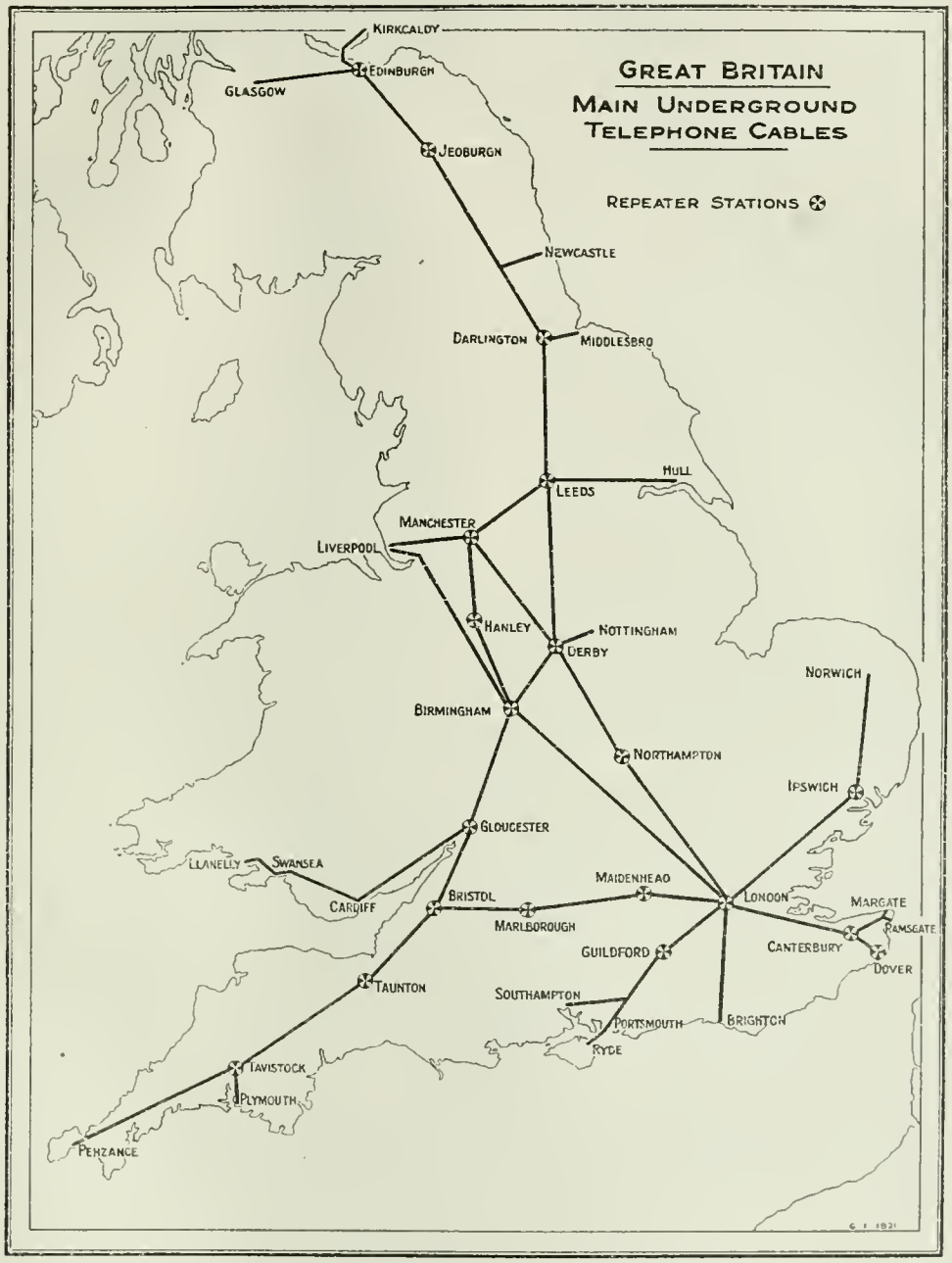

FIG. 14.—Main underground telephone cables in Great Britain.

The second system reverts to the original idea of using separate line circuits for transmitting in the up and down directions respectively. It is generally known as the four-wire system and is shown in schematic form in Fig. 13.

The speech currents circulate in only one direction around the circuit and are relayed by the one-way amplifiers in each side.

side of the four-wire circuit. Under this condition the repeaters could be operated with an amplification limited only by the cross-talk between the two sides of the four-wire circuit, and between adjacent circuits.

In practice the terminal balances can be made sufficiently accurate to enable the repeaters to be operated so as to give a transmission improvement equal to the suppression of an attenuation length not less than

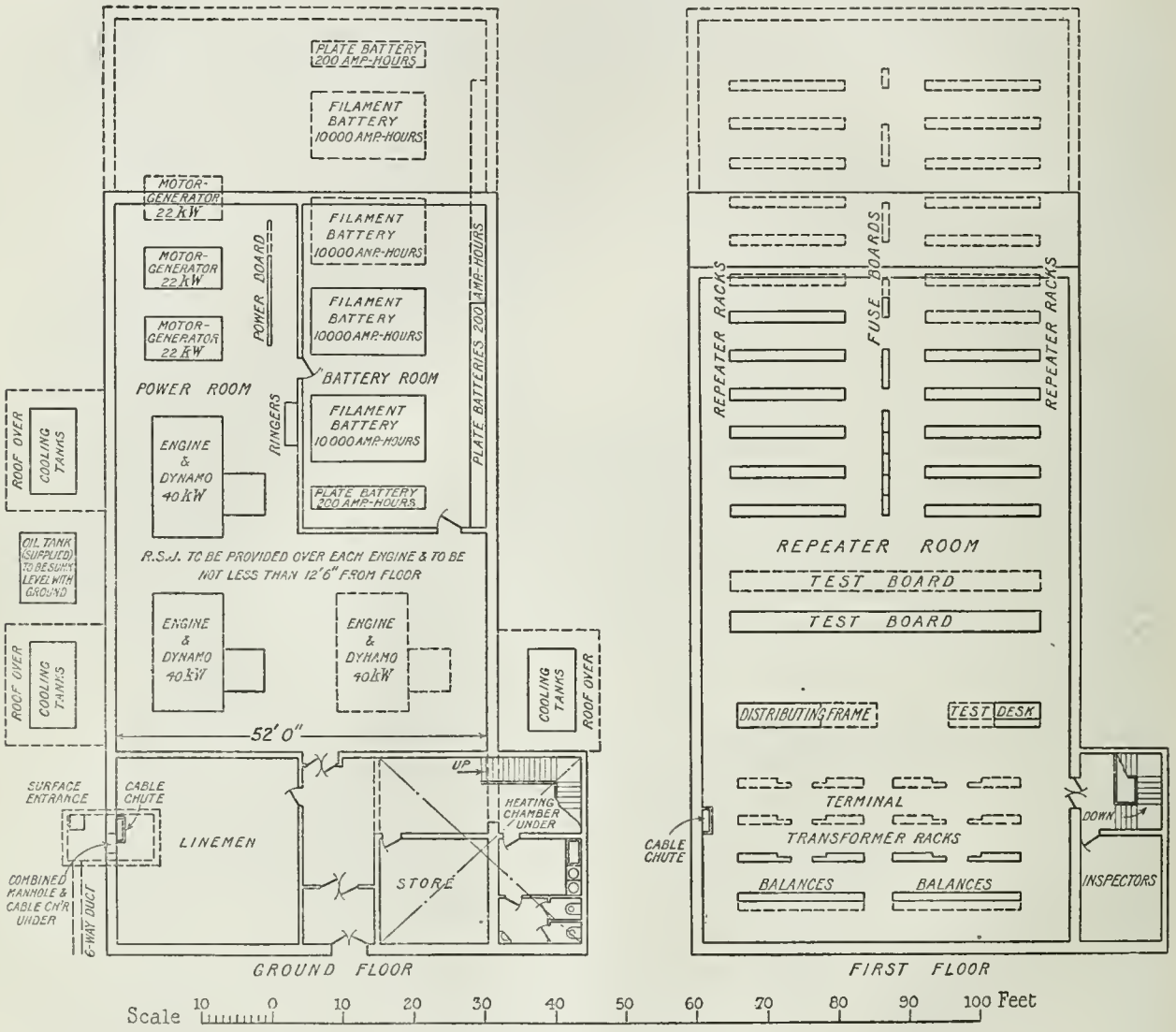

FIG. 15.—Telephone repeater station; proposed arrangement of plant.

The broken lines represent the ultimate building.

The terminal telephones or extension lines are connected with the four-wire circuit by means of the differential transformers T and T_1 and are balanced by the artificial lines RC and RC_1. If those balances are accurate and the transformers are perfectly differential, then the whole of the incoming current from the last amplifier in either side will divide between the terminal two-wire circuit and its corresponding balance, and no current will pass over to the other

that of the total line circuit; that is to say, the overall transmission equivalent of the circuit between terminals may be zero. The repeaters must of course be so spaced along the circuit that the output energy at any station does not exceed the magnitude permitted by the cross-talk factor of the cable.

With modern balanced cables the repeaters may be spaced up to 30 standard miles apart with a reasonable factor of safety for commercial service; consequently

very small conductors may be used. For long-distance cable circuits, beyond the economical range of the two-wire circuit with differential repeaters, the four-wire circuit offers important advantages and will be widely used.

Although the foregoing description of the telephone repeater is necessarily superficial it is thought that sufficient information has been given to facilitate an understanding of the extensive scheme on which the long-distance service in this country is now being developed.*

Reference has been made to the London–Liverpool trunk cable and to the circumstances which led to its provision. Studies of traffic requirements between London and the Manchester and Leeds areas indicated the desirability of the same provision in these cases, and, but for the incidence of the war, a cable of similar capacity and efficiency to the London–Liverpool cable would have been commenced in 1914.

Revision of the scheme in 1918 with our later knowledge proceeded as follows :

The trunk circuits were required to give a transmission efficiency of 12 miles of standard cable between test-boards. This efficiency could be obtained with cable conductors weighing 200 lb. per mile of wire, loaded with 100 mH per mile, and a cable of maximum diameter would provide 42 circuits including phantoms. Such provision would barely suffice for two years' development, and a second cable would have to be commenced immediately on completion of the first. The development at the end of 8 years would require several such cables, for which duct space should be provided at the outset.

Now, assuming the installation of a midway repeater giving a transmission improvement of 14 miles of standard cable ; then for an equivalent of 12 standard miles between terminal test-boards a cable containing 100-lb. conductors would be suitable. A cable of maximum diameter would provide 90 circuits including phantoms. The annual costs per circuit including repeaters would be approximately one-half that of the previous scheme, and the provision of a second cable would be postponed for about 3 years.

Further, assuming the installation of two intermediate repeaters each giving a transmission improvement of 14 standard miles ; then the required efficiency between terminal test-boards could be obtained with cable conductors weighing 40 lb. per mile of wire, and the cable would provide 240 circuits including phantoms. The total annual charges per circuit would be approximately one-third those of the first scheme.

It was decided to proceed with the London-Manchester development on these lines, and at the time of writing this paper the duct route between the terminal stations has been completed and the cable is well on the way. Duct space has been provided to take care of developments for the next 20 years. The duct route forks at Derby, one route going north to Leeds and the other north-west to Manchester (see Fig. 14). The repeater stations will be located at Northampton and Derby, and, as new buildings are required and cannot be completed and equipped to be ready for the completion of the cable, a temporary repeater installation is being provided in an existing Post Office building at Leicester, with sufficient equipment to operate the number of circuits required for 2 years ahead. The circuits will be worked temporarily at a rather lower efficiency than that designed for the full scheme, but they will be equal to most of the existing aerial lines and will relieve the present congestion on the routes between London and Lancashire and Yorkshire. In addition to this, the risks of interruption of service by storms will be minimized. A floor plan of a telephone repeater station is shown in Fig. 15.

At the present time the construction of similar main cable routes from London to Southampton and Portsmouth and from London to Bristol and the west has been commenced (see Appendix I). In due course the northern cable route will be extended beyond Leeds through Newcastle and Edinburgh to Glasgow, so that in the course of a few years all main telephone communications between the important commercial centres of this country will be underground, giving a quality and stability of service which will be equal to, if not better than, any in the world.

Submarine Cable Development.

The pioneer work performed by Post Office engineers in the development and introduction of gutta-percha covered cables will be evident from the paper by Colonel O'Meara already referred to. Since that time the subject has received continuous attention both by the Post Office engineers and British cable manufacturers, who have always been in the van of progress in regard to submarine cables. In 1911 Messrs. Siemens Bros. & Company introduced a form of balata dielectric as a substitute for gutta-percha in loaded submarine cables, on account of the greatly reduced leakance of the former as compared with the latter. The effect of this improvement was to reduce materially the attenuation constant and increase the range of speech in loaded cables (see Appendix III).

Various attempts have been made in the past to obtain three or four telephone circuits from a four-wire cable, notably by Mr. F. Jacob (see *Electrical Review*, 26 August, 1882) and Martin and Dieselhorst in their British patent for multiple twin cables, but in 1914 the Post Office engineers called attention to the fact that it is possible to obtain four circuits from a loaded four-wire submarine cable,* if the circuits are isolated from the land lines at the ends of the cable by transformers, viz. the usual two side circuits and one loop phantom circuit, the fourth circuit being obtained by using the four wires of the cable in parallel with earth. Experiment shows that the fourth circuit can be made successful in a continuously-loaded cable, but it has not yet been successful in a coil-loaded cable.

In certain circumstances where a four-wire loaded cable could not be satisfactorily laid owing to the weight of the cable, it might be desirable to lay single-

* A. B. Hart : "Telephonic Repeaters," No. 75 ; C. Robinson and R. M. Chamney : "Telephone Relays," No. 76. *Institution of Post Office Electrical Engineers, Professional Papers.*

* J. G. Hill : "Two Loaded Phantom Circuits in a Four-wire Submarine Cable," *Journal of the Institution of Post Office Electrical Engineers*, 1914 vol. 7, p. 197.

wire loaded cables, and an investigation of the electrical constants which might be expected in such a cable has been made by the Post Office (see Appendix IV). It is found that the effective resistance of the conductor in such a cable would be materially greater than that of half the resistance of a loop circuit of the same gauge, and experiment shows that, when tested at a frequency of 800 periods per second, the resistance is approximately 2·5 ohms more than that of the resistance of the same single wire when measured by direct current. The increase of resistance was found to be the same for conductors weighing 500 lb. per nautical mile per wire and 100 lb. per nautical mile per wire. The increased resistance in the two cases is due to the effect of the cable sheath and the return path consisting of sea water, and these are similar in the two cases.

The usual type of submarine cable contains four wires, but where it is necessary to carry a large number of circuits across a wide river or estuary four-wire cables are unsuitable. To meet this requirement a continuously loaded cable has recently been designed and is now being manufactured for the Post Office. This cable will contain seven quads, and it is hoped that seven phantom circuits will be obtained in addition to the 14 side circuits. The weight of the conductors is 70 lb. per nautical mile per wire, and of the dielectric 110 lb. per nautical mile per wire. The attenuation constant of the cable circuits for speech purposes is 0·05 per nautical mile, which is practically the same as that of the unloaded telephone submarine cable conductors weighing 160 lb. per nautical mile per wire and insulated by gutta-percha weighing 300 lb. per nautical mile per wire. This cable was the standard type for long-distance communication before the advent of loading, and it contained only four wires. This enables us to see at a glance the progress which has been made.

The introduction of telephone relays has made it possible frequently to use submarine cables of a less efficiency than the coil-loaded cables previously required. This makes it practicable to increase the use of continuously loaded cables in which the difficulties of maintaining loading coils in submarine cables are entirely eliminated.

The present policy is, on this account, to extend materially the use of submarine continuously loaded cables.

High-frequency Telephony.

This paper would be incomplete without some reference to high-frequency or so-called "wired wireless" telephony. This system has received some attention in the technical Press of Europe and America during the past two or three years, and it is becoming evident that developments of the system are destined to play an important part in the future in long-distance communication.

The principle of the system has been known to scientists for a decade, but the solution of the practical problems involved in adapting it to commercial use was delayed in this country by the war. These problems are by no means inconsiderable or simple of solution. They involve researches into the behaviour of aerial wires and underground cables over a range of frequencies hitherto practically unexplored (see Appendix V).

The characteristics of lines at the comparatively low frequencies of speech are well known, and apparatus has been designed which enables measurements to be made with precision. Such apparatus, however, is either entirely unsuitable, or suitable only with great modification for the purpose of making measurements at the comparatively high frequency of carrier-waves. The range of these waves lies roughly in the region between the highest note audible to the human ear, and the frequencies used in wireless telephony. Over this range, factors which at audio frequency are so small as to be negligible become of first-rate importance.

It may be said that the most attractive, and at the same time the most valuable feature of the high-frequency carrier-wave system is its adaptability to multiplex telephony, that is to say, the simultaneous operation of several communication channels over one wire circuit.

The system is already in commercial service in Germany and in the United States on long aerial telephone circuits, but the conditions under which those services are possible are practically non-existent in this country. Furthermore, it will be realized from what has been said previously in this paper that the economical value of the system for use in the United Kingdom lies in its application to underground cables. There are, however, a few cases where the system may be applied economically to aerial lines. Some of these are now being dealt with and will be brought into commercial service and linked up with the trunk system during the coming year.

APPENDIX I.

Particulars of Representative Types of Loaded Main Underground Cables.

Cable	Length	Number of Pairs	Weight per Mile Single Conductor	D.C. Constants of Cable per Mile Loop: Resistance, R	D.C. Constants of Cable per Mile Loop: Capacity Wire to Wire, C	A.C. Constants of Loaded Cable per Mile Loop at $\omega = 5\,000$: Inductance, L	A.C. Constants of Loaded Cable per Mile Loop at $\omega = 5\,000$: Attenuation Constant, β	A.C. Constants of Loaded Cable per Mile Loop at $\omega = 5\,000$: Characteristic Impedance, Z_0	Inductance of Loading Coils	Average Distance between Loading Coils
	miles		lb.	ohms	μF	henry		ohms	henry	miles
Leeds–Hull	58·6	48	70	28·7	0·065	0·052	0·0166	897 ∠3° 4′	0·133	2·55
		6	100*	—	—	—	—	—	—	
London–Birmingham	109·5	24	100	18·95	0·0575	0·0535	0·01092	905 ∠5° 41′	0·133	2·5
			100†	18·38	0·0568	0·053	0·01074	891 ∠5° 36′	0·133	
		12	150	13·13	0·0697	0·0537	0·00884	864 ∠8° 40′	0·133	
		14	200	9·62	0·0654	0·0536	0·00664	860 ∠8° 7′	0·133	
		2	300	6·55	0·0567	0·0547	0·00408	959 ∠13° 7′	0·133	
		6	150 phantom	6·56	0·1056	0·0345	0·00756	563 ∠6° 48′	0·0825	
		6	100 phantom	9·5	0·0905	0·0357	0·00926	610 ∠8° 55′	0·0825	
Birmingham–Liverpool	89·9	24	100	17·9	0·0579	0·053	0·01057	1 062 ∠1° 4′	0·133	2·5
			100†	17·32	0·0575	0·053	0·01014	1 069 ∠2° 57′	0·133	
		12	150	12·44	0·0685	0·0537	0·00846	1 031 ∠1° 4′	0·133	
		14	200	9·19	0·0572	0·0536	0·00618	1 088 ∠5° 45′	0·133	
		2	300	6·28	0·0545	0·055	0·00413	1 074 ∠2° 26′	0·133	
		6	150 phantom	6·22	0·10075	0·0345	0·00682	565 ∠1° 18′	0·0825	
		6	100 phantom	8·95	0·0864	0·0357	0·00837	615 ∠2° 11′	0·0825	
				D.C. Constants of Unloaded Cables						
				K	C					
London–Manchester‡	186·5	160	40	44	0·065	0·109	0·0208	1 298 ∠2° 35′	0·175	1·6
			40 phantom	22	0·090	0·066	0·0162	857 ∠2° 11′	0·106	
London–Bristol‡	122	308	20	88	0·065	0·155	0·033	1 550 ∠3° 31′	0·175	1·125
			20 phantom	44	0·090	0·094	0·0255	1 023 ∠2° 58′	0·106	
London–Southampton and Portsmouth‡	85	254	20	88	0·065	0·222	0·0292	1 855 ∠2° 33′	0·250	1·125
			20 phantom	44	0·090	0·138	0·0235	1 240 ∠2° 8′	0·155	

* 100-lb conductors used for telegraphs. † Not phantomed. ‡ In course of construction.

APPENDIX II.

Details of the Thermionic Valve used by the Post Office for Telephone Repeaters.

The valve used at present by the Post Office is a hard valve with electrodes arranged concentrically. The filament is of non-thoriated tungsten.

Connections from the electrodes are brought out to four solid pins in the valve cap which make contact with four corresponding springs in the valve holder.

The filament is designed to give a total emission of not less than 10 milliamperes when a potential of 150 volts is applied between the filament and the plate, the plate and grid being connected together.

The working temperature of the filament is such that the average life of the filament may be expected to be about 2 000 hours (actual burning time).

The current required to heat the filament does not exceed 1 ampere, and the maximum voltage-drop across the filament is 4·7 volts. The normal priming voltage between the grid and the negative end of the filament is — 4·5 volts. With this condition and with 150 volts plate voltage the valve has the following characteristics :—

Internal impedance: 20 000 ohms ± 20 per cent.
Mutual impedance: 2 750 ohms ± 20 per cent.

The amplification factor defined by the ratio "internal impedance/mutual impedance" is not less than 6·75.

In order to obtain a reasonably straight plate-current/grid-voltage characteristic curve, it is specified that the mutual impedance of any one valve shall not vary more than ± 20 per cent from the value at — 4·5 grid voltage between grid voltages of 0 to — 8.

APPENDIX III.

Submarine Telephone Cables laid since 1910.

Particulars of Cable	Length	Weight per Naut		Loop Resistance per Naut of Conductor without Loading (at 60° F.)	Alternating-current Constants							
		Conductor	Dielectric		Circuit	Frequency of Test	Added Resistance per Naut due to Loading	Inductance (L) per Naut	Capacity (C) per Naut	Leakance Capacity or GC	Attenuation Constant (β) per Naut	Characteristic Impedance (Z_0)
Anglo-Belgian.	nauts.	lb.	lb.	ohms		~ per sec.	ohms	henry	μF			ohms
St. Margarets–La Panne III, 1911. Coil loaded	47·9	160	150	14·3	Physical	800	11·5	0·100	0·157	12	0·0178	800 $\angle 1° 24'$
					Phantom	800	4·6	0·050	0·314	12	0·0181	400 $\angle 1° 16'$
Anglo-Irish.												
Nevin–Howth I, 1913. Coil loaded	63·3	160	150	14·3	Physical	800	6·8	0·100	0·166	15	0·0150	690 $\angle 2° 40'$
					Phantom	800	3·2	0·050	0·320	15	0·0150	446 $\angle 0° 52'$
Anglo-French.												
Abbotscliff–Grisnez IV, 1912. Continuously loaded	21·0	300	300	7·6	Physical	1 000	—	0·0135	0·176	109	0·0185	278 $\angle 2° 59'$
					Phantom	1 000	—	—	—	—	0·0185	—
Dover–Sangatte I, 1917. Coil loaded	20·8	310	200	7·4								
Dover–Dunkirk I, 1917. Coil loaded	41·6				Physical	800	5·2	0·080	0·189	20	0·0112	709 $\angle 1° 50'$
					Phantom	800	2·5	0·040	0·378	20	0·0114	338 $\angle 1° 40'$
Dungeness – Audresselles III, 1918. Coil loaded	26·0											
Dover – Sangatte II, 1918. Coil loaded	21·0	160	150	14·3								
Dungeness – Audresselles II, 1918. Coil loaded	27·6				Physical	800	6·2	0·100	0·166	20	0·0145	776 $\angle 1° 3'$
					Phantom	800	3·0	0·050	0·320	20	0·0140	395 $\angle 1° 2'$
Submarine Telephone Cables under Construction, with Anticipated Constants.												
Anglo-Irish.												
Temple Patrick–Port Mora. Continuously loaded	22·0	169	195	13·5	Physical	800	2·0	0·0245	0·195	20	0·025	356 $\angle 3° 43'$
					Phantom	800	—	—	—	—	0·025	—
Cable, Submarine, 28 core	10·0	70	110	32·7	Physical	800	1·2	0·020	—	—	0·050	—

APPENDIX IV.

Tests on Single-wire Submarine Cables.

Details of tests on Lundy Island cable.

Weight of conductor per nautical mile : 107 lb.
Weight of dielectric per nautical mile : 150 lb. (dielectric : gutta-percha).
Length of cable : 16·4 nautical miles.
Sheathing : 10 wires No. 2.
Date of tests : October 1912.

Direct-current tests : Constants per nautical mile.

Resistance : 11·0 ohms.
Capacity : 0·327 μF.
Insulation resistance : 2 570 megohms.
Inductance : not measured owing to earth currents.
Electrification : steady.

Alternating-current tests.

Summary of results at different frequencies.
Testing current : 1 mA.

$2\pi f$*	Line Constants per Nautical Mile				$\frac{G}{C}$
	R	L	G	C	
	ohms	mH	micro-mhos	μF	
2 000	12·00	1·84	—	0·315	—
3 000	12·70	1·72	21	0·315	67
4 000	13·08	1·62	40	0·315	126
5 000	13·36	1·58	43	0·316	137
6 000	13·60	1·50	62	0·306	200
7 000	13·90	1·44	81	0·316	256

$2\pi f$*	Characteristic Impedance, Vector Ohms	Attenuation Constant per Nautical Mile	
		Real Part, β	Imaginary Part, α
2 000	138 $\angle 36° 24'$	0·0525	0·0715
3 000	120 $\angle 33° 17'$	0·0645	0·0935
4 000	108 $\angle 31° 2'$	0·074	0·1130
5 000	99 $\angle 28° 53'$	0·079	0·1340
6 000	94 $\angle 27° 16'$	0·085	0·1510
7 000	90 $\angle 25° 16'$	0·090	0·1670

The tests were made by means of a "Franke" machine.† The open and closed impedances of the cable were measured and the cable constants calculated from the measured results. The tests were also repeated with currents varying from 1 to 6 mA at a frequency such that $2\pi f = 5\,000$. The resulting variation of the cable constants was negligible.

* f = frequency of alternations per second.
† J. G. Hill : "Telephonic Transmission," p. 237 [Longmans, Green & Co.], and *Electrotechnische Zeitschrift*, 1911, vol. 32, p. 448.

Details of tests on St. Margarets–Dunkirk cable.

Weight of conductor per nautical mile : 500 lb.
Weight of dielectric per nautical mile : 300 lb. (dielectric : gutta-percha).
Length of cable : 39·155 nautical miles.
Sheathing : 10 wires No. 2.
Date of tests : May 1919.

Direct-current tests : Constants per nautical mile.

Resistance : 2·75 ohms.
Electrostatic capacity : 0·4 μF.
Inductance : not measured.

Alternating-current tests.

Summary of results at different frequencies.
Testing current : 1 mA.

$2\pi f$*	Line Constants per Nautical Mile				$\frac{G}{C}$
	R	L	G	C	
	ohms	mH	micro-mhos	μF	
4 000	5·25	1·40	50	0·391	128
5 000	5·49	1·27	62	0·387	160
6 000	5·88	1·23	73	0·392	186
7 000	6·31	1·20	76	0·396	192
8 500	6·99	1·17	121	0·399	303
10 000	7·63	1·14	188	0·400	470

$2\pi f$*	Characteristic Impedance, Vector Ohms	Attenuation Constants per Nautical Mile	
		Real Part, β	Imaginary Part, α
4 000	69·7 $\angle 20° 17'$	0·0410	0·1009
5 000	65·9 $\angle 19° 37'$	0·0461	0·1188
6 000	63·3 $\angle 18° 29'$	0·0512	0·1400
7 000	61·5 $\angle 17° 30'$	0·0570	0·1606
8 500	59·8 $\angle 16° 45'$	0·0647	0·1924
10 000	58·6 $\angle 16° 15'$	0·0706	0·2234

The constants were calculated from measurements of open and closed impedance taken with an alternating-current Wheatstone bridge, using an oscillator as source of current.

A set of tests taken with a current of 5 mA showed no appreciable difference in the results.

* f = frequency of alternations per second.

APPENDIX V.

Tests of an Underground Telephone Cable.

A.S.P.C. type conductor, 20 lb. per mile.
Length, 4·445 miles. Insulation (d.c.), 120 megohms per mile.

Frequency f	$2\pi f$	Characteristic Impedance	Propagation Constant	Attenuation Constant	Wave-length Constant	Constants per Mile Loop			
						R	L	C	G
						ohms	H	μF	micro-mhos
587	3 690	670·8 \43° 34′	0·1030 /46° 24′	0·0900	0·0943	87	0·00114	0·053	0·3
688	4 320	620·0 \43° 21′	0·1414 /46° 36′	0·0970	0·1025	87	0·00114	0·053	0·3
796	5 000	577·0 \43° 6′	0·1522 /46° 50′	0·1042	0·1110	87	0·00114	0·053	3·0
1 150	7 220	481·5 \42° 18′	0·1830 /47° 41′	0·1232	0·1352	87	0·00114	0·053	0·2
1 608	10 100	407·5 \41° 14′	0·2163 /48° 43′	0·1428	0·1625	87	0·00113	0·053	0·7
2 125	13 350	354·5 \40° 7′	0·2505 /49° 58′	0·1611	0·1917	87	0·00113	0·053	Zero
2 911	18 290	306·0 \38° 26′	0·2945 /51° 26′	0·1828	0·2310	88	0·00113	0·053	Zero
3 685	23 150	275·0 \36° 39′	0·3332 /53° 12′	0·1997	0·2670	88	0·00113	0·053	3·0
15 400	96 500	159·2 \21° 30′	0·850 /70° 4′	0·292	0·804	90	0·00106	0·055	Unreliable
17 200	107 900	154·8 \19° 25′	0·941 /71° 32′	0·299	0·894	90	0·00106	0·056	Unreliable
19 730	123 800	151·0 \17° 58′	1·059 /73° 20′	0·304	1·014	91	0·00106	0·056	Unreliable
21 880	137 200	150·0 \16° 40′	1·148 /74° 29′	0·307	1·106	92	0·00106	0·056	Unreliable
23 050	144 500	148·9 \16° 28′	1·186 /75° 12′	0·309	1·156	92	0·00105	0·056	Unreliable
24 480	153 500	148·0 \16° 12′	1·276 /75° 42′	0·315	1·235	94	0·00105	0·056	Unreliable
26 320	165 100	144·6 \15° 51′	1·353 /76° 23′	0·321	1·323	96	0·00104	0·057	Unreliable
28 550	179 000	142·0 \15° 22′	1·465 /77° 8′	0·327	1·430	96	0·00103	0·058	Unreliable

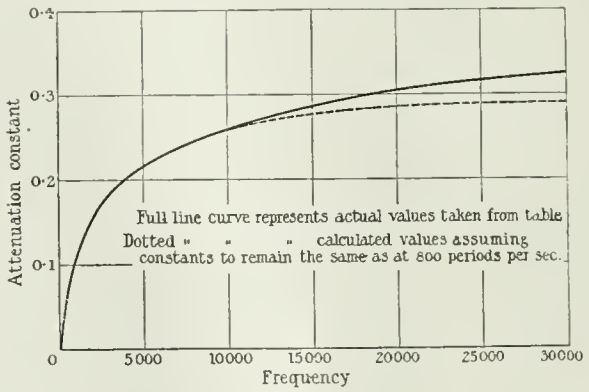

Fig. 16.

Discussion before The Institution, 17 March, 1921.

Professor J. A. Fleming: Dealing, as it does, to a large extent with thermionic valves used as telephone repeaters, this paper has considerable interest, not only for telephonists, but also for radio engineers. Although the valve is mentioned here chiefly in connection with telephony, I think it is not incorrect to say that but for the previous effort to find new and more sensitive detectors for wireless telegraphy, and the developments which followed the introduction of my oscillation valve in 1904, it is highly probable that the thermionic repeater would never have made its appearance in ordinary telephony, because all previous research on telephonic repeaters had followed on quite different lines. The form in which it is now used for that purpose in telephony is one of the standard forms of valve evolved before and during the war as a wireless amplifier and detector. In order to show some of the stages of its evolution, I have exhibited a few historical specimens on the table. In the glass case are the first actual thermionic valves used by me in 1904, with which I made the discovery of the possibility of rectifying the high-frequency currents

used in wireless telegraphy. It will be seen that one of those specimens contains a grid in the bulb, the second contains a flat plate, and the third contains a plate and a cylinder surrounding the filament. Then next to it on the left hand is the cylinder valve for which I obtained a patent in 1904, which has been the subject of so much litigation, and has been upheld as the master patent on the subject. The next is the so-called audion of de Forest, patented in 1907, in which are a grid and plate. Next to that is a valve which I made at the beginning of the war, containing a cylinder and spiral grid, and I am inclined to think it was one of the earliest spiral-grid valves. It was made for me by the late Mr. Stearn. Next to that is an American valve made largely during the war, which is of practically the same pattern ; and last of all comes the valve which wireless telegraphists call the R valve, and sometimes the French valve, because it was patented in 1916 in Great Britain by Péri and Biguet. I think that pattern is quite similar to the form now used by the Post Office as a repeater. The author makes only a brief reference, in a footnote, to the immense research during the war in order to determine the relation of structure to the performance of the valve, and he does not mention the prior work done in the United States in connection with the New York to San Francisco and Los Angeles trans-continental telephone line, which would have been quite impossible without the assistance of the thermionic valve. The copper equivalent of that line alone is equal to 140 miles of standard cable, which is about five times the possible distance of good speech, but by the introduction of 13 repeater stations it has been brought down to the equated length of 12 miles of standard cable, which is a most wonderful performance. On the other hand, although telephony has found another great use for the master weapon of the wireless telegraphist, yet there is an important difference between the requirements in the two cases. All the properties of the valve depend upon the form of the characteristic curve and its sections parallel to the axes taken as plate current, plate potential and grid potential. Now in wireless telephony we make great use of the fact that the characteristic curve of plate-current/grid-voltage is not a straight line, but in telephony it is important that this line should be straight, because if not there will be distortion of speech, within certain limits at any rate. Although the author gives on page 395 of the paper seven conditions which must be fulfilled by a thermionic valve in order to be useful as a repeater, he does not there mention the fact that this characteristic curve should be a straight line. Nevertheless, that condition is implied and mentioned in Appendix 11, at the bottom of page 403 ; but I hardly think it is given the prominence it deserves. There is no reference in the paper to the use of oxide-coated filaments in valves, and the work on that subject which has been done very largely in the United States ; in fact, during the war there were more than twice as many valves made for all purposes with oxide-coated filaments as with plain tungsten filaments. A very interesting paper on that subject was published by Dr. Arnold in the *Physical Review* for July, 1920. It has been found that platinum wires coated with a mixture of strontium oxide and barium oxide give a very large thermionic emission at a very low temperature, about red heat. Now, as the life of the valve depends very greatly upon the temperature of the heated cathode, it is claimed that these oxide-coated filaments have advantages over the plain tungsten filaments. I should like to ask if the Post Office have investigated that point. For instance, it has been stated that a tungsten filament operating at a temperature of 2 500° K. with a power expenditure of 77 watts per square centimetre of filament surface gives an electron emission of 11 mA per watt, whereas an oxide-coated filament gives 23 mA per watt with a power expenditure of only 7 watts per square centimetre in the filament. I should also like to ask whether the Post Office have investigated the helium-filled valve which was used very largely during the war as a detector, and which was found to be almost equivalent to the hard tungsten valve. Another matter to which no reference is made in the paper is the remarkable work which the thermionic valve can perform in repeating from wireless service to wire service, and vice versa. That was illustrated last October by a large-scale experiment in the United States. A ship in the Atlantic, the s.s. "Gloucester," about four hours out, spoke by radio-telephone to the mainland. At that point the speech was relayed on to a land line by thermionic valves to New York ; then again it was relayed by thermionic valves on to the trans-continental line to San Francisco and Los Angeles, after which it was relayed again on to a radio line, and sent on to Catalina Island, about 30 miles out in the Pacific. The operator in the Atlantic spoke over a distance of 4 000 miles to the operator in the Pacific, without any human interference, simply by the aid of the thermionic valve. There can be no doubt that we are not yet at the end of the wonders which can be performed by this valve. I feel sure that it will render great services to telephony, as it has done to wireless telegraphy and telephony. Again, submarine cable engineers when they come to investigate its properties will find it of great assistance in their work. Its history and development have been one of the romances of electrical science, and the specimens I have exhibited to-night on the table show the first samples of this appliance made by me, in which the thermionic emission from incandescent filaments was rendered the obedient servant of the telegraph and telephone engineer.

Mr. A. J. Stubbs : The author says on page 390 : " In the first decade of the present century it appeared that the ultimate possible efficiency in long-distance telephone transmission had been reached," but I think he does not really mean that exactly. At that time the ultimate efficiency of our circuits was reduced to 60 per cent by the fact that we had to introduce into our trunk lines short lengths of underground work, but we knew even then that our ultimate objective was all-underground work. We attained that objective, as the author has said, by 1914 when, by means of " loading," we had secured thoroughly efficient underground telephone circuits between London and Liverpool. Thus again, in the author's sense, we had

attained the ultimate possible efficiency. At that stage we were able, in the largest cable that we could economically construct, to obtain 42 circuits between those two points. The author tells us that four years afterwards, the introduction of the thermionic valve enabled us to increase the number of circuits in the largest possible cable from 42 to 90 if we inserted those little "magic" tubes in the middle of the length, and that, by the simple process of sub-dividing the cable route into three instead of two sections, we could increase the number from 42 to 240 equally efficient telephone circuits *at about the same cost.* In conclusion, I venture to submit that the Post Office telephone system is not very far behind the most able and the most advanced of the telephone administrations of the world in the splendour of its engineering accomplishment.

Mr. R. L. Jones: For universal service, subscribers must be guaranteed the loudness and clearness of speech requisite for good intelligibility. At the same time that the service is extending its boundaries and increasing the number of subscribers per unit area, we have a fixed value of the allowable transmission equivalent which defines the standard of service. In a long trunk-cable circuit with repeaters, the total gain may be as much as 93 per cent of the total equivalent. If the repeaters are allowed to vary by only about $\pm$ 5 per cent from their normal values, either (1) the circuit will be inoperative, due to distortion of speech or singing, or (2) the subscriber-to-subscriber equivalent will be unsatisfactory. In such a trunk, the maintenance problem is very important. The necessity for a high grade of transmission maintenance is, however, a requirement resulting not solely from the application of repeaters. In any extensive telephone network where long, built-up connections are made, there are many points where losses are likely to be found. In a system the size of that in the United Kingdom there are several hundred thousand circuit units in which abnormal losses may occur. It is only by active transmission maintenance that the money values corresponding to this waste can be kept from amounting to a very large figure. There are in use in the plant departments of the American Bell system for routine transmission maintenance purposes several interesting types of measuring equipment. Some are portable, and some are designed as a part of the regular test-board panels. Some are useful in the maintenance of repeaters, others in that of high-frequency carrier-current systems, and still others for the transmission maintenance of the extensive system of central office equipment and telephone lines. There may be mentioned:

(1) Sources of testing currents, including among others a portable vacuum-tube oscillator with a range of 200 to 3 000 periods per second, and another one with a range of 100 to 50 000 periods per second.

(2) Transmission measuring equipment for the rapid and precise measurement of transmission losses in central office apparatus and lines. It permits the measurement of the transmission equivalent of a complete trunk circuit with or without repeaters.

(3) Equipment is available for testing the degree of balance between the trunk line with its associated equipment, and the balancing circuit. A rough test is provided by using the repeater set itself as an oscillating generator. An impedance bridge may, of course, be used. An impedance unbalance set has been standardized, which gives the unbalance directly. It is more precise than the rough test first mentioned, and gives results more rapidly than the bridge method.

(4) Impedance-measuring equipment is needed for checking over a range of frequencies the impedance characteristic of lines and networks used in repeater and carrier-current circuits. The equipment required consists of a special alternating-current bridge with auxiliary apparatus. For the carrier range a bridge is available, capable of an accuracy of one-quarter of 1 per cent up to 50 000 periods per second. A high-frequency vacuum-tube detector unit is used as an auxiliary for high frequencies.

Fig. A shows the possibilities of the second type

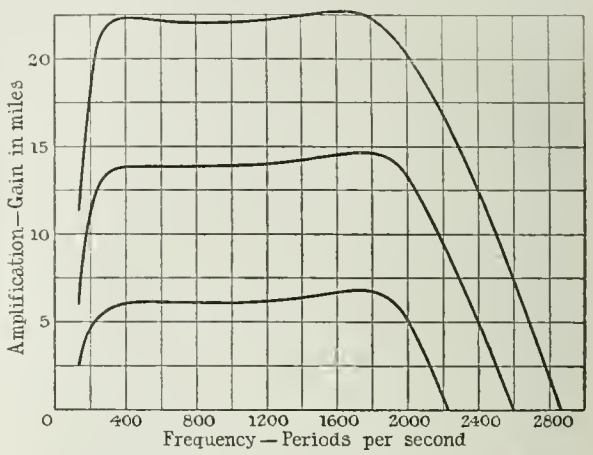

FIG. A.—Transmission gain characteristics of a Western Electric two-wire repeater set.

of measuring set. It also serves to illustrate the characteristics which a good repeater set ought to have. It gives the calibration of a 2-wire repeater as recently tested in the London laboratory of the Western Electric Company. The data give amplification or improvement in standard miles for the complete two-way, two-tube repeater over a frequency range of 135 to about 2 600 periods per second. The three curves show characteristics for three different settings of the potentiometers. With the transmission measuring set referred to, all this data can be taken in less than an hour. The flat characteristic over the range from about 200 to 2 000 periods per second, and its preservation at various gain settings, are two of the most important criteria of quality for repeater set design. If this flat characteristic is not achieved the intelligibility of the conversation is affected, especially when a number of repeaters are operating in tandem on the circuit. Outside the speech range it is desirable that the amplification be low, as this simplifies the balance problem. The curves show to what degree this condition also has been achieved. In conclusion, I should like to mention trunk-cable developments in America.

There are about 2 400 miles of trunk cables in use in the United States, and in a short time cable circuits about 1 000 miles long will be in operation. Recently a 4-wire cable circuit slightly longer than this was put into service for commercial trial by looping back and forth in a cable between New York and Reading, Pennsylvania. The circuit is of approximately 20-lb. conductors, loaded and phantomed, and the total equivalent is 550 standard miles. With the aid of 22 repeater sets operating in tandem, an equivalent of 10 standard miles with excellent quality has been achieved and all arrangements have been perfected, so that this circuit can be maintained in commercial service as long as is desirable.

Mr. J. E. Kingsbury : We are all, I suppose, inclined to consider that any advances in our own particular branch of art or science are of more importance than those in any other, but I think that the author has put before us to-night a record of advancement which is unequalled in any other branch of our science, and perhaps exceeds any other practical advance that has been made in the same period. In order to bring home to us exactly what that advance is, I propose to quote from a document a little older than any of those from which the author quotes. It is a memorandum which was issued to those of us who were present at the inauguration of the telephone trunk lines of the Post Office from London on 26th June, 1895. In this memorandum it is said : " For the ' backbone ' line from London through Leeds to Edinburgh, Glasgow and Dublin the heaviest copper wire ever erected has been used. It weighs 800 lb. per mile. Efforts have been made to reduce the underground portion as much as possible, as underground wires seriously impair the efficiency and clearness of telephone speaking." That memorandum had a map attached to it. Since it deals with the same territory it has, of course, much similarity with that in Fig. 14 of the paper. The difference is to be found in the statement on the author's map that the lines represent main underground telephone cables. What the 1895 memorandum shows is a backbone system in which there is no cable and in which the heaviest copper wires have been used in the main. There are 2 719 miles of 800-lb. copper, 4 914 of 600-lb. copper and 3 025 of 400-lb. copper. The fact that there is no cable shown in the 1895 map and nothing but cable shown in the 1921 map indicates the advance which has been made since the trunk lines were taken over by the Post Office in the former year. This means in practice that hitherto the telephone service which requires the most delicate transmission and operation has been at the mercy of the elements, both electrically and mechanically. In the case of a storm the overhead wires come down, service is interrupted, confidence in the use of the service is impaired, and consequently its utility in every possible respect is in the worst condition. In the case of buried cables the conductors are in the best possible position for maintaining stable electrical conditions and are not interfered with by the mechanical effects of the storms, being thus always available for use when required. That is one of the results arising from the developments of which the author has told us. Let us now consider the further developments which have arisen, as a result. Although great improvements were made in long-distance cables it was at very considerable cost. I think it was in 1891 that the Conference standard cable having conductors of No. 19 B. and S. gauge was introduced, and the transmission equivalents of that cable form the standard to-day. No. 19 gauge is equivalent to about 20 lb. a mile in the weight of conductor. The author takes as his standard of transmission 12 to 15 miles of standard cable. When that cable was introduced it is clear that with a transmission as good as the author contemplates on his lines it could only have been used for a distance equal to that, say, between London and Croydon. To-day it could be used, with attachments of the types that the author has described, for diplomatic talk between London and Rome or, to take industrial comparisons, say, from Glasgow to Milan. From this it becomes readily realizable how great the advances are which have been made in the first place in loading and subsequently in relays. The electrical form of relay has long been the aim of the telephone engineer. The reason for the lack of further development of the mechanical relay should not surprise us, because, obviously, it could not be operated as a machine by a very attenuated current, and the inertia of the moving mechanism must necessarily have produced distortion. I should like to ask whether the development studies which have been made contemplate service on a " no-delay " basis. I think that is a matter of very considerable importance. With cables and repeaters there is a possibility of alternative routes for relief of local pressure, and the development of the service on a no-delay basis appears to be what should be aimed at. Everyone who wants to use the telephone wants to use it at once. It is true that the provision of plant on a no-delay basis involves a larger plant charge from the point of view of interest on capital, but, when one realizes the amount of saving in plant maintenance due to the use of cables, one can see it is a very substantial set-off, and when cables are used the comparative economy per circuit of providing the maximum number of circuits makes it possible to provide sufficient circuits between any places in the United Kingdom to give all the service that is required at the time, instead of endeavouring to spread it over a longer period.

Mr. P. E. Erikson : On page 396 the author shows impedance/frequency curves of a loaded 70-lb. cable circuit, loaded with 135-mH coils at intervals of 2·6 miles. The range of frequency covered is from about 320 to about 1 920 periods per second, the impedance being represented by its two components, effective resistance and reactance. On page 398 it is stated that the cable tested was electrically short and that the prominent irregularities disclosed by the curves were due to unequal spacing of the coils and variations between the inductance values of the coils along the circuit. One method of treating this cable so as to eliminate the influence of the irregularities is given, and is said to consist in shunting the line with a suitable series combination of a resistance and a condenser, at the same time lengthening the line by the addition of an artificial line. The importance of uniform line

characteristics for successful telephone repeater operation was recognized early in the development of the repeater art. In fact, the amount of transmission gain obtainable with the two-way, two-amplifier circuit (Fig. 6) is very largely dependent on the precision with which the balancing network resembles the actual line.

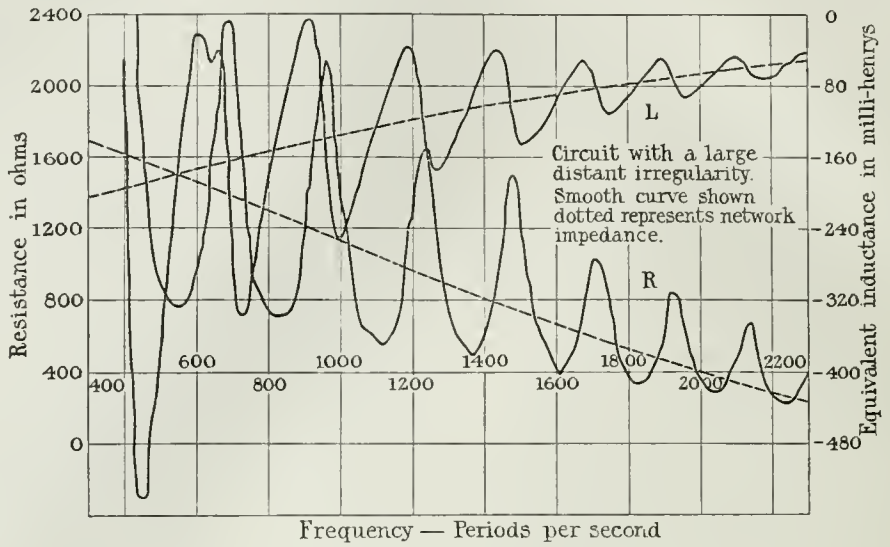

FIG. B.—Frequency/impedance curves for a loaded 20 lb.-per-mile conductor cable.

It is possible to design a network to simulate the impedance of almost any line, but this would obviously be a cumbersome and expensive way of dealing with the problem. In the commercial operation of telephone repeaters it is necessary, for the sake of standardization, to introduce as few types of balancing networks as possible. The method adopted is to eliminate as far as practicable all irregularities from the lines intended for repeater working. As the standard types of lines employed in telephone work are relatively few, it follows that the number of balancing networks will be correspondingly small. It is rather unfortunate that the 70-lb. cable, the characteristic of which is shown in Fig. 8, should have been chosen for a specimen curve. The conditions under which the tests were made cause the curves to indicate increasing impedance with an increase in frequency. This is somewhat misleading, since the normal slope of the impedance curve, for a long loaded cable of the type shown, should indicate decreasing values with increasing frequency. In order to illustrate the different impedance characteristics between a line containing a large irregularity and the same type of line of uniform structure, I have prepared two figures. As both the curves were plotted from actual measurements on long loaded cables, the true slope of the impedance curves will be evident. The curves shown in Fig. B represent the effective resistance, R, and the inductance part, L, of the reactance component of a 20-lb. cable, loaded with 175-mH coils, spaced 1·13 miles apart. The frequency range covered

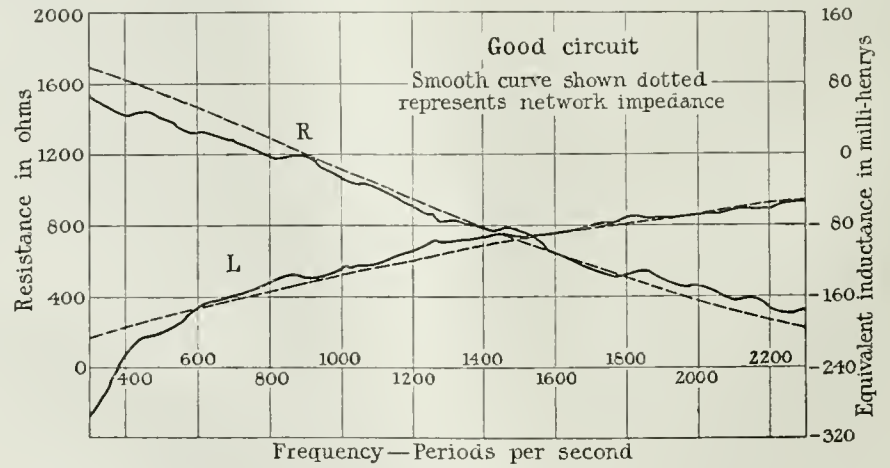

FIG. C.—Frequency/impedance curves for a loaded 20 lb.-per-mile conductor cable.

extends from about 350 to 2 300 periods per second. The periodically recurring waves in the curves are due to currents reflected back to the measuring instrument by a large distant irregularity. The amplitude of the waves of the curves depends on the amount of current which is thus reflected back, and also on the distance to the irregularity. It would, of course, be out of the question to design a network to balance a line having such irregular characteristics. The proper procedure is to locate and remove such irregularities. In this particular case the irregularity which caused the wavy impedance curves is a short-circuit on the cable about 13·5 miles from the testing point. This short-circuit was deliberately made before the circuit was tested, to demonstrate its effect upon the impedance characteristic. Fig. C shows a good circuit, i.e. one with a smooth characteristic. It was obtained by impedance measurements on a 90-mile length of 20-lb. cable, loaded with 175-mH coils, spaced 1·13 miles apart. The coils were very accurately spaced, and the inductance values of the individual coils kept between very narrow limits. While under test the remote end of the cable was terminated by an impedance corresponding to that of the cable itself, viz. 1 500 ohms. This, of course, has the effect of making the cable under test infinitely long, electrically. The dotted curves represent the impedance of the network which was designed to balance this cable for repeater working. The latter is of relatively simple structure, and it will be seen that its characteristic approximates very closely to that of the cable.

Mr. F. Tremain : I took part in the experiments referred to in the paper, and I should like to say that the variable lays which were used to avoid interference between neighbouring pairs were coloured in order that the capacity when the whole cable was joined up should be made uniform throughout. In 1901 I initiated the first loading experiments undertaken by the Post Office. A quantity of cable was delivered on drums for the London telephone development, each containing 4 pairs of wires in $\frac{1}{4}$-mile lengths. These cables could not be utilized immediately, and the opportunity was seized to use them to form an artificial cable, equip them with loading coils, and use the circuits so built up for superimposing ; thus producing various types of circuits. The London–Birmingham cable was then loaded in a similar manner and the results were observed by a number of experts, who agreed that a two and a half to threefold improvement in speech had been effected. Those 4-pair cables were the original of the quad-pair type which I patented about that time, and which I still think is the best type for superimposing. It is based on the old gutta-percha quad which has been so successful in a great many submarine cables, that is, four pairs of wires laid up round a centre of yarn. I should like to know whether any balancing was necessary in the loaded submarine cable described in Colonel O'Meara's paper to which reference is made, and on which he states there was no cross-talk. I also carried out a number of experiments between Nevin and Newcastle, co. Down, on a 60-knot length of cable. Between the diagonal pairs of solid-core cable with gutta-percha insulation there was no cross-talk, but between a similar one with an air space in the middle, cross-talk was observable. This, I think, points to the fact that air space in cables, if it leads to a spongy condition, is likely to increase the cost of the equipment by reason of the very careful balancing needed. I should like to ask the author if he can say what is the percentage addition to the cost of underground and submarine cables as the result of having to balance in this careful manner, and also what is the additional cost of loading such cables. If the need for balancing can be avoided it would appear that a substantial saving would be effected by producing a more stable article. When we loaded the London–Birmingham cable we found that we had got what we called a hyper-sensitive circuit which was very susceptible to external influence. It seems to me that now that we can counterbalance the capacity by inductance it might be well to go back to something more approximating to a solidly insulated conductor, so as to obtain a greater radial stability. For instance, if two enamelled wires were stranded together with a helical wrapping of paper, in an open spiral, I suppose we could get a wire-to-wire capacity very little different from what is now considered satisfactory in modern cables—something in the neighbourhood of 0·06 μF per mile. Before leaving the question of the solid cable I should like to ask whether it is known what the insulation would be of two wires twisted together with only enamel between them. If the cost of such pairs or quads of conductors would not be prohibitive, it would be an exceedingly stable construction ; a number of such cores could be made up into a kind of wire rope, each pair or quad being laid up symmetrically around a centre of yarn. I think if the author would add to the paper a complete bibliography of the subject it would be extremely useful for students. In Appendix V the insulation of the cable used for the high-frequency measurements is given as 120 megohms per mile ; presumably this is a misprint, as the author would hardly use cable with such a low insulation resistance. The need for balanced and equal circuits on either side of a repeater leads me to raise again the question of differentiating between local and long-distance subscribers. It would be interesting to know what percentage of subscribers use the long-distance trunk telephone service ; probably not more than 10 per cent. I have always been averse from the policy, adopted in this country, of making the long-distance service available for every subscriber, except in the case of Continental trunks. I think special circuit provision for those accustomed to make many trunk calls would be readily paid for, and the other subscribers, on the rare occasion of their making long-distance calls, would be better catered for at a local post office or exchange.

Mr. F. Gill : Fig. D shows the relationship between total tons of copper and distances traversed for various kinds of circuits, with a transmission equivalent of 10 standard miles. Curve I is for a non-loaded open copper circuit, and curve II for a loaded open wire, both curves stopping when the size of the wire reaches 800 lb. per mile. Curve III shows an unloaded repeatered copper circuit. These three types exhaust the open-wire circuits. We now come to the cables. Curve IV is for a heavy loaded cable (stopping at 300 lb. per mile).

Curve V is for a heavy loaded cable with one repeater (stopping at 166 lb. per mile) and curves VI and VII are for the same heavy loaded cable having two and three repeaters in tandem. The last carry about 864 miles with a total expenditure of 128 tons of copper. Curve VIII is for a 4-wire circuit using repeaters, and in that case only 80 lb. of copper per mile are used throughout. It will be seen that two things have been done ; first, the amount of copper required has been greatly reduced, and second, the possible distance has been much increased. The total amount of copper required is, of course, not the only criterion. The costs also must be considered, because an increase in the amount of copper used will increase pole-space or duct-space costs, and a decrease in the amount of copper will add to the loading and repeater costs. But the amount of copper is very important, since we are coming to the time when we shall deal with hundreds of circuits, not one or two. If the author had to carry 500 circuits through the country overhead, say of 800-lb. wire, I suppose he would want about 10 pole-routes. I do not think he could find space for them in the country. If he tried to carry the 500 circuits in heavy copper cables, he would probably want about 10 pipes, while if he carried them in 4-wire circuits, with repeaters, he would require only three ducts or one pole-line. I should like to show what has been done with carrier-current circuits. In the United States there are at the present moment in actual commercial service carrier telephones and telegraphs, i.e. high-frequency or " wired-wireless," on 3 500 miles of physical circuit, and these carrier circuits provide over 19 000 miles of extra channels. The lay-out shown on the screen is Fig. 52 from " Carrier Current Telephony and Telegraphy," by Messrs. Colpitts and Blackwell, read before the American Institute of Electrical Engineers on 17th February, 1921. It shows a carrier circuit between Harrisburg and Chicago with a length of 742 miles. It will be seen that there are four intermediate repeater stations, and at the first only the carrier current is repeatered. At the second both the carrier and the voice are repeatered. At the third station the carrier is repeatered again, and the voice circuit breaks off and continues on another line, while the carrier runs through. At the next station the same thing happens. The hatched portions indicate cable, so that the carrier works through cable, some of it loaded, and some unloaded. In this system the carrier current itself is suppressed ; it does not go over the line. For about 200 miles of this circuit there is on the same route of poles another telephone carrier circuit and also a carrier telegraph circuit. In summing up the matter, Messrs. Colpitts and Blackwell state that this system has been applied successfully and commercially. It has fulfilled all the conditions of

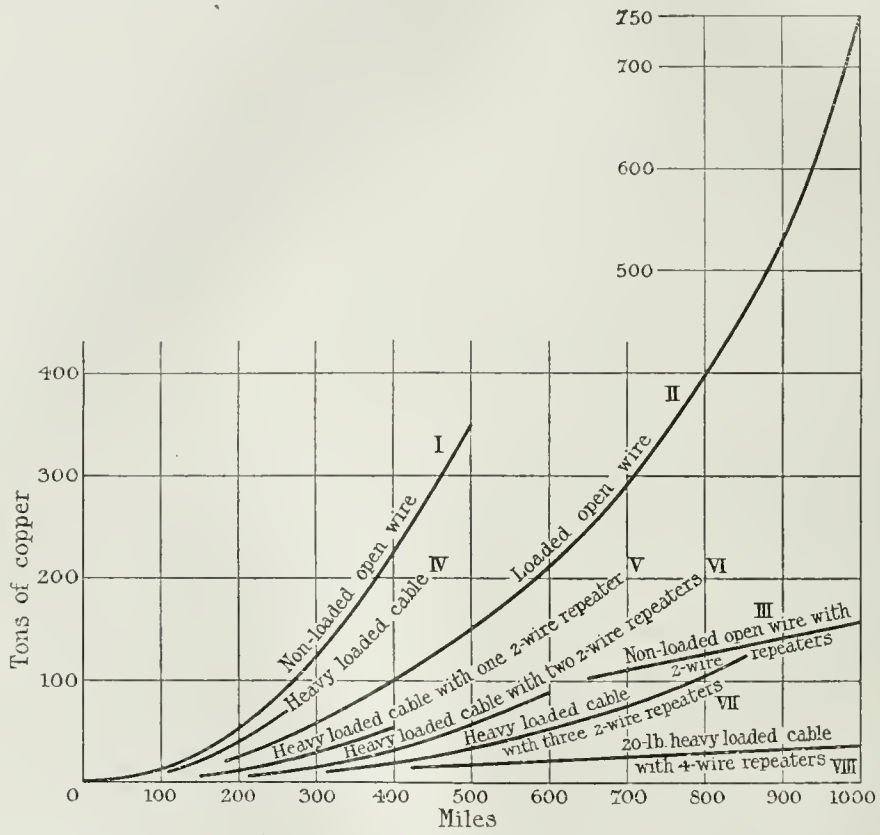

Fig. D.—Effect of loading and repeaters on the total weight of copper required.
(Transmission equivalent : 10 miles of standard cable.)

high-grade long-distance service ; it has proved flexible and has been maintained by the ordinary personnel.

(*Communicated*) : The principal stages in the progress of the art of long-distance telephony in the United States were as follows :

Open-wire construction.—In July, 1900, loading was first commercially applied to an aerial line 670 miles long, between New York and Pittsburg, when loading coils having an inductance of 0·225 H spaced $2\frac{1}{2}$ miles apart were employed. In March, 1904, the first commercial phantom circuit was completed on a non-loaded aerial line between Darlington and Boston. This circuit was 37 miles long and was phantomed on 173-lb.-per-mile conductors. The first application of loading to phantom circuits was on an aerial line between Philadelphia and Pittsburg, a distance of about 275 miles. This circuit was of 173-lb.-per-mile conductors and was completed in August, 1910. Thermionic repeaters were first commercially used early in 1914 and in connection with an open-wire line of 435 lb. per mile between New York and Chicago, a distance of 850 miles. This line was loaded with 0·25-H coils spaced about 8 miles apart, and the repeaters were located at Pittsburg. On July 29th, 1914, Mr. Vail at New York conversed with San Francisco, a distance of 3 400 miles, over a loaded 435-lb.-per-mile line. This line was the first to use thermionic repeaters in tandem and it was opened for commercial purposes in January, 1915.

Cable construction.—In 1895 the problem of obtaining phantom circuits on a cable was under consideration, but it was not until January, 1905, that a satisfactory duplex cable, which was designed on the " spiralled four " principle, had been evolved. At this time, however, it was considered preferable to use loaded cable rather than duplex cable, and it was not then thought practicable to load phantom-cable circuits. In May, 1900, the first loaded underground cable was put into service between Jamaica Plain and West Newton, and the first large commercial installation of loading on underground cables was put into service in August, 1902, when 50 pairs were loaded in a cable between New York and Newark, N.J., a distance of about 11 miles. Investigations of cable suitable for phantom working, and of the manufacturing processes required, had been continued ever since the early studies above mentioned. The multipled-twin or quadded construction was finally concluded to be the most desirable. By August, 1911, 75 miles of the New York–Washington cable had been completed, whilst by June, 1912, the Philadelphia to Washington section was put into service. This cable contained 148 conductors of 166, 83 and 41 lb. per mile, the majority of which were arranged in multiple-twin construction and balanced for phantom working. The process of eliminating capacity unbalance which was employed on this cable was patented in the United Kingdom by patents Nos. 2 508 and 2 509 of 1913. These patents, which describe the process in detail, were first applied for in the United States in April, 1912, and in this country in January, 1913. The application of thermionic repeaters to cables was first made commercially at Philadelphia on the New York–Washington all-cable circuit. On the New York–Philadelphia section loaded 66-lb.-per-mile conductors were used, and on the Philadelphia–Washington section loaded 83-lb.-per-mile conductors were used. The total length of circuit was about 220 miles, and it was put into operation early in 1915.

Mr. G. C. Marris : On page 395, in paragraph (*d*) the author, speaking of the repeater unit, says : " Its amplifying factor, in association with its input and output transformers, should be practically constant over the range of speech frequencies, and over the required range of input voltage amplitude." This paragraph really presents a problem of great difficulty and many ramifications. In the past, trial-and-error methods have evolved telephone transformers and repeating coils which would do all that was required, but with valve transformers the telephone engineer has had to call in the aid of most precise alternating-current measurement. Those who are accustomed to the design of transformers for power transmission will probably sympathize when they realize the form in which the problem arises. That is, to design transformers with equal transformation ratio at all frequencies from 200 to 2 000 periods per second, over a wide range of input voltages, to work on open-circuit or on a high resistance load, and to be wound with accurately-balanced opposing windings. Under these conditions it must be agreed that the present degree of success is very creditable. The transformer, however, is not the whole problem, but must be treated in conjunction with the valve with its 6 independent electrical variables, and it seems certain that the manufacture of both valve and transformer may be made easier by rather closer consideration of overall design. There has always been a tendency to take the valve as a complete unalterable instrument and adapt circuits to it. To a great extent this is an essential proceeding because of the limits to manufacturing possibilities. In power transformers the ratio of transformation is very approximately that of the number of turns, because the mutual impedance is high compared with the ohmic resistance. In the valve, transformer resistance (total circuit resistance) must be a much higher fraction of inductive factors, since the valve is essentially a high-resistance appliance. But within limits the valve can be modified on these lines. Again, the range of grid voltage and ratio of input transformer are closely connected with the amplification factor of the valve and its electrostatic capacity, and I think it would be rash to say that the present solution is the best. A further point is, the necessity for symmetry in the behaviour of the valve about the zero line of impressed alternating-current voltage, to which Professor Fleming referred. It is here that a characteristic curve may lead one astray, since a three-dimensional diagram is needed to follow the operation of amplification, while the mathematical analysis which would indicate the limits of distortion produced by a given lack of symmetry has not yet been worked out. I should like to warn those engineers who do not often deal with valves to beware of one thing, that is, the trap into which so many of us fell in the early days of valves, of looking at a characteristic curve relating to a selection only of the variables, judging the behaviour of the valve from that, and for-

getting the other characteristics which simultaneously control its action and which entirely alter the meaning of the first curve. It is remarkable that, in this country, attention was at first focused unduly on the grid-voltage/filament-current characteristic, while in America the anode voltage/filament-current curve seemed most to interest investigators. Both are equally important, but traces of the original over-emphasis occur even in recent books and papers. Again, a space-current/filament-current characteristic need not flatten right out before the operation of the repeater unit becomes substantially independent of filament temperature. It is the bearing of this curve upon the others that must be considered. In conclusion, I should like to emphasize the fact that a valve in a telephone repeater is put to a very refined use and pushed to the utmost limits of uniform behaviour, probably much more so than in any of its other applications. The valve has to amplify speech to satisfy an unskilled user; its associated telephone circuits are designed to a high degree of numerical accuracy; they are fixed and cannot as in even the most up-to-date wireless work be adjusted to the occasion by a skilled operator. Further, the valve and its repeater must be adapted for mass production and operation under conditions which will give economical maintenance and long life. However, one hopes that these facts will not lead telephone engineers to demand anything which will make manufacturing difficult, until painstaking test has proved its necessity beyond a doubt.

Mr. E. H. Shaughnessy: The author refers on page 389 to the 3-electrode thermionic amplifier. I should prefer the expression " thermionic tube amplifier " as distinct from " valve amplifier," because the word " valve " generally, at any rate to the wireless engineer's mind, conveys the impression of rectification, and, although de Forest introduced the vital third electrode in 1908, I think it was not pointed out until 1911 how to get rid of the valve-like action, and how pure amplification could be obtained without any distortion or rectification. The Post Office has been driven to underground trunk services, but it is important to appreciate that a circuit must be totally underground, and not partly overhead and partly underground. One can see that by loading, and putting in two repeaters between London and Liverpool, something like 240 circuits can be provided in one cable, and the total annual charges per circuit would be approximately one-third of those of a loaded cable of a gauge heavy enough to work the distance with repeaters. It would be interesting to know how the necessity of running the trunks underground in this country has affected the capital cost and annual charges per circuit, as compared with an equivalent service by means of copper wires overhead. I should like the author to give some information on this point in his reply.

Mr. S. M. Catterson (*communicated*): Reference is made in the paper to an improved type of loading coil introduced by the Western Electric Company. The use of this improved type of coil is of very great importance to the progress of development, and the following brief remarks give particulars of the improvements made, and describe how they have been produced. The use of loading coils of the type first introduced, in conjunction with repeaters, would provide a system which would be strictly limited in its commercial possibilities for two reasons. First, the earlier types of loading coils cause some slight speech-wave distortion which, whilst not greatly detrimental on a straightforward loaded line, would be highly detrimental on a line where this speech wave was amplified one or more times, as the greater the number of repeaters, the more detrimental will be the effect. In fact, the transmission scheme outlined in the paper for the United Kingdom would probably be quite impossible with the old type of coils, on this account. Secondly, as the author has pointed out, repeaters of the two-way element type require a balancing network, and the success of the system depends entirely upon the degree of balance between the line and its corresponding network. If the electrical characteristics of the line change it is obvious that an unbalance will result. The inductance of the earlier type of loading coil was not so stable as the present one and, whereas this was not so important on a straightforward loaded line where a change in inductance on a percentage of the coils would probably mean only slightly less efficient transmission, yet on a repeatered line such a variation would put the line entirely out of order. For this reason, also, the scheme outlined in the paper for the United Kingdom would probably be out of the question with the old type of loading coil. The improvements which have been effected in the loading coil are the result of much careful study and experiment and have been made possible by a change in the core construction. As is well known, the loading coil is of the toroidal type. In the earlier types of loading coils the cores were made of a bundle of iron wires, and the resulting coil had an effective resistance which varied somewhat with the frequency, giving the distortion effect to which I have referred. Moreover, if a direct current exceeding one-tenth of an ampere passed through the windings, there was the possibility of the core becoming magnetized, causing a change in inductance which gave rise to irregularities in the impedance of the circuit. As in a loaded section of line between repeater stations up to 80 or 90 loading coils would be included, the chances of trouble from this cause would be considerable. The new type of coil has a core made of compressed iron-dust, and the inductance characteristic of the new loading coil is such that the attenuation remains practically constant over the important range of speech frequencies, thus producing hardly any distortion of the speech wave. Furthermore, the coil is so stable magnetically that a current 20 times as great as that allowable for the iron wire core may be passed through its windings without any detrimental effect on the inductance. It is evident, therefore, that, whereas the old type of coil was satisfactory for the purposes for which it was designed, it is not at all satisfactory for the new conditions, and the new design of this coil to meet the demands of the new conditions is a fundamental step in the progress which has been made.

Mr. F. Jacob (*communicated*): In my pioneer patents (231 and 2 381 of 1882 for superposed circuits) not the " possibility " but the actuality of forming super-

posed circuits, in which the earth formed the return in the ultimate circuit, was definitely described, as was also the necessity, if this system was to be used to the best advantage, of successive twinning of pairs to form further pairs for what may be termed the progressive orders of phantom circuits. I would suggest also that the expressions "cross-talk" and "overhearing" should not be used indiscriminately, but that the former should be confined to the disturbance between two independent circuits, and the latter to disturbance of a phantom circuit due to defective electrical balance in its component pairs; thus the disturbance excited in one pair by speech in the other pair would be termed "cross-talk," and that by speech in the phantom on one of its pairs or vice versa would be "overhearing." The term "overhearing" would therefore arise only in the case of superposed circuits.

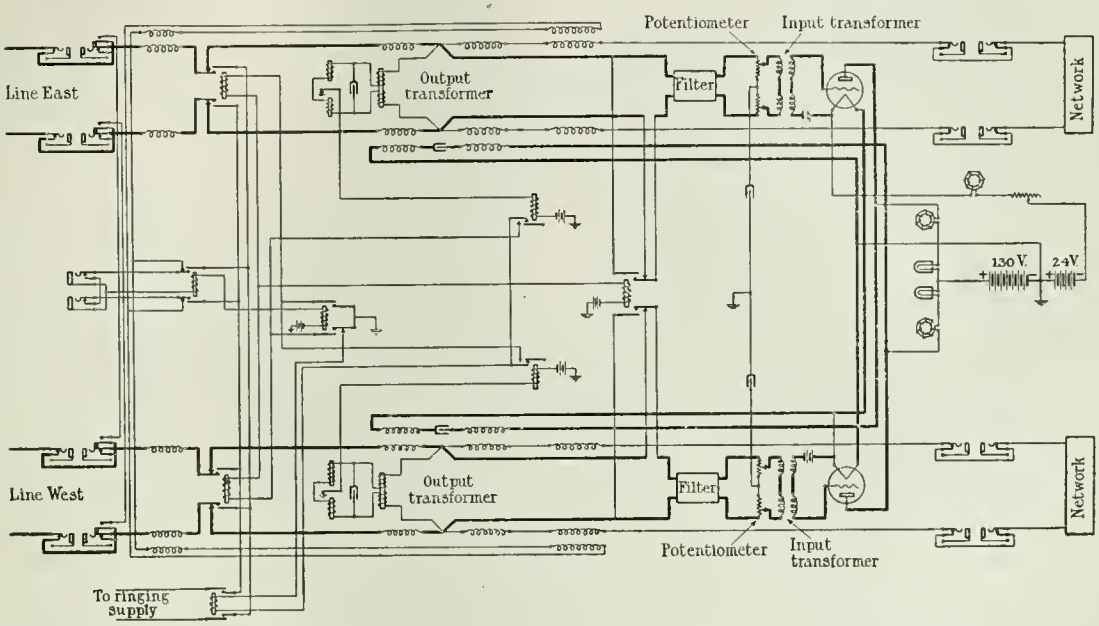

FIG. E.—Diagrammatic view of a commercial Western Electric type of two-wire repeater.

Mr. H. F. Kortheuer (*communicated*): The paper does not tell us much about telephone repeater equipment, and it may be of interest to describe briefly the general trend of circuit and equipment development in the United States. Vacuum-bulb telephone repeaters have been in commercial use in America since 1915; in 1918 approximately 700 repeaters were in actual service, and at the present time there are approximately 1 500 in use. Of this number about half are on cable circuits and the other half on open-wire circuits. The 2-wire repeaters in use on cable circuits are two-way repeaters with separate amplifiers for up and down conversations. Fig. E illustrates a commercial 2-wire repeater. It should be noted that this repeater differs from that shown in Fig. 6 of the paper. The repeater is equipped with filters which suppress frequencies above the useful limit, and thereby simplify the construction of the networks which balance the up and down lines. The circuit also contains potentiometers by means of which the degree of amplification can be controlled to suit the conditions of the circuit in which the repeaters are working. The arrangements provided for transmitting ringing signals in either direction are two systems of relays which respond to alternating ringing current, and by their operation cause a fresh supply of ringing current to be transmitted over the line. Signalling current having a frequency of 135 periods per second is used on lines arranged for simultaneous telegraph service, to avoid interference with the latter. Signalling current having a frequency of 16 to 20 periods per second is used where the circuit arrangements are such as to permit efficient transmission of this frequency. In more recent developments in signalling, the repeater is designed to amplify 135-period ringing current in a manner similar to amplification of speech frequencies. The use of this scheme simplifies the repeater set, as on 2-wire sets no signalling relays are required, and on 4-wire circuits only two relays are required for each amplifier. The circuit shows a number of jacks connected in series with the East and West lines. These jacks are located on panels adjacent to the repeater units. Their function is to enable the repeater attendant to connect the repeater to a testing circuit to make routine tests in order to determine that the repeater is functioning properly, and to make measurements to indicate that the proper gain is being given at all frequencies within the range of speech. These jacks also provide means for disconnecting the repeater from a circuit in case of trouble, and temporarily connecting another repeater in its place. Tests may also be made from these jacks to determine if the networks are in good condition. The two jacks shown on the left-hand side of the circuit are monitoring jacks, which enable the repeater attendant to connect his telephone to the repeater circuit in order

to listen-in on the circuit. The development of commercial equipment arrangements for repeaters in America has resulted in several different forms to meet the various requirements of different classes of repeater installations. The type first installed in large numbers was a self-contained unit about 1 ft. wide, 1 ft. deep and 6 ft. high. On this unit were mounted all of the parts of the repeater shown in the previous circuit, as well as the testing jacks. The relay rack type was the next step in the development of repeater mounting arrangements. The American Telephone and Telegraph Company at present have under construction an extensive cable system between New York and Chicago. The length of this cable will be approximately 900 miles. There are 17 repeater stations. The 4-wire type of repeater is to be used on the circuits extending the full length of the cable, while 2-wire repeaters are to be used for intermediate circuits. The approximate number of repeaters in each of the 17 offices will be 100. Each of the pairs in the cable, in addition to being utilized for telephone circuits, will also provide accommodation for a metallic-circuit duplex telegraph channel.

Mr. A. Morris (*communicated*): As indicated in the summary, the paper deals principally with the improvements which have been made during the last decade in line-plant design, and in the engineering methods of providing for the transmission of speech. The very early historical side of the subject is also dealt with. In view of this I suggest that some idea of the "development aspect" of the paper might be conveyed by a suitable amendment to its title. The account of the classic experiments of Tremain and Martin in 1899 on the cross-talk in underground cables is particularly interesting, since it shows that not only the principal cause of cross-talk in telephone cable lines, namely, mechanical distortion and asymmetry of make-up, but also its elimination by the "crossing method" was well known to the Post Office Engineering Department over 10 years ago. In dealing with the modern method of eliminating cross-talk, the author rightly draws attention to the fact that the basic principle adopted by Tremain and Martin in this connection is still followed. Perhaps it should be emphasized, however, that whereas in 1899 the most suitable method of connecting 4-wire cores of adjacent lengths of cable together was determined by experimental trial involving measurements of actual disturbance both before and after jointing, the present method of actually measuring those electrical characteristics which are contributory to cross-talk enables the best mode of connection to be calculated and the final result accurately forecasted. This is rendered possible by virtue of the completeness of the knowledge of the effects of each of the eight methods of connection upon the electrical characteristics of cable cores. In regard to the absolute measurements of capacity and resistance which were made by Tremain and Martin, it should be made clear that they were used only in a very indirect manner in the investigation into the elimination of cross-talk. The degree of accuracy, especially of the capacity measurements, is surprising. In conclusion I wish, as a colleague of the late Mr. C. E. Hay, to endorse the author's remarks and public expression of appreciation of his scientific work. The solution of many telegraphic and telephonic problems has been rendered possible by the ingenuity of his apparatus design, and by the skill displayed in his precision measurements.

[The author's reply to this discussion will be found on page 425.]

Scottish Centre, at Edinburgh, 8 March, 1921.

Mr. R. Waring: The paper introduces two features in connection with long-distance telephony that were introduced many years ago in long-distance telegraphy, namely, the duplex and the repeater. The familiar differential duplex principle reappears in its telephone application in Figs. 2 and 6. But while the compensation circuit—or the "balance" as the author describes it—presents no fundamental change from its telegraph prototype, there is a striking difference in the repeater. There is no mechanism; it has no moving parts. So long as the early workers followed telegraphic lines and sought a mechanical solution there was, to use the author's words, but moderate success. The thermionic valve was a complete new departure, and its success lies in the absence of moving mechanism by which functions have to be performed by the exceedingly small variations of current that are characteristic of the electric waves in the transmission of speech. The author refers to the early troubles with short lengths of air-space cables, due to lightning. This was a real difficulty at the time, but I think some of it was due to workmen following the practice which had obtained when gutta-percha-covered conductors were in general use, i.e. if there was a foot or two to spare, it was left in a coil in the joint box at the foot of the pole. At any rate, it was at places such as these that I have found one or two faults caused by lightning. Perseverance in tracking down and eliminating troubles from lightning reminds me of similar efforts in connection with breakdowns in the first loading coils installed in trunk cables. These coils were coreless—an iron core was then considered to be detrimental—and they were built up on wooden bobbins. I was associated with Mr. Tremain in the work of loading the first Liverpool–Manchester trunk cable with these coreless coils in the nineties, and also in the subsequent task of cutting some of them out when they became faulty. Throughout the whole range of the work on underground cables a dogged struggle with difficulties has been waged. The author has shown the work as it stands to-day. He has touched but lightly on the scaffolding, as for example in the brief remark (on page 393) that over 90 000 capacity tests were made on the Leeds–Hull cable, and again on the same page where he sums up in the few words: "It soon became evident that the very success of the first cable had opened up some new problems." These are hints only; the work behind them, which I have called "the scaffolding,"

is left to the imagination. There is one point to which I will refer in conclusion. Scotland has had since 1906 a stable telegraph service to England provided by means of the Edinburgh–Glasgow–London underground cable. We now see a similar stability in sight for the telephone service. With a stable and prompt service, there will surely be a marked development in traffic which should belie those figures relating to Scotland in the table on page 390, which show that Glasgow, with 6 circuits to London at present, is expected to need 12 only in 1930, as compared with 80 between London and Liverpool. I think that the G.P.O. will require to modify their estimate of development as regards the London–Glasgow traffic.

Mr. J. R. M. Elliott: On page 395, in paragraph (*e*), one of the conditions which the ideal valve should fulfil is stated to be that the electrodes and their supports should be sufficiently rigid to ensure that no vibrations from external sources affect the performance of the valve. This is a matter of the utmost importance and is one which we may hear more about as the valve becomes more generally used. I have heard complaints of the want of rigidity in valves of British manufacture and I have also heard it stated that the modern German valves are much superior in this respect. If this be correct, it behoves British manufacturers to lose no time in attending to this serious defect. I should like the author to tell us of Post Office experience in this connection. In connection with the map on page 399, the present trunk centres have grown up by degrees, and through force of circumstances have become situated in large towns. In adopting the most economical lay-out of route, new trunk centres may have to be established, but in some cases these may not coincide with the large towns, as in the past, and may actually be thrown into country areas. Perhaps the author will say whether this is likely to take place to any great extent. On page 401, reference is made to a temporary repeater station installed at Leicester. It is unfortunate that a special effort could not have been made to provide the buildings at Northampton and Derby in time for the completion of the cable. Temporary expedients, particularly in the matter of premises, have nowadays a tendency to become more or less permanent and, so long as it is possible to carry on, the question of efficiency, or otherwise, is apt to fall into the background when the costs of new premises are under consideration. I notice that no reference has been made to the insulation resistance of cables. It seems to me that this is rather an important matter. The author may have had some good reason for this omission, but I should like to know whether it is necessary to maintain any minimum standard of insulation for the long circuits shown in the map. In Appendix II, details are given of the thermionic valve used by the Post Office for telephone repeaters. The amplification factor defined by the ratio "internal impedance : mutual impedance" is to be not less than 6·75. As it is apparently to be inferred that the value of the amplification factor is to be somewhere in the neighbourhood of 6·75, perhaps some information could be given as to the reason why a higher value is not adopted.

Mr. A. B. Gilbert: The development of the telephone repeater will be the means of bringing towns much nearer to each other. Perhaps the author will tell us what the gain from the telephone repeater is likely to be in this respect. It would seem that the country will, in the near future, enjoy the use of trunk lines between all important centres, not only free from disturbance and breakdown due to bad weather, storm and other conditions as a consequence of the extension of the underground cable system, but with a volume of speech received from the distant end of a trunk connection so enhanced by the introduction of the telephone repeater as to give the public greater confidence than formerly in conducting its business over the trunk lines.

Mr. W. Wood: Apart from the technical information that the paper contains, the history and development of long-distance telephony from its inception up to the present time shows what wonderful strides have been made. The author shows the high scientific research and numerous tests which have to be carried out to eliminate cross-talk from other circuits of all kinds in the same or adjacent cables and on open lines. The thermionic valve, of which the author has said so much, is now one of the most important pieces of apparatus for long-distance telephony. I should like to draw attention to an important matter, i.e. inductive interference from power circuits, especially in view of the development of high-power overhead transmission in various parts of the country which will take place in the near future. In America the question has been gone into most thoroughly and a Committee has reported in the greatest possible detail on the construction and operation of power and communication lines for the prevention or mitigation of inductive interference. Close co-operation will be required between telephone and power engineers, and unless definite instructions and rules are recognized by both, the problem is well-nigh insolvable. As in the case of disturbance from neighbouring telephone and telegraph circuits, which the author says have to be treated to suit varying conditions, so will different conditions obtaining on power lines of different formation and loads have to be separately considered and the special methods of barreling or transposing agreed upon between both parties. As is well known, residual voltages and currents are a principal cause of inductive interference and it is interesting to notice the effect of wave-shape on interference. Wave-shapes on different power systems in practice are found to have very wide variations: an unfavourable wave-shape, that is, one having a high proportion of harmonics, is most likely to make telephone circuits inoperative. This points to the importance of strict attention being given to the design of machinery. At the present time it is almost impossible to tackle the problem satisfactorily unless rules are laid down. The Post Office, railways, and power companies have research departments of very high standing, and organization has only to be set up to deal with this matter.

Mr. J. D. Taylor: The author has dealt with the subject from the point of view of how it affects the United Kingdom as a whole, but it may be useful if for a moment or two we take a local view. In 1896

Edinburgh was connected to the outer world by 8 long-distance telephone circuits; in 1912 the number of such circuits had increased to 97, and, in 1920, to 164 circuits carrying about 8 000 long-distance calls per day. It is interesting to observe that, in spite of this heavy load of long-distance traffic, in addition to the large number of local calls, 99 per cent of all telephone calls which originate at Edinburgh are effected without delay. A telephone subscriber at Edinburgh, who "calls" the exchange for a subscriber at, say, Glasgow, is connected within a few moments to the required Glasgow subscriber. Not very long ago a subscriber at Edinburgh had to wait an hour for his turn for a connection to Glasgow; now the requirement is met on demand. The following comparative details of the long-distance telephone service of the United Kingdom indicate how the service has grown. In 1898 the number of working trunk circuits was less than 1 000; to-day it is nearly 7 000. The mileage of trunk circuits in 1896 was less than 30 000, and was nearly doubled in the two years from 1896 to 1898. It increased by 15 times between 1896 and 1920, and the total mileage is now nearly 450 000. The increase in the average value of calls between 1898 and 1920 was $1\frac{1}{2}$ times. The revenue from trunk circuits in 1898 was about £130 000 per annum; it was £1 860 000 per annum in 1920. The number of trunk calls has risen from 5 000 000 in 1898 to between 50 000 000 and 60 000 000 to-day. The provision of backbone trunk telephone circuits by means of underground cables, fully efficient telephonically and free from damage by storm, is a very big step forward. It is a step which can perhaps be appreciated fully only by those who have had to face and adjust the dreadful havoc wrought by storms to overhead telephone lines. The author draws attention to the point that specially long and important telephone circuits form a group quite distinct from shorter local circuits, and in this connection I would raise the question of signalling "service" traffic over the longer circuits. It is, of course, most important in the case of long-distance telephone circuits, which are costly to provide and maintain, to ensure that the maximum time shall be available for revenue purposes. The non-revenue-producing time occupied by an operator in arranging calls, and also the need for severing each connection immediately the conversation has been completed, are factors which emphasize the importance of having the most efficient signalling system on long-distance telephone circuits. At one time, telegraph circuits were superimposed on certain telephone circuits, and these telegraph circuits were used to pass to and fro the details required by each operator to effect the desired connections. This arrangement had certain defects, but it certainly had the great advantage of not encroaching on the revenue time for record service work. With stable, almost fault-free underground telephone loops, it should be possible to provide satisfactory call or service circuits by superimposed telegraph circuits in cases where the very costly and important main circuits are loaded with revenue traffic. It now appears certain that the telephone repeater will ultimately enable the engineer to construct plant which will permit standard speech to be enjoyed between any two telephone stations, regardless of the distance between them, and will also make practicable the provision of telephone circuits which a few years ago could not be entertained.

Mr. A. B. Hart: Mr. Elliott said he had heard it stated that valves obtained from Germany were very much better than could be got in this country. I am not a valve manufacturer, but I have been associated with valve developments in this country almost from the beginning and I am sure that any valves obtained from Germany could be made quite as well here. The glasswork in the German valves is of a very high order, perhaps better than anything we could do, but I believe that we can produce valves in this country that are equal as regards efficiency to any produced in the world. Soon after the Armistice I had occasion to be in Cologne. We heard that the German administration had set up a telephone circuit between Constantinople, Vienna, Berlin and Brussels and some point in France, linked up by telephone repeaters, of which one was said to be stationed at Cologne. I went to Cologne and, with the very kind assistance of the Officer Commanding, brought back some important parts of this German repeater and examined them in London. We concluded that the German telephone repeater was less efficient than our own. I think it only right that we should take credit for things of this kind. The Germans have been very much to the front in electrical science, but I think British manufacturers are to be complimented on the quality of valves turned out during the war, especially when one considers the conditions under which they were manufactured. Appendix III contains a reference to a submarine cable of 28 cores. Will the author say where this cable is to be laid? In Appendix V, I notice in the last column that the leakance factor is given as "unreliable" at the high frequencies. As I had something to do with those tests perhaps I might be allowed to explain that the apparatus with which they were made was the best that was available at the time. At 30 000 periods the figures which should have been in that column would have been negative, that is to say, we should have been showing a leakance which had a negative value. What this would mean I do not know, and I think that perhaps it may be accounted for by some apparatus error.

[The author's reply to this discussion will be found on page 425.]

North-Western Centre, at Manchester, 22 March, 1921.

Mr. W. J. Medlyn: The author has dealt with the comparison between gutta-percha-insulated conductors and the newer methods of paper insulation in lead-covered cables from the point of view of efficiency in speech transmission, but another question of very great importance is that of cost. Apart from the additional duct space required, the average cost per line in the older type of cable was so great that its extensive use would

have been a heavy financial burden tending to retard the natural development of the service. On page 390 mention is made of the traffic development since the trunk lines were taken over by the Post Office in 1896. I have looked up some statistics on this subject which may perhaps be of some interest. The first record shows that during the year ending 31st March, 1898, there were 5 888 274 trunk calls or, in round figures, say 6 millions. For the year ending 31st March, 1906, this number had increased to 18 millions, and seven years later to 36 millions, whilst the latest published statistics for March 1915 show that the total had risen to 40 587 833. The table given on page 390 indicates that provision is being made for a big increase on the last-mentioned total during the next few years. Manchester telephone users are, of course, particularly concerned in the facilities for intercommunication with the surrounding important towns, owing to the intimate trade relationships which exist. There is already a complete network of cables between all the South Lancashire towns, including Manchester and Liverpool, and extending North as far as Preston and Burnley, and also into Yorkshire. We have faith in the development of the system, because in spite of the serious set-back due to the war and the consequent criticism, we are sparing no effort to improve the service, and we know that substantial progress has already been achieved. One of the most important improvements is to increase the facilities for intercommunication between the main centres of industry, and in most cases a call originated in Manchester can now be put through almost at once to the principal towns in South and Mid-Lancashire. The capacity of cables already working is being rapidly increased For example, there are now 132 lines working in existing cables between Manchester and Liverpool, and a new cable carrying 160 additional lines is nearly completed. Additional lines to carry the traffic between Manchester and Oldham are needed, and the work of laying a new cable between those centres is also well advanced. The author refers to the provision of a new cable between Manchester and London, and we hope that the additional lines there will be available in the course of a few months. There will be a repeater station in Manchester to work in connection with that cable. Other extensions are in hand, and we are looking forward to the time, which I think is not far distant, when intercommunication between all the important Lancashire centres will be given immediately on demand, with an extension of similar facilities to many of the more distant places, and when the Post Office staff, as well as the telephone users, will have little cause for anxiety regarding the stability of the service, which will be just as secure during the period of winter storms as at any other period of the year. Two years ago Manchester people suffered severely from a breakdown due to a snowstorm, but by the underground extensions that are now in hand we hope to relieve ourselves of the anxiety of maintaining a service under those conditions, and consequently we shall be able to give a more stable, secure and regular service to the telephone users. The methods by which such results are rendered possible have been fully explained in the paper.

Mr. L. E. Wilson : The electrical properties of a telephone line are not generally understood, and very few people outside the industry realize the complications. Telephones now being a specialized industry, it is of great interest to many to know how these transmission difficulties are being overcome, and I make an appeal to those engaged in this industry to give us technical details of these important services, and not leave it to the lay Press to provide the public with misleading statements.

Mr. T. E. Herbert : The development of the work of cable-balancing initiated by Messrs. Tremain and Martin to its present-day form constitutes a most remarkable achievement. It is to me an ever-constant source of wonder that delicate tests of such extreme accuracy are made on the roadside by means of the specially equipped motor van. The details of the apparatus used, together with an account of the principles involved and detailed instructions for the organization of such work, will shortly be available, and I think it will be generally admitted that the translation of laboratory work of high standard into ordinary everyday routine might not inaptly be called a " Romance of Industry." Another treatise might be written on the subject of the fault localization and routine testing of dry-core cables. The precise position of a fault is not usually indicated by the column of smoke which is said to characterize some of the faults on a power distribution network, and methods of extreme accuracy have had to be evolved to meet the requirements. In 1896, when the trunk lines were acquired by the Post Office, there were 23 circuits between Manchester and Liverpool. The first 13 were constructed by the National Telephone Company, 8 by the Mutual Company, and 5 by the Post Office. All the circuits were carried on pole lines, and they were never all in working order on one day. The circuits constructed by the Mutual Company were an endless source of trouble, and 18 of these lines had to be reconstructed. To-day there are 132 lines with a further 160 in course of provision by cable. These circuits are independent of the vagaries of the climate, and the telephone user will ultimately have the best service in the world. But the completion of the vast schemes necessary to obtain the full benefit of all the work done necessarily takes time. The supplies of raw materials and their conversion into lines and apparatus have thrown a huge burden on the manufacturers, and I, for one, fully realize the immensity of the task confronting them. Here in Lancashire, where we are in close touch with the directors of some of these large concerns, it is not difficult to appreciate what they have done and are doing to provide us with the requisite supplies. I should like to say that I strongly resent the campaign which has been waged against the telephone system. It is, of course, quite unnecessary to enter into any details, since the paper will have demonstrated how much has been done towards the attainment of our ultimate goal, which is an ideal telephone service. The author has shown what the Post Office Engineering Department has done in the design and provision of the trunk system, and needless to say, the operation of the plant designed and provided by the Post Office has been given equally careful and

detailed thought by the officers responsible for the supervision of the operator's work. For obvious reasons the precise financial effect of the various developments sketched in the paper have not been dealt with, but it is only fair to say that this aspect of the subject is constantly under review and the economies effected are as remarkable as the scientific achievements now so admirably summarized. The thermionic valve appears to have solved most of our problems, but I venture to say we are only at the beginning of its development, and here I should like to make the suggestion that possibly the receiver microphone relay may perhaps have to be rescued from the scrap heap to enable trunk circuits to be separated electrically from the lines of varying length to which they have to be extended. As the author has adumbrated, a number of these problems resolve themselves into questions of economics, and there are many cases where it is cheaper to provide extra lines or lines of heavier gauge than to provide the auxiliary apparatus necessary with lesser line plant. At the present time such remarks apply to the carrier or "wired wireless" system. In its present stage of development it has several limitations, but I believe that in the future it will be possible to do much more at a greatly decreased expenditure. It may not be here inappropriate to refer to a paper by Messrs. Collpit and Blackwell on "Carrier Current, Telegraphy and Telephony," read before the American Institute of Electrical Engineers, in February, 1921. This paper brings this part of the subject up to date so far as it has developed under American conditions, but it does not, of course, include details of the work done by the research section of the Post Office Engineering Department.

Mr. G. S. Wallace: I am glad that the first part of the paper provides an opportunity for those who are concerned with the manufacture of telephone and telegraph cables to voice their views on the problems that have been put to them by the Post Office authorities during the past few years. They are concerned, not only with the electrical properties of the cable, but also with its size and mechanical strength, and the cost of special machinery for handling a non-standard type of cable must in certain cases be very heavy. Now that the war is over, much attention will doubtless be given by engineers in America and on the Continent to the question of the most efficient and economical cables for long-distance telephony, and I should like to ask the author if the time has yet arrived when cables can be balanced and manufactured in the factory on more standard lines than they are at present. One can quite realize that up to a few years ago the difficulty has been to obtain any commercial speech transmission on ordinary standard telephone cables of over 40 or 50 miles in length, irrespective of cost, but sooner or later the cost per circuit in the cable itself will be a vital factor and will need to be kept as low as possible. It would be interesting to know if it would be feasible for the cable research engineers and the cable manufacturers of this country to arrange with their colleagues in other countries for an International conference with a view to the design of certain standard types of long-distance cables which would meet general requirements. Cables of different designs may give equally satisfactory results in practice, and the cost of manufacture would be reduced and the rate of delivery accelerated if the principle of standardization could be adopted. I should also like to know if excessive strains or stresses during the process of "drawing-in," etc., would be likely to upset the balance of these carefully designed cables by altering their wire-to-wire capacity, and also whether even a slight reduction in the insulation of the cable at certain points would not set up cross-talk sufficient to give ground for complaints from trunk users. Even with underground cables there is always a possibility of damage, and I should be glad if the author would tell us what should be done to minimize the duration of the breakdown in cases where a section of balanced cable has to be taken out and replaced by a new length of cable, as this might upset the balance of the whole of the circuits concerned. I am now referring particularly to cables in which a considerable number of wires may have been crossed for balancing purposes. In view of the improvements that have been made in thermionic valves during the past five years, and the ideal conditions that they should fulfil, I should like to ask the author if he thinks that there is still scope for material improvement in this direction, and whether British firms are alive to the competition they are likely to meet in America, Germany, France, and Italy, both as regards present types and improvements. I should particularly like to know if there is any chance of a valve being evolved which will take a current less than one ampere. About two years ago there was some talk of a "negative resistance" valve, and its application to repeater work was, I understand, about to be investigated by the research section of the Post Office. I am sure that any information regarding that most ingenious device, if it is likely to become a practical proposition, would be very interesting. Figs. 14 and 15 are very interesting, especially as Manchester is shown as a proposed repeater station. What is the circuit capacity of the repeater station shown in the latter figure? Presumably at the smaller stations—such as Taunton, Edinburgh, Ipswich, etc.—a much simpler scheme will be possible, and I would suggest that, where possible, the existing test rooms and battery rooms might be utilized, as there is no doubt that in a few years the average exchange maintenance faultsman will be able to tackle a repeater trouble as readily as he does an ordinary relay fault. A material saving in maintenance cost might result from such an arrangement.

Mr. T. Plummer: In the reference in the paper to interference caused by single-wire telegraphs and loop Wheatstone working, no mention is made of the use of single screened conductors in the outside layer of a composite cable, i.e. single wires with an outer lapping of thin copper tape over the paper insulation, all the lappings making contact with one another and the lead sheathing. From an historical point of view it seems desirable to have on record the results of the investigations for eliminating disturbance by means of this device. Some additional information on the improved methods of localizing leakage faults on these long-distance balanced and loaded cables, which are

not led in at any intermediate point for sub-localizing, would also be useful. The results are very accurate, and it is no mean achievement to be able to localize a fault to, say, 100 yards in a cable of 100 miles in length, bearing in mind that the leakage looked for would be of the order of 5 000 megohms per mile, and that the fault has to be cleared before those using the circuits for long-distance conversations become aware of its existence. The reference in Appendix I to the Birmingham–Liverpool cable being in course of construction was an error. This cable was brought into use only two weeks before the big breakdown due to the snowstorm of 1916, referred to in the paper, and, as stated, it saved the situation generally in maintaining communication between London, the Midlands and the North. It is interesting to note from the figures in Appendix II the variations in the constants of two identical cables, i.e. London–Birmingham and Birmingham–Liverpool, manufactured and laid under the same conditions by different contractors. In regard to telephone repeaters, their maintenance is very simple, and one man can look after a number. They require scarcely any attention on underground circuits, where the conditions are stable, and once the current has been switched on, and the heating current adjusted for the valve filament, they practically run themselves all day. This is a very important matter in regard to the economical working of the arrangement. The paper contains no reference to the arrangements for repeating the calling or clearing signals on long-distance calls. These are rather complicated and relatively much more difficult to provide for than the speaking, in view of the practice of introducing transformers at the cable ends. Some information on this phase of the subject would be useful. I should like to know the results of the trials of two valves in series for telephone repeaters. In wireless working, as many as six amplifiers are used in series to pick up weak signals. Is the extra sensitiveness secured in this way any improvement for long-distance talking? The longest underground trunk circuit in this country at present in everyday commercial use is that between London and Hull. The route followed is via Birmingham, Derby, Sheffield and Leeds, a distance of about 300 miles, with telephone repeaters in circuit at Birmingham and Leeds. This circuit, which is included in the pioneer balanced cable between Leeds and Hull, referred to in the paper, gives very good results both for volume of speech and articulation.

Mr. A. E. White: It is stated on page 401 that for long-distance cable circuits the 4-wire circuit will be widely used. What will be the minimum distance for which that type of circuit will be introduced? Presumably the 2-wire circuit with the double differential repeater, as shown in Fig. 6, will be the arrangement adopted for the London–Manchester circuits. In the event of the thermionic valve failing to function, is there any automatic arrangement whereby the attention of the repeater station can be called? The cable chute in Fig. 15 is shown close to the terminal transformer racks, and this seems to indicate a departure in the termination of the lead-covered cables. Hitherto it has been the practice to terminate them on the main distribution frame before connecting to apparatus.

Mr. T. E. P. Stretche: I hope that any reports of this paper which may appear in the non-technical Press will not have this result—that every time we require to erect a few wires in an urban or rural district the inhabitants will expect the line to be placed underground—because such a course is obviously impracticable.

Mr. J. S. Terras: In connection with the diagram of the main underground cables which the author showed on the screen, it is doubtless understood that it does not represent all the trunk telephone cables in the country, but only those that are intended to be used with telephone repeaters.

Mr. A. E. Banks: The primary object of the paper is doubtless to place upon record in the annals of the Institution the development of long-distance telephony since 1905, but incidentally it also serves another purpose. Coming at a time when the administration of the telephone service occupies the foreground of public attention, it should go far to show that the Post Office have not failed to recognize, develop and apply, any advance in electrical science which could be utilized to the advantage of the telephone system. It is significent that each step towards improved speech transmission and extended means of communication has been accompanied by an increased volume of traffic, and this is a sure indication, if indeed any were required, that a definite field exists for still greater development in these directions. Coming now to the type of valve described in the paper I should like to know if the exact value of the heating current required can be given, and also what is now regarded as the best method of ensuring that this value is not exceeded. Messrs. Robinson and Chamney suggested in their paper two years ago that the heating current should be increased until the mutual conductance of the valve reached a predetermined value. Perhaps the author will say if this method is still considered to be the most satisfactory, and also, in regard to signalling, whether he is in favour of the method of applying a vibrating reed so as to produce currents of a frequency suitable for transmission by repeaters. Some information as to the design of the continuously loaded submarine cable and as to the length and type of overhead circuit on which it is proposed to introduce multiplex telephony would also be interesting.

Mr. J. M. Jackson: The Post Office engineers have done very well in originating a system of localizing high-resistance faults. A short time ago, following the new method, I tested out a fault on a cable between Manchester and Bolton in conjunction with one of the engineers at Bolton. The resistance of the fault was about 5 megohms; we made a localization test and we located the fault at once.

Mr. L. H. Crane: The particulars of the trunk-line underground scheme shown in Fig. 4 are especially interesting. In regard to the proposal for a cross-country overhead system on steel towers, it would be interesting to know what special wayleave powers, if any, were contemplated in connection with this scheme. A special Act of Parliament would, I presume, be required on the lines of the Acts required for the construction of railways. The present powers of the

Post Office would be quite inadequate. From a consideration of the repeater circuit shown in Fig. 12 I understand that the Up line cannot speak at the same time as the Down line. If so, will it not in practice lead to trouble in cutting off, owing to both ends endeavouring to speak at the same time? This would be most liable to occur when the connection has just been established, and the subscribers are calling to find if they are through. Is there any connection between the 4-wire repeater shown in Fig. 13 and the new 300-pair, 20-lb. trunk cables, and have these cables been specially designed to work with this circuit? It would be interesting to know the number of lines the telephone repeater station shown in Fig. 15 is designed to accommodate. Will it be six 300-pair, 20-lb. cables, or six 160-pair, 40-lb. cables? I presume the engine and dynamos are for the purpose of charging the filament batteries, and the motor-generators for charging the plate batteries. Apparently, to judge from the size of the filament batteries, the valves are in circuit with the battery permanently, or at least during the daytime. Is it not possible to arrange the circuits so that the valves are only in circuit when required, namely, when the line is connected at both ends by either operator or subscriber? I should think that suitable relays could be designed for connecting in the balancing circuit of the repeater shown in Fig. 6 to effect this purpose. This would also enable automatic calling and clearing signals to be arranged for. Perhaps some form of polarized relay might solve this problem.

Mr. C. E. Morgan: I understand that the application of the 4-wire repeater circuit, on which a zero standard cable equivalent is obtainable, is limited to circuits in the region of 200 miles in length. In connection with the 2-wire repeater circuit, apparently we cannot improve the transmission to less than a standard cable equivalent of 12 miles. In a number of cases we have a demand for transmission values in the neighbourhood of 7, 8 or 9 standard miles for cross-country circuits, and it seems to me that it means the undesirable retention of the overhead lines or the expensive alternative of providing heavy-gauge underground conductors. I should like to know whether any further improvements may be looked for in the application of these repeaters for short-distance circuits.

Mr. E. P. Hill: What importance does the author attach (in view of the general adoption of underground trunk telephone systems) to the interference from adjacent power circuits, of, say, a harmonic of 10 to 15 times the normal frequency, assumed 50 periods per second, and what influence will the proposed thermionic-valve system have upon the extent of this interference?

[The author's reply to this discussion will be found on page 425.]

Liverpool Sub-Centre, at Liverpool, 11 April, 1921.

Mr. A. J. Eames: This paper embraces a survey of recent progress and improvements in long-distance telephony, and I should like, for comparison purposes, to refer to two earlier papers especially interesting to telephone engineers in this country. They were brought before the Institution in 1896: one was entitled "Telephone Exchanges and their Working," by Mr. Dane Sinclair, and the other was by Sir John Gavey, on "The Telephone Trunk Line System in Great Britain." The latter is mentioned in the present paper. The developments which have since taken place in both subjects can only be regarded as very remarkable. Until 1896, and in fact for some years afterwards, a telephone engineer's aim was to carry a long-distance telephone circuit entirely on aerial supports; even short sections of underground route in the circuit were looked upon as detrimental to satisfactory telephone speaking, as in fact was the case at that period with the telephone plant then in use. Yet Sir John Gavey evidently anticipated considerable underground cable developments, for he suggested in his paper of 1896 that the introduction of air-space cables by the use of paper insulation appeared to offer the best solution of a difficulty then looming, namely, to find space for the numerous wires required between busy centres. It is doubtful if, at that time, there was absolute confidence that a long-distance service provided wholly, or mainly, by means of underground wires hundreds of miles in length would become a practical proposition; but it is now authoritatively stated that long-distance balanced cables, loaded and equipped with repeaters, are decidedly superior to long aerial circuits, at least in this climate. Although such a great deal has been achieved since 1896, some difficulties still remain. For instance, one of the problems still requiring attention appears to be the determination of the best arrangement or distribution of loading—spaced or continuous—in an underground circuit, to harmonize with the working conditions required for producing the best effects by the use of repeaters, on one pair of wires, for any required distance. It is remarkable that, although it was no doubt intended to provide the maximum number of circuits in each of the London–Liverpool and London–Manchester telephone cables, 75 circuits, including phantoms, was the best that could be done in the former case in 1916, as compared with the 240 circuits in the latter case in 1921. The same size of duct was available in each case. It is, I understand, expected that the London–Manchester cable will provide, alternatively, 104 pairs for telephone, and 56 pairs for telegraph circuits, with satisfactory results. The improvements would appear to hold out a good prospect of relieving some of the difficulties in regard to the provision of local exchange plant, by the efficiency of the trunk lines being gradually raised. The margin for exchange-line extension circuits, considering that these have to be efficient for trunk use, is rather small, especially when dealing with the more exceptional cases at the larger centres, where trunk junctions are sometimes of appreciable length. Certain exchange-line extensions can only be provided by bunching wires to reduce

their conductor resistances so as to keep them within specified standard cable limits, or other means almost equally unsatisfactory have to be adopted. If all trunk circuits are eventually improved so that none exceed, say, 12 or 15 standard miles, this would admit of further facilities being given to telephone users.

Mr. H. H. Harrison: It is interesting to trace one or two of the steps by which the present stage of telephone transmission has been reached. The author has pointed out that one of the most important early steps was Edison's duplex circuit. If for the valve shown in Fig. 2 the earlier microphone telephone receiver amplifier be substituted, we have the Edison circuit. The next step was the invention of the double-amplifier type of repeater by a member of the American Telegraph and Telephone Company's engineering staff. A further step was furnished by the mathematicians, who showed us how to construct a simple network of condensers and resistances which would behave externally in the same way as a line with distributed constants. This has made possible the balancing networks and the frequency-band filters which play so important a part in modern long-distance telephony. One might be inclined at first sight to condemn the four-wire repeater method as being an uneconomical way of solving the problem, until it is remembered how enormously the weight of conductors can be reduced as soon as repeater stations are introduced *en route*, and how advantage may be taken of the new methods of multiple telephony. The extended and full use of wires which modern methods allow, settles all doubts as to the four-wire scheme. Carrier-current multiplex telephony which is mentioned at the end of the paper is by no means new, and it is interesting to trace its history. The modulation of current at a frequency above that of audibility by a low- or audible-frequency current was proposed in 1887 by Hutin and Le Blanc, and also by Cornelius D. Ehret. Hutin and Le Blanc aimed at multiple telephony by the use of several carrier currents at different frequencies. They realized the need for making the modulated high-frequency current audible, and they proposed an electrodynamic telephone receiver which would vibrate in response to the audible frequency of modulation. Ehret had a different object in view. He invented an ingenious single-frequency rotary amplifier, and the carrier-current method of transmission was the logical one to use. I believe that none of these early inventors put his ideas to actual test, a possible reason for this being the lack of a high-frequency generator. Multiplex telephony or telegraphy is far from being generally adopted in Europe, due to the huge losses in the underground cables which inevitably form part of a long-distance line. The problem calls for a special design of cable, and until this is accomplished no great expansion is to be expected. Apart from the full use of wire plant effected by carrier-current transmission, it does not seem to be generally appreciated that a circuit employing this method gives an absolutely silent line and that consequently speech is possible. We have seen telephone transmission revolutionized by the adoption of loading, while loading itself can be partially dispensed with by the use of the valve amplifier. It is interesting to speculate whether the valve itself is to become displaced by, possibly, the mechanical rotary amplifier.

Professor E. W. Marchant: I remember travelling to London the day after the blizzard in 1916 and seeing the havoc that had been wrought to the overhead wires in the Midlands. The train had to be hand-signalled from Crewe to London. The amazing thing was that the Post Office was able to maintain a telephone service at all. That, of course, was due entirely to the fact that there were telephonic cables. What interests me most on the scientific side of this paper is the great possibility of development in trunk telephones which is opened up by the application of the thermionic valve. I was rather surprised to hear Mr. Harrison suggest that this might be superseded by the rotary amplifier for telephony. It seems to me that the thermionic valve is almost an ideal piece of apparatus, because the moving parts of the relay are incredibly small. The electrons traverse the space between the filament and the plate, controlled during their passage by the incoming telephone currents, and I cannot imagine any mechanical device so efficient as a relay. I had an opportunity on Saturday of testing the line between London and Liverpool and I was very impressed by the excellence of the speech, which was far superior to that on local services. The " wired wireless " system is extremely interesting, and appears to be the best way of effecting multiplex telephony. That the system should be silent except as regards the high-frequency currents which are being used, is almost obvious, because very accurately tuned circuits are being dealt with, and the circuit is practically dead to all oscillations except those that are of that frequency. I suppose it is impossible to use it on an underground line. From what Mr. Harrison said, I understood that the speech between New York and San Francisco is being carried out partly in that way. That is very remarkable indeed because, when very long distances are being dealt with and very high-frequency currents are being used, one would anticipate that the charging currents into the line would be so large as to make this system almost impracticable. There must have been a large number of repetitions on the line.

Mr. W. Holttum: I think everybody will agree that this paper has come at a very opportune time, when our system of telephone transmission is being revolutionized by the introduction of the valve repeater. This improvement considerably complicates the problem of designing the most economical transmission line, and I should like to ask whether the Post Office have arrived at a solution of what is the most economical combination of size of conductor, weight of loading, and spacing of repeaters, in order to give the cheapest possible transmission line and, if the information may be divulged, what that combination is. That, of course, will be an outstanding problem in connection with the new system, and, moreover, the solution will vary somewhat with the market prices of the different components. I should also like to know whether the same combination will hold for any distance of transmission; I should imagine that would be so. There are transmission lines in America, I think, with

as many as 22 repeating points; can the author say whether there is any noticeable distortion in such a case, and whether there is any practical limit to the number of repeaters which can be put in a line, for this reason or any other? Also, would a loaded and "repeatered" line of a given length give speech equal to or better than a heavier line of equal length and attenuation, which is loaded only and not repeatered? With regard to "wired wireless," I should like to ask whether it is really regarded as impracticable to use it on underground cables, that is, of course, not on our existing loaded cables, but on special cables for the purpose. The tables of constants for the various loaded lines in this country given in the Appendixes are very interesting. The figures for leakance in Appendixes III and V seem, however, to call for comment. In Appendix III the ratio G/C is given as 12, 15, and 20, and in Appendix V it works out to about 6. As 20 is a very good value for a cable, I should like to suggest that an explanatory footnote should be supplied referring to these values, as otherwise one might be inclined to question them. It is noticeable that in Appendix V the insulation is given as 120 megohms. I do not know whether this low insulation would increase the alternating-current leakance to any extent, but at any rate it is remarkable that low insulation and low leakance should go hand in hand.

Mr. F. Mercer: There is one point which I should like to mention in regard to the plan of the underground telephone cable system of Great Britain given on page 399 of the paper. The tendency at the present time is to lay cables containing a very large number of circuits, a standard example being the 160-pair, 40-lb. conductor cables having an equivalent of 240 circuits. A breakdown on such a cable would involve serious consequences unless the traffic which is normally carried by it could be diverted in some way. I think I am right in saying that some of the cables contain many times the number of circuits between centres that were originally provided by overhead lines. When, therefore, all circuits in the cable are being used to their utmost capacity the overhead lines, in the event of a complete breakdown of the cable, could not cope with the situation, and the only remedy would be to provide one or two extra cable channels. For this reason it would appear doubtful whether cables containing very large numbers of circuits should be used except in cases where alternative cable routes are provided, and instead two small cables, each containing a smaller number of circuits, should be put down, although the actual cost per circuit would obviously be considerably greater. It does not seem likely, as has been suggested by other speakers, that loading will be superseded by repeaters —or at least not for some time. The matter is one of economics and must depend on the three factors, weight of conductor, amount of loading, and number of repeater stations, until the first factor, i.e. weight of conductor, becomes so small that for mechanical reasons it would not be advisable to go further. At this stage the elimination of loading will tend to become an economical proposition, but I think we have not by any means reached it yet. The paper contains an interesting account of the history of balancing cables. This balancing forms quite an appreciable factor in the expense of laying a trunk telephone cable, and I think a great deal of work can yet be done in simplifying and cheapening the process. Although progress has been made in factory processes towards making cable with good inherent balance, and systems have been devised for simplifying and reducing the work of balancing, it still remains a complicated process. Now it has been found from experience that it is possible to effect a partial balance between sections of cable with loading coils connected, and that this balance is maintained for the speech-frequency range. That being so, it seems fairly evident that balancing could be effected on long sections of cable in which several loading-coil cases are connected, and might conceivably be done by interconnecting the circuits of a four-wire core with condensers, inductances, and resistances of suitable values. This seems to offer a possible solution whereby the present necessity of balancing loading-coil sections of cable would be eliminated, and it would have the added advantage that replacements of cable could be effected without the necessity of rebalancing the leading-coil section concerned. It is rather disappointing to note that it is contemplated that four-wire repeaters will be largely used, because the number of available circuits on four-wire cores in which they are connected will be reduced to one-third, assuming, as seems probable, that phantoming is impossible under these conditions. There is one point in Appendix I which is rather puzzling, viz. why the loading on the phantom circuits is such that the attenuation on those circuits is less than on the physical circuits. I think the table given would have gained materially in value if figures had been included for cross-talk on the completed cables. It does not appear as though Post Office engineers are calculating on the general adoption of high-frequency telephony, at least for some time, because there is no doubt that its introduction would have a profound effect on telephone transmission schemes. A few points in this connection are: (1) The fact that loading, unless in continuous form, would be uneconomical owing to the large number of loading points which would be required; (2) The number of repeater stations must be largely increased to make up for the lack of loading and extra losses entailed with high-frequency currents; (3) The tendency to interference between neighbouring circuits would be much greater than with ordinary speech currents.

Mr. A. J. Pratt: The author has placed before us some of the difficulties experienced by engineers in dealing with the expansion of the long-distance telephone system of this country. The evolution of the telephone repeater and its application in overcoming these difficulties has proved the wisdom of proceeding along cautious lines. The laying of new underground plant over long distances is a costly operation of considerable magnitude. The use of repeaters working on conductors weighing not more than 40 lb. (or even less) per mile will reduce the number of long-distance cables required during a given period. There is, however, one aspect of the case which deserves attention. The fitting of loading coils on main cables involves the construction

of large underground chambers to accommodate the loading-coil pots. These chambers or manholes are necessarily of ample dimensions. The iron pots themselves, containing the coils, average between 25 and 50 inches in height, excluding the length of the cable stubs, with a maximum diameter of 2 ft. These pots provide telephone circuits up to a total of 50. As the underground telephone system expands it will become a matter of increasing difficulty to secure suitable sites for manholes, especially in large towns, and in view of the short spacing (1·6 miles) between the coils for 40-lb. conductors (see Appendix I)—the spacing of the new London–Bristol loaded cable is only 1·125 miles. There is an interesting reference on page 402 in the section dealing with submarine cables, to the development of the use of continuously loaded cables. It is true that, according to the initial experiments on these cables, the results showed that the electrostatic capacity of the iron-wound loops was roughly twice as great as that of the untreated loops and that there was an increase in the resistance with frequency,* both serious items. Mr. Martin, however, prophesied improvements in the construction of continuously loaded cables. Moreover, continuously loaded cables provide for distributed and not lumped inductance and, according to the statement on page 397 of the present paper, inductance uniformly distributed throughout the length of the circuit is part of the necessary condition for satisfactory transmission. Bearing in mind all these points and also the one regarding manhole construction previously referred to, it would be interesting if the author would give us some information as to the possible application of continuously loaded cables to land lines, especially in view of the recent improvements and progress made in the manufacture of these cables. On page 393 the modern method of cable-balancing is mentioned. As is now fairly well known, this system was introduced to overcome the capacity unbalances existing between the pairs of wires in the four-wire cores of multiple-twin cables. Careful manufacture of cable is effective for dealing with resistance and inductance unbalances ; the quality and thickness of the paper insulation, and the volume of air space ensure freedom from cross-talk on unloaded cables, but loaded circuits are more sensitive to disturbance and it is necessary, in order to ensure satisfactory phantom working, for the electrostatic capacity unbalances between adjacent pairs to be neutralized by the crossing of selected wires at joints. The Leeds–Hull cable was balanced by what is known as the single-bridge system, in which wire-to-wire and wire-to-earth capacities are dealt with as combined quantities. The more modern method provides for the separation of the wire-to-wire and wire-to-earth capacity unbalances, and reducing them, as separate quantities, to zero. This system was followed on the Liverpool–Birmingham cable in which there are now 25 phantom circuits working. Most of the balancing work is done on the road, but it would be interesting to learn what actual or real progress has been made by cable manufacturers in effectually balancing lengths of cables in their works during manufacture and prior to delivery on the ground. Perhaps it can also be stated if there are any phantom circuits working satisfactorily at the present time on unbalanced but loaded cables. I should like in conclusion to refer to the work of the late Mr. Charles E. Hay. He was a Liverpool man who went to London on research work. He had a keen mathematical mind, and I believe I am right in saying that, apart from the pioneer workers (Mr. Tremain and the late Mr. A. W. Martin), he was mainly responsible for placing the present system of cable-balancing on a perfectly sound mathematical basis.

* A. W. Martin : "The Loading of Telephone Cable Circuits," *Institution of Post Office Electrical Engineers, Professional Papers*, 1909, No. 30.

Sir W. Noble (*in reply*) : Several contributors to the discussion have called attention to the omission from the paper of various details in the development of the thermionic valve, the telephone repeater, and the telephone cable. It was not possible within the intended scope of the paper, namely, a survey of the development of long-distance telephone service since 1910, to deal fully with any one of those matters. In fact, both the telephone repeater and the telephone cable may well be the subject of a separate paper, which I hope may be forthcoming at a future date. I have explained in the paper the reason for not dealing fully with the thermionic valve.

I should like to thank Messrs. F. Gill, R. L. Jones, P. E. Erikson and H. F. Kortheuer for the valuable information which they have contributed on the long-distance telephone service of the United States.

In reply to Professor Fleming, the Post Office have investigated the properties of oxide-coated filaments—in fact, the first thermionic valves used for telephone repeaters were of this type—and the subject is still under investigation. The helium-filled valve has not been used for telephone repeater work up to the present time.

I am in full agreement with Mr. Kingsbury on the advantages to the public of a "no delay" service. The extent to which such service can be given in the long-distance system can be determined only by experience. Mr. J. D. Taylor has contributed some interesting information on this point in connection with the Glasgow–Edinburgh service.

Mr. Tremain and other speakers have asked for further information on the balancing of telephone cables. The importance of improving factory methods so as to produce cables with a minimum degree of unbalance is fully appreciated by manufacturers in this country, and there has been a marked advance in this respect in some recent deliveries. I fear that Mr. Tremain's suggestion for a tighter cable is not a solution of the problem. Drawing-in strains do not appreciably affect the balance of cables. The need for replacement of faulty lengths of a balanced cable has arisen in very few cases and, where it has been necessary, the use of interruption cable has prevented any serious stoppage of service. The 1910 Anglo-French coil-loaded submarine cable was not balanced for phantom working. I have no information on the insulation between the wires of a twinned enamelled pair.

The direct-current insulation of the cable referred to in Appendix V was taken with the cable terminated in silk and cotton tails on the usual distribution frame and protector terminals; this accounts for its comparatively low value and the apparent value of *G/C*.

In reply to Mr. Shaughnessy, although exact figures cannot be given it may be stated that the capital cost per circuit in a long-distance cable is considerably less than that of an equivalent overhead circuit; and in regard to annual charges, the maintenance of an underground long-distance circuit is almost negligible (probably not more than 3 per cent) in comparison with that of an overhead circuit.

Mr. Jacob's suggestion for a differentiation between the terms "cross-talk" and "overhearing" is a good one and should be considered.

In reply to Mr. Elliott, the Post Office have experienced no difficulty in regard to the "rigidity" of British thermionic valves for telephone repeater work, nor have they any reason to believe that German valves are superior in this respect. The standard of insulation resistance for long-distance telephone cables is 10 000 megohms per mile, and this standard is maintained in practice without difficulty.

In reply to Mr. Hart, the 28-core submarine cable described in Appendix III was designed for estuary work, and cables of this type will be laid across the Severn, the Humber, and Spithead.

In reply to Mr. Wallace, the repeater station shown in Fig. 15 was designed for an initial capacity of 500 and an ultimate capacity of 1 000 circuits.

In reply to Mr. White, the London–Manchester cable was designed for two-wire circuit working. The four-wire repeater is applicable to a circuit of any length, provided the conditions are suitable. An automatic alarm can be provided, where required, to indicate the failure of a thermionic valve.

Mr. Crane's assumption as to the use of the engines and dynamos is not quite correct. The filament and plate batteries will both be charged by motor-generators, and the latter will be driven by direct-coupled engine-sets, or from the public power mains where a supply is available. It is not practicable, nor would it be economical, to provide automatic arrangements for bringing all repeaters into circuit only when required by the terminal operators; but it may be found convenient in some cases to equip a limited number of circuits with such an arrangement for night service.

In reply to Mr. Hill, there is no reason for anticipating greater interference from power circuits with the new balanced and "repeatered" cables than with the earlier unbalanced cables on which repeaters were not used. In this connection, however, I might mention that the possibilities of damage by electrolysis present a much more serious problem, one which has caused the Post Office some anxiety. The balanced cables are, of course, in no way more immune from damage of this nature than the unbalanced cables.

I share with Professor Marchant his surprise that the mechanical rotary amplifier is mentioned by Mr. Harrison as a possible competitor with the thermionic valve.

Mr. Holttum asked for the most economical combination of conductor weight, loading and repeaters; it is not practicable to give a general answer, since each case must be dealt with on its merits. It is not impossible to design an underground cable for carrier-current high-frequency transmission, but I doubt whether such a cable would be an economical proposition.

Mr. Mercer was puzzled by the loading of the phantom circuits shown in Appendix I. The better transmission equivalent of the phantom circuit as compared with its two associated physical circuits is, of course, primarily due to its lower resistance. The amount of inductance that may be added to the phantom circuit is determined by the requirements in regard to the frequency cut-off point, which should be approximately the same for both physical and phantom circuits.

Mr. Pratt's inquiry as to the possible application of continuously loaded cables to land lines opens a very wide field, and it was impossible to deal with the subject fully in the paper. It may, however, be stated that at present the choice between coil-loaded and continuously loaded land cables is a simple question of economics, the answer being always in favour of the former. The factors which militate against coil-loaded submarine cables are not met with in land lines.

THE EFFECT OF ELECTRON EMISSION ON THE TEMPERATURE OF THE FILAMENT AND ANODE OF A THERMIONIC VALVE.

By G. Stead, M.A.

(Communicated by Professor Sir J. J. Thomson.)

(*Paper received 5 January, and read before the* Wireless Section of the Institution 16 *March*, 1921.)

Summary.

The temperature at any point on a tungsten filament which is emitting electrons is altered by the passage of the emission current through the filament and also on account of the latent heat of evaporation of the electrons.

An account is given of some direct measurements, made with an optical pyrometer, of the distribution of temperature along an emitting filament under various conditions.

It is found that when the negative end of the anode battery is connected to the negative end of the filament battery the distribution of temperature along the filament becomes increasingly unsymmetrical as the emission current increases, the negative limb being hotter than the positive limb at a given distance from the lead.

In the particular filament employed in these experiments it is estimated that the total electron current from the filament is increased by 35 per cent when the emission amounts to 11 per cent of the current from the filament battery.

An account is also given of some measurements of the temperature of an anode which is undergoing electron bombardment, and a curve is reproduced showing the relation between the anode temperature and the number of watts dissipated by the anode per square centimetre of surface.

Preliminary Note.

The experiments described in the present paper were carried out in the Cavendish Laboratory in May 1918, and were undertaken on behalf of the Wireless Telegraphy Department of H.M. Signal School, Portsmouth. The work is now published with the consent of the Admiralty.

(I) Introduction.

In a previous paper * I have discussed the question of the distribution of temperature along a tungsten filament in the neighbourhood of a lead. The temperature-distribution curves given there, however, apply only to filaments from which no electron emission is taking place. In a thermionic valve, under working conditions, the filament is, of course, constantly emitting a stream of electrons, and this affects the temperature of the filament for two reasons :—

(*a*) It has been shown by Richardson † that there is a latent heat of evaporation for electrons, i.e. energy must be supplied in order to liberate electrons from the surface of the filament. The power required for this is about 5 watts per ampere of emission. Thus the temperature of the filament will be lowered to a greater or less extent at each point which is emitting electrons.

(*b*) It is customary to connect the high-potential battery between the anode of a valve and the *negative* end of the filament. In this case, as soon as the high potential is applied a current, a little smaller than the emission current, must traverse the negative limb of the filament in the same direction as the filament current, and the filament is heated by this as well as by the current supplied by the filament battery. A small fraction of the emission current will also give rise to a current traversing the positive limb in the opposite direction to the current supplied by the filament battery, so that the positive limb will tend to cool, but the cooling will be much smaller than the heating of the negative limb. Thus the distribution of temperature along the filament will be unsymmetrical, the temperature rising to its maximum value more rapidly in the negative limb than in the positive one.

It is clear that the effects (*a*) and (*b*) act in opposite directions, and also that they will be most marked when the emission current becomes comparable with the filament current, i.e. in the case of a very hot filament of small diameter. The theoretical treatment of the question appears to present great difficulty, but the magnitude and direction of the resultant effect can be determined by experiment.

(II) Effect of Electron Emission on Filament Current.

Any change which occurs in the average temperature of the filament will give rise to a corresponding change in filament resistance, and hence the filament current produced by a given filament voltage will also alter. The following table shows the effect of emission in reducing the filament current, the ammeter which measured the filament current being placed between the battery and the positive end of the filament. The filament currents shown in column 4 were produced by the same filament voltage as the corresponding currents in column 3.

It will be seen that when the electron emission takes place the filament current is reduced, and hence the filament resistance appears to be increased. If so, effect (*b*) is larger than effect (*a*). It must be noted, however, that some of the reduction in current must be due to that portion of the emission current which traverses the positive limb of the filament. In a thermionic valve under ordinary working conditions the average anode current is usually about half the total emission, so that the effect is less than that shown in

* *Journal I.E.E.*, 1920, vol. 58, p. 107.
† "Emission of Electricity from Hot Bodies," p. 164.

the following table. It is plain that where the emission current is small, as in a receiving valve (e.g. 5 mA), the effect is inappreciable.

Filament Length	Filament Diameter	Filament Current (no Emission)	Filament Current (Emission)	Emission Current
cm.	cm.	amp.	amp.	mA
12	0·0195	4·38	4·20	480
12	0·0195	4·30	4·15	400
2·2	0·0058	0·825	0·805	80
2·2	0·0058	0·762	0·752	40

(III) Effect of Emission Current on Distribution of Temperature along a Filament.

In order to investigate this effect a special two-electrode valve was made consisting of a straight filament, about 3·5 cm. in length and 0·0088 cm. in diameter, surrounded by a cylindrical anode, which was provided with a narrow longitudinal slit through which could be seen any desired part of the filament. A Cambridge optical pyrometer was then mounted upon a cathetometer, and the valve was so placed that any portion of the filament could be sighted through the instrument. In this way the temperature distribution along the filament could be observed for any desired values of the filament current and the emission current. Corrections were made for absorption of light by the bulb and for the emissive power of tungsten, exactly as described in the previous paper.*

Before carrying out the pyrometer observations the valve was highly exhausted by means of a Gaede pump, followed by charcoal and liquid air, and was subjected to the usual baking process to remove gases from the glass, whilst the anode was raised to a bright red heat by electron bombardment to get rid of occluded gases. The bombardment of an anode tends to blacken the bulb, and a blackened bulb, of course, would render the pyrometer readings inaccurate. In order to avoid this the electrodes were surrounded by a loose inner glass tube during the bombardment, and this tube could afterwards be slipped aside by inverting the bulb. The actual pyrometer readings were taken as quickly as possible in order to minimize the error due to the blackening of the bulb during the experiment.

The filament was made to form one arm of a Wheatstone bridge, so that its resistance could be measured when there was no electron emission, and again when emission was occurring. The resistance was found to increase when emission took place, thus indicating a rise of average filament temperature. This increase in resistance is naturally accompanied by a drop in filament current, but in the experiments here described this was compensated for by a readjustment of the rheostat which controlled the filament current. It is probably on this account that no cooling of the positive limb was observed. The distribution of temperature along the filament was then determined by means of the optical pyrometer for various values of the emission current. The results obtained are shown in Fig. 1, in which the increasing asymmetry of the temperature distribution, as the electron emission current increases, is clearly seen. The curve marked $i = 0$ is the symmetrical curve obtained when no emission is taking place. The curve marked $i = 160$ mA corresponds to

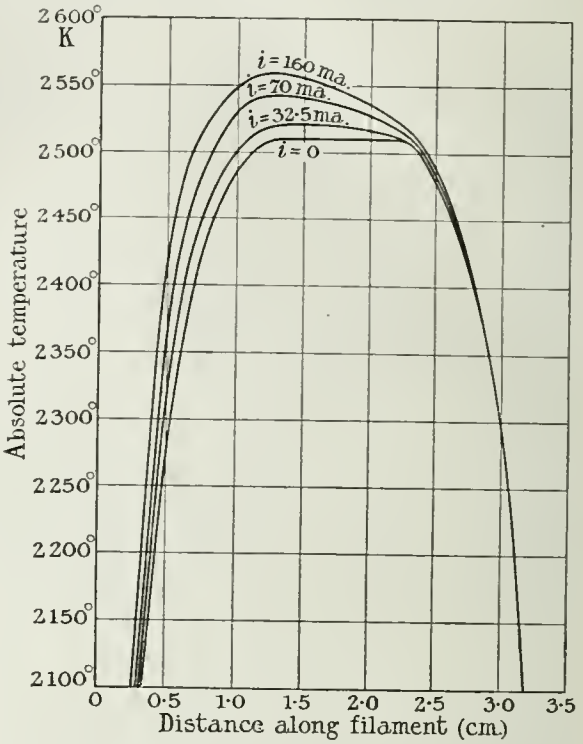

Fig. 1.—Distribution of temperature along an emitting filament.

approximate saturation, whilst the two remaining curves were obtained with emission currents smaller than the saturation value. The increase in maximum filament temperature appears to be roughly proportional to the emission current.

It is a matter of some interest to know how much the saturation current from a given filament is increased by the change in filament temperature due to the emission current itself. An estimate of the magnitude of this effect for the filament here considered can be obtained from the curves in Fig. 1 by making use of the standard emission curves given in the paper by the present author already referred to. All that is required is to calculate the total emission corresponding to the curve $i = 0$, and compare it with the emission corresponding to the uppermost curve of Fig. 1. From this calculation it appears that the emission is increased by 35 per cent when the emission current amounts to about 11 per cent of the filament current.

(IV) Dissipation of Energy by the Anode.

The power represented by the product of the anode current and the anode voltage has to be dissipated by the anode itself, and thus the anode temperature

* *Loc. cit.*

must rise until the rate of radiation is equal to the power supplied. Where the power is large, as in a transmitting valve, the temperature of the anode will be high, unless the anode has a very large surface area. The upper limit of temperature which is possible is, of course, the melting point of the metal. The metal

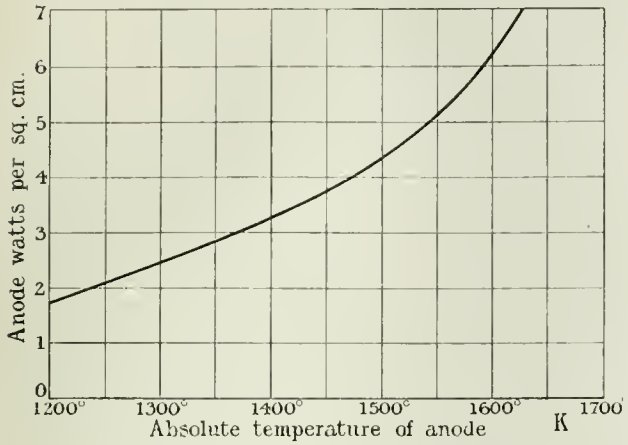

Fig. 2.—Dissipation of energy by anode.

most commonly employed is nickel, the melting point of which is about 1 720° K. It is important to know the number of watts per square centimetre which can be dissipated by a nickel anode, without raising it to a temperature which is dangerously near to the melting point. Information on this point was obtained in the experiments on the temperature of emitting filaments described above. For the larger emission currents employed the anode was raised to a red heat, and pyrometer observations were made to determine its temperature. The anode current and voltage were measured at the same time, and the area of the anode was known. It was therefore possible to plot a curve to show the relation between the temperature and the watts dissipated per square centimetre of anode surface. This curve is shown in Fig. 2.

It is very difficult to obtain accurate observations of the anode temperature near the melting point of nickel, owing to the rapid blackening of the bulb at such temperatures. Thus the temperatures recorded in Fig. 2 are almost certainly too low. It is also necessary to point out that a nickel anode does not radiate as a perfectly black body, so that the pyrometer temperatures require to be corrected. In order to make this correction a knowledge of the emissive power of nickel is required, and, since no information on this point was available, a rough correction was made by assuming that the emissive power of nickel for the wave-lengths transmitted by the optical pyrometer is the same as that of tungsten, for which observations by Worthing * are available. Thus the temperatures recorded in Fig. 2 are only very roughly true temperatures on the Kelvin scale. Nevertheless, for purposes of valve design, the results are useful as a guide to the power which a nickel anode of given size is capable of safely dissipating.

In conclusion I desire to thank Professor Sir J. J. Thomson for his kindness in allowing these experiments to be carried out in the Cavendish Laboratory.

* *Physical Review*, 1917, ser. 2, vol. 10, p. 377.

Discussion.

Professor C. L. Fortescue: Although the fact that the act of switching-on the anode potential changes the temperature of the filament has been known for some time, this is, so far as I am aware, the only occasion on which accurate measurements have been made. I think it is impossible but to admire the accuracy with which the author has obtained these results. In Fig. 1 the temperatures are measured by pyrometrical methods, and a large number of readings are obtained which give good curves over a range between 2 500 and 2 550° K. It is, however, a great pity that the author did not show us the actual dots or crosses from which those curves are plotted, as I believe that they are very consistent. The distribution as shown in Fig. 1 is a matter of very considerable practical importance. It is a question, for instance, which has to be considered very carefully in taking the characteristics of a large transmitting valve. Usually the filament-current ammeter is put in the positive filament lead, but as a general rule corrections for the distribution of the current along the filament are not made in taking the characteristics. I have taken a number of characteristics, as a matter of fact, in which I have introduced such a correction, worked out on the assumption that the electron emission is uniform over the length of the filament. If that is done, the characteristics are quite noticeably different from those which are obtained if the current in the positive filament lead is simply kept at a constant value. Perhaps the author will state what should be the correct value for the current when the characteristic curves are being taken. Then, again, it is a matter of practical importance to know the peak value of the temperature. When the filament of a valve fails, the break often occurs towards the negative end. This may be, of course, partly due to the fact that the bombardment by positive ions (if these are present) is heavier at the negative end; but I think it may also be due to the fact that the temperature there is appreciably higher, at any rate in all valves giving a large emission current. The curves in Fig. 1 enable us to form some estimate of what these differences of temperature are likely to be, and for that reason they are of considerable value in designing filaments for transmitting valves. With regard to Fig. 2, which shows the dissipation of energy from the nickel anode, I am not quite clear whether the filament watts have been allowed for or not. I do not know what the filament currents were, but I should think that the power in the filament would very likely be of the order of 10 or 15 watts.

Professor G. W. O. Howe: I am very pleased to support Professor Fortescue's expression of the importance of the work which lies behind this paper. Everyone who has thought about the careful plotting of character-

istics of valves has, I am sure, felt worried about the filament current. One is always told to keep the filament current constant, because it is known that the characteristics depend very much on this being done; but the doubt always arises as to exactly which current has to be kept constant. I have made various experiments; I have tried putting an ammeter both in the positive and in the negative side, and have tried to keep the average of the two constant, and I have tried bringing in the anode tapping in the middle of the filament battery, and not on one side, without, however, coming to any very definite conclusion as to what should really be kept constant. But there is no doubt that in giving characteristics of valves to a high degree of accuracy it should be definitely stated what is kept constant, and where the filament ammeter is placed. This paper reminds me of a suggestion which I made some months ago in the discussion on Professor Fortescue's paper on amplifiers. It was stated that the efficiency obtained from amplifiers did not agree with that predicted from the static characteristics, and I suggested that the dynamic characteristics of a thermionic valve might differ from the static characteristic, because during the high-frequency cycle the temperature of the filament could not change on account of the thermal capacity, whereas, when the static cycle is being plotted, the temperature of the filament changes for every new point on the characteristic curve. Thus, during the high-frequency oscillation the filament will maintain approximately a mean value. This paper tends rather to strengthen one's belief in that explanation. I am somewhat worried about the table on page 428. The author refers to the average temperature of the filament, but I find it rather difficult to form any accurate opinion, or any opinion at all, from these figures as to the change of temperature. The difficulty lies in determining to what extent the decrease of current in the first case from 4·388 to 4·2 amperes is due to the mere anode current passing in the reverse direction through the ammeter, because it will be noticed there that the emission current is very large; it is 0·48 ampere; that is to say, there is an emission current of 0·48 ampere which has to be superimposed partly in the left-hand and partly in the right-hand limb of the valve, and the total reduction noticed in the right-hand side is only 0·18 ampere. I do not see how any opinion can be formed from these figures as to there being any change in the resistance of the filament, and I should be glad if the author would throw a little more light on that point.

Mr. L. B. Turner: The author has pointed out that two opposing temperature effects occur when the electron current between filament and anode is set up by switching on the high-tension (anode) supply. First, the increase of current flowing in the filament near the negative end is accompanied by an increased production of heat in that portion; and secondly, the emission of electrons from the filament involves a disappearance of heat corresponding to the latent heat of evaporation. Commonly, the first effect is greater than the second, and the temperature rises. I should like to know whether it is not at least conceivable that the first effect might be less than the second, the anode current then producing everywhere in the filament a fall of temperature. The phenomenon might be expected in cases where the filament is unusually dull or has an unusually high latent heat.

Dr. J. H. Vincent: Will the author give us his advice on the following matters? What does he consider to be the best procedure to adopt in coupling up a normal oscillating circuit so as to stand the best chance of keeping the filament current steady (I should like him to put upon that word "steady" the interpretation that he considers best, because it is not at all clear to me what we are really to aim at in this matter). I imagine that we want to get the most uniform distribution of temperature along the filament. If that is so, it appears that the normal procedure in the past has been wrong, but I am not at all sure about that, and for this reason I should like further information on the point. As regards the last section of the paper, it seems to me that in this case, too, we ought to have a double effect due to the electrons which leave the filament getting caught by the anode. Then I presume they will warm the anode both through bombardment and also through absorption. I imagine that there ought to be some way of distinguishing or separating these two effects, as it has proved possible to do in the case of the filament.

Mr. R. C. Clinker: I should like to mention two points in connection with the paper. I presume the conditions are quite different when alternating current is used to heat the filament from those which prevail with direct current. Certainly that will give a more equal distribution of temperature, because both ends will be negative in turn, so that the heat distribution will be much more uniform. I should also like to ask the author whether, in Fig. 2, the anode watts per square centimetre refer to the external surface only or to the internal surface as well. I understand that a cylinder was used, and not a plate.

Mr. J. W. Ryde: The author in his previous paper on the tungsten filament mentions that there was a discrepancy between his scale of temperatures and that given by Langmuir. This also applies to the scale in Fig. 1 of the present paper. If we compare the temperatures on the two scales for the same value of watts per centimetre, it will be found that the difference is of the order of 100 degrees, the author's figures being the lower. Some experiments carried out by Mr. Gossling and myself at the Research Laboratories of the General Electric Company, London, throw some light on this discrepancy. It is well known that the image of a small bright object appears broadened on the retina due to irradiation, and its apparent intrinsic brilliance is reduced in consequence. From this it follows that, if we are determining the temperature of a fine filament by matching it against a broad sector, as in the Cambridge optical pyrometer used by the author, its temperature will appear too low. In order to determine the magnitude of the effect, a carbon strip about 3 mm. broad was mounted in an exhausted bulb so that it could be heated electrically. This strip was placed behind the filament being experimented upon, in such a way that, on looking through the pyrometer, the strip could be seen with the filament crossing it, and also so that either

the strip or the filament could be independently matched against the sector. The filament was first glowed and its apparent temperature read from the pyrometer, then the carbon strip acting as background was heated up until the filament disappeared. The temperature of the strip was then determined by matching it independently against the sector in the pyrometer. In this way, two values for the temperature of the filament were obtained, the former being affected by irradiation, and the latter being free from it. After making the necessary corrections for glass absorption and the emissivity of tungsten, it was found that up to a temperature of 1 500° K. the effect of irradiation was to reduce the temperature readings by 60 degrees. At temperatures up to 2 300° K. the difference amounted to about 100 degrees. As the diameter of the filament employed was the same as that used by the author, this effect satisfactorily accounts for the discrepancy between the two temperature scales. The pyrometer employed by Langmuir would be relatively free from the effect.

Mr. J. Scott-Taggart: With reference to the portion of the paper where the author discusses the effect of the anode current adding itself to or subtracting itself from the filament current, an interesting experiment consists in putting one ammeter in the negative side and another in the positive. A key is in the anode circuit. With the key up, that is to say, with the anode current not flowing, both ammeters register the same current. On depressing the key, however, the two change; one increases in value due to the anode current, while the other decreases. This experiment is a method of showing the effect which is comparable with the experiment that we have seen to-night. When the anode current is actually flowing, perhaps the best method of measuring the actual filament current would be to take the mean reading of the two ammeters. This effect of heating the filament more strongly at the negative end has, of course, a very important bearing when dealing with transmitting valves, and several methods have been suggested of overcoming it. Mr. W. C. White, of the General Electric Company, has suggested the use of alternating current, and making a connection to a tapping half way along the transformer winding. Captain Round has also suggested the use of a tubular cathode heated by means of hot wires passing through the centre of it, and others have also suggested passing a filament through a cylinder or other form of cathode, and allowing a bombarding current to heat up the cathode. It then takes the place of the ordinary filament. Both these arrangements overcome the trouble of the heating up of one end of the filament. There is one point in connection with the heating of the filament, and that is that most of the electronic current comes from the negative end, because the anode has a higher potential to the negative end than it has to the positive end. Consequently we would expect a greater withdrawal of electrons from that end, and a consequent tendency to cool which would lessen the tendency to heat up. There is another question which I should like to ask, and that is as to the effect of applying the anode voltage: What effect has that on the actual electronic emission due to the adding of the electronic current to the filament current? Obviously, one half of the filament is heated, and the other is cooled. Is the resultant effect the establishment of an unchanged electronic current from the filament? A practical point of value of the horseshoe filament in transmitting valves is that the electronic emission is fairly uniform along its whole length, because the negative end is hotter and the positive end is cooler, and these two ends come together at one end, while the middle of the filament is at the other, so that the electronic emission from the whole length of the filament is comparatively uniform.

Dr. W. H. Eccles: I might mention one thing which has occurred to me and is of interest to physicists. If we look at the curve marked $i = 0$ in Fig. 1 we find that it is nearly symmetrical. Now one of the standard methods of measuring the Thomson effect of a metal, that is to say, the specific heat of electricity, is to take a uniform wire of the metal, heat it by a current, and observe the lack of symmetry of temperature along the wire. We have to accept this inner curve of Fig. 1 as showing that the asymmetry caused by the specific heat of electricity in tungsten is of no account as compared with the effects of the latent heat of the electricity leaving the metal.

Mr. E. V. Appleton (*communicated*): The paper deals with a problem of direct interest to those concerned with the physics of the thermionic triode. In practice, radio-telegraphic workers deal with filaments the electronic emission of which is actually collected, and thus, if the emission varies with the value of the anode current, they deal with filaments of varying emission. Dr. van der Pol and myself have recently had to consider the same problem in connection with the study of some anode-current oscillograms of a low-frequency triode generator and, as the conclusions reached are different from those in the paper, it may be of interest to consider the points of divergence. The chief interest of the present experiments to radio-telegraphic workers naturally lies in the answer given to the question: Does an increase in the anode current of a typical triode (e.g. R type) produce a greater or reduced filament emission? In the case of a filament 3·5 cm. long and 0·0088 cm. in diameter the answer given in the paper is that the emission is markedly increased by closing the anode circuit. The conclusion reached by Dr. van der Pol and myself is that exactly the reverse happens and that the filament as a whole is cooled, resulting in a reduced emission. Some experiments carried out by Dr. Owen * and Mr. Archer seem, in my opinion, to support our result, as they found that the anode current fell in value after closing the anode circuit. Whether the filament is cooled or heated depends, of course, on the size of the filament and on the filament voltage; but as the filament mentioned in the present paper is very similar to that of an ordinary R-type triode the marked difference between the results is curious. It may be that the author would have observed a reduction in emission if he had not made the readjustment of filament current each time after closing the anode circuit. It may be noted that no such alteration of filament current is made in actual practice, e.g. in plotting static characteristics. If this

* *Proceedings of the Physical Society*, 1921, vol. 33, p. 104.

were the case, the general statement in the summary regarding the effect of anode current on filament emission would have to be further qualified. In section II the deductions made from the figures in the table seem to be hardly permissible. Because the reading of an ammeter placed in the positive limb of a filament falls on closing the anode circuit it cannot be said that the filament resistance is increased. If the ammeter had been placed in the negative limb, an increase in the reading would have been found, and thus, arguing in the same way, it would have to be assumed that the filament resistance is reduced and that effect (*a*) is greater than effect (*b*) [see section I]. The truth seems to be that no such simple experiments as these can give us any information as to the alteration of filament resistance. Much depends on the value of the resistance of the filament rheostat as compared with that of the filament; but the decrease in filament current shown by columns 3 and 4 might be *wholly* due to emission current, and not to alteration in filament resistance. Some experiments which I have recently carried out show that with a filament 3·5 cm. long the reduction in the reading of the ammeter on the positive limb is due wholly to the emission current; also, the filament resistance is actually reduced. Confirmation of this was obtained from a separate oscillograph experiment, in which evidence for reduced emission was again found.

Mr. G. Stead (*in reply*): The curve shown in Fig. 2 gives the anode watts dissipated per square centimetre of anode surface, and no allowance has been made for filament watts. In order to obtain total watts per square centimetre it is necessary to increase the ordinates of the curve by 1·33 watts at 1 200° K. and 1·56 watts at 1 620° K. The anode watts per square centimetre are calculated on the assumption that the radiation takes place only from the external surface of the anode.

Professor Fortescue and Professor Howe desire to know what should be done with the filament current when taking characteristics. Static characteristics are drawn in order that they may be used to predict the probable behaviour of a valve in a given circuit. Now when a valve is generating oscillations nothing remains constant in the filament circuit except the voltage of the filament battery and the resistance of the rheostat, if one is used. It seems to me, therefore, that the same quantities ought to be kept constant when taking characteristics. Moreover, they ought to have the same value in both cases, for it is clear that when a 4-volt battery is applied direct to the filament the conditions are not the same as when a 10-volt battery is applied through a suitable resistance. In the former case any change in filament resistance due to electron emission would produce a maximum effect on the current supplied by the filament battery. I suppose that if a 4-volt filament were heated by a 200-volt supply any changes which occurred in filament resistance would be almost negligible compared with the resistance of the rheostat, and so the current supplied by the filament battery ought to remain sensibly constant. So far as I can see, instructions to keep the filament current constant are meaningless, unless by "filament current" we understand "current supplied by filament battery," for the actual current through an emitting filament must of necessity be different in different elements of the filament.

Dr. Vincent suggests that the normal procedure of connecting the negative end of the high-potential battery to the negative end of the filament is not a good one. From the point of view of filament life this is no doubt true, for in this way the temperature of the negative limb of the filament is increased to the maximum possible extent. If connection were made to the positive end the negative limb would heat up to the minimum extent, but the positive limb would cool to the maximum extent. It seems likely that if connection were made at a suitable point in the filament battery, the increase in emission due to the heating of the negative limb might be just compensated by the diminution in emission due to the cooling of the positive limb. Dr. Vincent's suggestion, that the warming up of the anode when subjected to electron bombardment must be due partly to the giving up of the latent heat of the absorbed electrons, has been shown by Richardson and Cooke to be correct (see "Emission of Electricity from Hot Bodies," p. 172).

I think there is no doubt that Mr. Turner is right in suggesting that electron emission may either raise or lower the average filament temperature according to conditions. I have seen a number of Mr. Appleton's oscillograms, and agree with him that they point to a cooling of the filament under the conditions of his experiments. I should like, however, to suggest one possible reason why pyrometer observations may give results differing from Mr. Appleton's conclusions. The emission from a filament is controlled by its *surface* temperature, and not by its *average* temperature. Now, of the two effects of electron emission, the cooling due to latent heat must initially be confined to the surface layer, whilst the heating due to the extra current in the negative limb will take place throughout the volume of the filament. It seems quite likely, therefore, that the immediate effect of switching-on the anode potential will be a sudden chilling of the filament surface, with a corresponding lowering of emission, even though the equilibrium conditions attained somewhat later correspond to an increased emission. If this is so, dynamical experiments such as Mr. Appleton's might indicate a fall of emission, although statical observations with a pyrometer would show that the emission ought to increase.

From the remarks made by Professor Howe and Mr. Appleton on the table given in the paper, I fear that I have not made my meaning quite clear. I wrote that the filament resistance "appears" to be increased, meaning thereby that a diminution in current when the electromotive force is constant looks like an increase in resistance, at first sight, and, if so, effect (*b*) is larger than effect (*a*). However, I pointed out at once in the next sentence that some of the observed effect must be due to a different cause. I included the table in the paper, first, because it gives an idea of the order of magnitude of the effect in the case of typical large and small valve filaments; and secondly, because, in point of fact, it represents the actual starting point of the work described in the paper, and is therefore historically interesting to me, at least.

SOME THERMIONIC TUBE CIRCUITS FOR RELAYING AND MEASURING.

By W. H. Eccles, D.Sc., F.R.S., Vice-President, and Miss W. A. Leyshon, B.Sc.

(*Paper received* 28 *February, and read before the* Wireless Section of the Institution 16 *March*, 1921.)

Summary.

The first part of this paper is concerned with a method of measuring the voltage factor of a triode, and with circuits arranged for voltage amplification in which the full value of the voltage factor can be utilized; the second part deals with some methods of detecting very light contact between two conductors; the third part describes triode circuits in which very small movements of a conductor may be made evident.

Voltage Factor.

The voltage factor of a triode may be looked at from two distinct points of view. The first may be termed the electro-geometrical standpoint; in this the change in anode voltage required to produce a given change in the electric field in the neighbourhood of the filament is compared with the change in grid voltage required to produce an equal effect on this field. It is clear that from this standpoint the relationship between these anode and grid voltage changes could be calculated from electrostatic theory and a knowledge of the dimensions of the different parts of the triode.

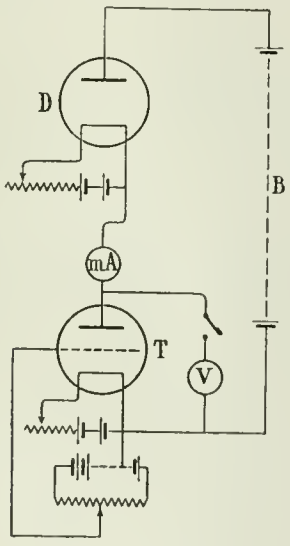

Fig. 1.

In the second or experimental way of defining voltage factor, the change in anode voltage required to hold the anode current constant when unit electromotive force is applied between grid and filament is the voltage factor at the given adjustment.

Measurement of Voltage Factor.

In the method now to be described the anode current is automatically kept constant by including a saturated diode in the anode circuit of the triode, and pairs of corresponding values of grid voltage (v_g) and anode voltage (v_a) may be determined for any given constant current through the triode.

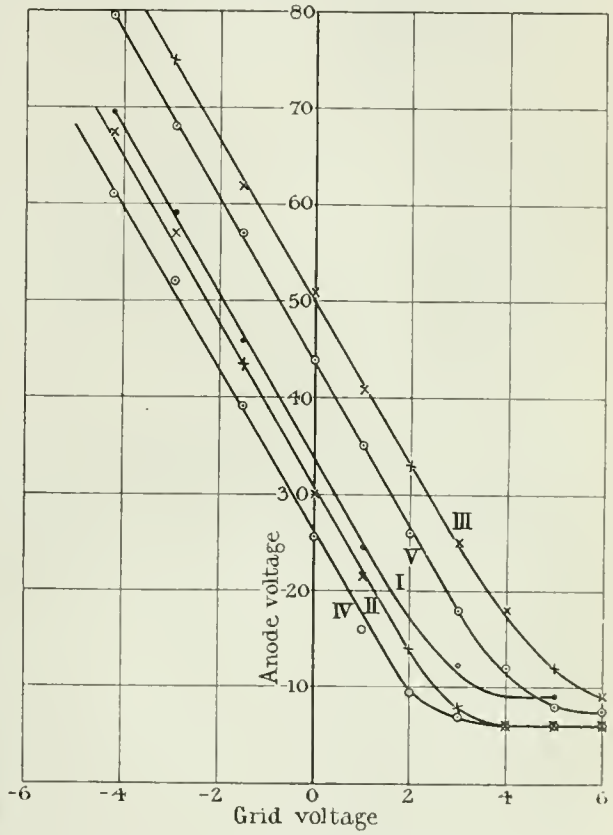

Fig. 2.

Curve I.—Filament voltage, 2·5; Anode current, 0·052 mA
II.—Filament voltage, 3·0; Anode current, 0·070 mA
III.—Filament voltage, 3·0; Anode current, 0·410 mA
IV.—Filament voltage, 4·0; Anode current, 0·070 mA
V.—Filament voltage, 4·0; Anode current, 0·410 mA

In Fig. 1, T represents a triode and D a diode in series with it, mA is a milliammeter, V an electrostatic voltmeter or quadrant electrometer. It is sometimes necessary to use a battery in series with V when this is an electrostatic voltmeter. The battery B must be of sufficient voltage to maintain the saturation current of the diode through the apparatus.

Suppose the relation between v_g and v_a is required

for a triode at filament voltage of 4 volts and an anode current of 0·4 milliampere. The triode filament-circuit resistance is adjusted until the filament circuit voltmeter reads 4 volts. The diode filament-circuit resistance is adjusted until the milliammeter reads 0·4 mA. The triode grid voltage is adjusted to, say, −6 volts and the anode voltage as recorded by the electrostatic

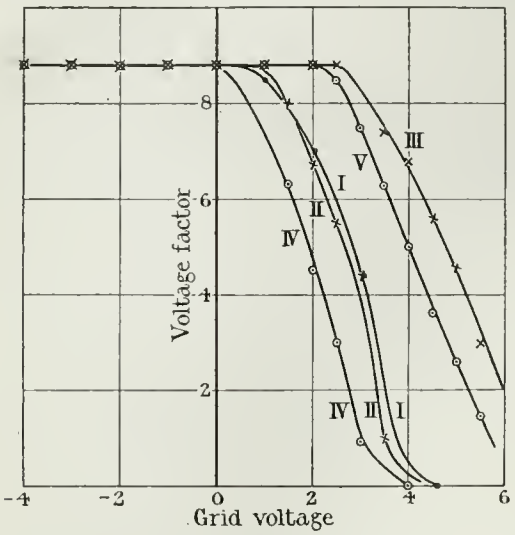

Fig. 3.—Curves deduced by differentiation of curves in Fig. 2.

instrument is noted. The grid voltage is then raised in steps of one or two volts and the reading of V noted for each successive value of the grid voltage. The tangent of the angle of slope at a point of the v_g, v_a curve is the value of the voltage factor at that point.

Fig. 2 shows some results obtained with an "R" triode using different anode currents and different

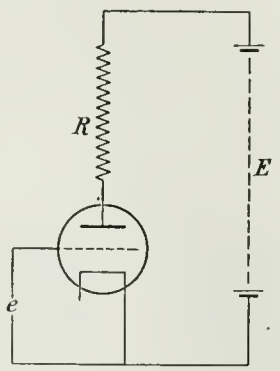

Fig. 4.

filament voltages. It will be noted that when the grid voltage is negative or only slightly positive the slope of the curves is constant and is independent of the value of the anode current and also of the filament voltage. Fig. 3 shows the tangent of the angle of slope plotted against grid voltage.

These curves indicate that as the grid voltage is increased the value of the observed voltage factor gradually falls to zero. It begins to fall off (owing to increase of grid current) at a lower grid voltage when the anode current is low, and for constant anode current begins to fall off at a high grid voltage when the filament voltage is low.

In the ordinary method of using a triode as a voltage amplifier, a high resistance is included in the anode circuit and the variations in potential difference across this resistance resulting from variations in the grid potential are utilized. In Fig. 4 let E be the voltage of the anode battery, R the resistance in the anode circuit, r the internal (differential) resistance of the tube, v the grid voltage, i the anode current, and V the potential difference across the resistance R; then for

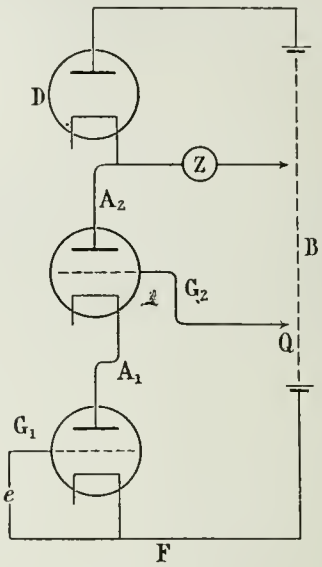

Fig. 5.

slight changes of grid voltage we have the change in anode current given by

$$di = \frac{\nu dv}{r + R}$$

when ν is the voltage factor; and therefore

$$dV = Rdi = \frac{\nu dv}{1 + r/R}$$

The amplification ratio is

$$\frac{dV}{dv} = \frac{\nu}{1 + r/R}$$

Now when the resistance R is replaced by a saturated diode the resistance offered to small changes of voltage is very large, or, in other words, the changes of current are extremely small. In the ideal case the differential resistance R tends to infinity, and the amplification ratio, by the above equation, approaches its largest possible value, namely, that of the voltage factor. Hence a

diode may replace the resistance or impedance in a voltage amplifier with resulting increased sensitiveness in the amplifier. This arrangement has been utilized by various inventors during the past five or six years.

In the foregoing the limited current has been supplied to the anode of a triode, but it is possible to pass the current into the grid and to use the anode as the control electrode.

Two Triodes in Series.

Fig. 5 shows an arrangement of two triodes in series, the current through them being limited by the saturated diode D. A voltage e applied across the grid filament of the first triode; results in a change in voltage of νe between A_1 and F_1 which may be indicated by the electrostatic voltmeter Z. If G_2 is connected to F through a suitable battery, which may be part of the battery B across the three tubes, then the difference of potential between G_2 and A_1 is altered by $\nu_1 e$—since the potential of G_2 is fixed and equal to that of Q, while the potential of A_1 has altered by $\nu_1 e$. This alteration of voltage between G_2 and A_1 will result in an alteration of voltage between A_1 and A_2 amounting to $\nu_2 \times \nu_1 e$. Therefore the change in the difference in potential between F and A_2 will be

$$\nu_1 e + \nu_2 \nu_1 e = \nu_1 e(1 + \nu_2)$$

and this voltage change will be indicated by Z; that is, the voltage applied between G_1 and F will be magnified $\nu_1(1 + \nu_2)$ times.

Use of a Diode as Amplifier.

When in the circuit arrangements of Fig. 1 the filament voltage of the diode is raised gradually so that the saturation current of the triode is reached, the position of the two tubes is reversed—the triode now limiting the current through the apparatus. If the battery voltage applied across the two tubes is sufficiently great, this change in the relations of the tubes is accompanied by a very sudden change in the distribution of voltage between them. With low filament voltages a very small change in the emission of the diode is sufficient to bring about this sudden change. This may be illustrated by an experiment in which the filament emission of the diode is altered by including a fine copper grid in its filament circuits; the grid has been blackened, in order that it may absorb heat radiation readily. The filament voltage of the diode is adjusted so that its saturation current is just greater than that of the triode, and then the filament emission is lessened by bringing a hot plate near to the copper grid. Since in this experiment the grid of the triode is not used, the triode may be replaced by a diode.

This arrangement constitutes an amplifier of the voltage applied in the filament circuit—the amplification obtainable being of the order of 1 200 when the voltage across the tube in series is about 125 volts.

When the filament emission of the diode is adjusted so that the saturation current is just less than the saturation current of the triode, a small increase in the positive potential applied to the grid of the triode may result in an increase in the voltage across the triode. With this arrangement we have the unusual result of an increase in the grid potential of a triode resulting in an increase in the anode potential. The explanation of the phenomenon is that when the grid is positive and the tube saturated, an increase in grid potential results in an increase of grid current at the expense of the anode current. The triode therefore behaves as if the saturation current had been lessened, and the distribution of voltage between diode and triode is disturbed.

The saturation current of the triode may be lessened by the application of an external magnetic field which tends to stop the flow of electrons across the tube. In consequence the application of a magnetic field in the correct direction produces an increase of the voltage across the tube. In this experiment it is evident that the triode may be replaced by a diode.

We have to thank the General Electric Company for the diodes used in the experiments shown.

Light-Contact Relays.

When the grid of a hard tube is not connected to the filament, but is left insulated, it gradually acquires a charge of negative electricity, which may result in its potential falling to perhaps as much as a volt below the potential of the negative end of the filament. If the potential applied to the anode is sufficiently high, the anode current flowing will be appreciable, and if now by the closing of a contact the grid is made much

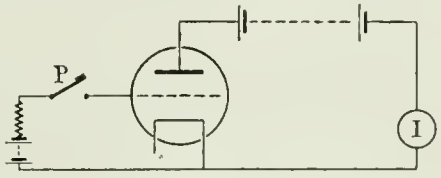

Fig. 6.

more negative this anode current will drop to a very small value, perhaps even to zero. This change of anode current can be shown by a variety of methods, the obvious one being to place an indicating instrument in the anode circuit as in Fig. 6. The grid current will be negligible after closing contact, since an equilibrium negative voltage was reached before the closing of the contact; but it is easy to make sure that the actual current passing through the contact may be limited to a very small value by including a high resistance in the circuit of the contact. Therefore in Fig. 6 the contact-maker P may be a very light one—for instance, the moving part of some instrument actuated by a very small current, such as the string of an Einthoven galvanometer. If the contact is made and broken at a musical frequency, the resulting variations in anode current may cause a note of the same frequency to be heard in telephones included in the anode circuit of the triode.

The change of anode current produced by the closing or opening of a light contact may also be shown by aid of an oscillatory circuit associated with the triode. The closing of a contact in the grid circuit may, according to circuit conditions, either start or stop oscillations,

or, intermediately, affect the frequency or amplitude of the oscillations generated. The circuit is given in Fig. 7.

The circuit S is tuned to the oscillation set in its

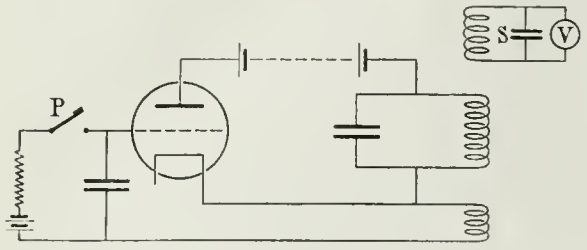

Fig. 7

normal condition, resonance being indicated by the voltmeter V. When the contact closes, the deflection of V alters.

Detection of Small Movement of a Conductor.

It is now well known that exceedingly small changes in the capacity or inductance of an oscillatory circuit maintained by a triode valve can be detected by the heterodyne method. In a recent paper read before the Wireless Society of London on 21st December, 1920, Professor Whiddington showed that a movement of one plate of an air condenser through a distance less than one-millionth of an inch could be detected by double heterodyning.

If another oscillatory circuit is coupled with the circuit maintained by the valve, this second circuit will, if the coupling be not too loose, affect the frequency of the oscillations to an extent depending on the constants of the added circuit. These changes have been discussed by S. R. Humby and B. F. J. Schonland,* by J. S. Townsend,† by B. van der Pol and others. Townsend showed that when the inductance is varied a sudden change in the wave-length and curious alterations in oscillating current may occur as the two circuits are brought nearly into tune. Analogous phenomena occur, we have found, when the capacity is gradually altered. Similar experiments are described by R. Stanley in the second volume of his text-book on Wireless Telegraphy, in which the grid and anode oscillatory circuits were separately tuned to some given frequency and the wave-length of the radiation determined when the system was kept in oscillation.

However, not all these circuits are sensitive to small alterations of capacity. Some useful circuits are suggested in Figs. 8 and 9. Here K is the condenser of small capacity whose capacity variation is to be detected, N is a large inductance, C, L form the oscillatory circuit and S is a grid leak.

The change in the capacity of K may be brought about by the movement of one of the plates of K or

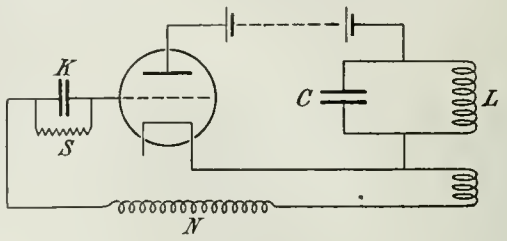

Fig. 8.

by the movement of an independent conductor within the electric field of these plates. The conductor near or between the plates of K might be the leaf of a gold-leaf electroscope, or the pointer of a table galvanometer, or it might be a wire attached to the string of an Einthoven galvanometer.

Both the light-contact experiments and those just shown may form the basis of relays which can be actuated by extremely feeble currents or by extremely slight movements of conductors. The indicating instruments might take various forms, e.g. a galvanometer in the plate circuit of the valve, thus utilizing the change in anode current brought about by the movement of the conductor, or a crystal and galvanometer across the

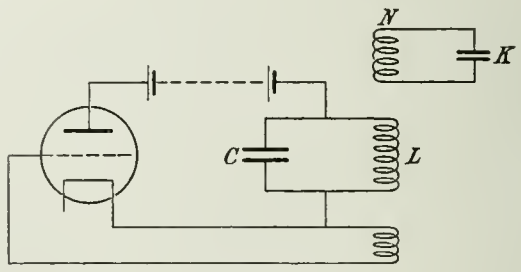

Fig. 9.

condenser of an oscillatory circuit, or some kind of printing or self-recording instrument could be used in place of the galvanometer.

The possibilities of the apparatus are many, and it may add appreciably to the ever-growing variety of uses to which the thermionic valve may be put.

We have to thank Messrs. L. Grinstead and T. W. Price, students at Finsbury Technical College, for carrying out a great many of the early investigations upon these circuits.

* *Electrician*, 1919, vol. 83, p. 443.
† *Radio Review*, 1920, vol. 1, p. 369.

Discussion.

Mr. R. C. Clinker: The experiment which I propose to show is in connection with a statement on the last page of the paper, where the authors mention the effect upon the frequency of the oscillations of another oscillatory circuit coupled with the circuit maintained by the valve. I think it may be of interest. Possibly there may be some here who have wanted to measure wave-length and have not noticed that this method can be used to get wave-lengths, or to measure inductance and capacity with great accuracy. The circuit is kept oscillating by means of a valve. Here is a loosely-coupled circuit

which I tune, and I move the condenser through the resonant point. What happens at this point is that the current in the separate circuit suddenly changes its phase through nearly 180 degrees, and a "click" is noticed in the telephones which are attached to the valve circuit, so that the exact time when the two are in tune is indicated very closely. The noise is not very loud, but it can be heard plainly when wearing the telephone head-piece. It is very much clearer, however, if the valve is made to "howl." We do not usually want to make a valve howl, but in demonstrating this effect it is an advantage because it is much more sensitive. When the resonant point is reached, the change of pitch of the howl can be heard; in fact, it can be heard when the coupling is very loose indeed. By using a condenser which can be very closely adjusted, it will be found that the pitch of the note in the telephone does not simply go down and up. It sometimes gives two minima, showing, apparently, that there is a double frequency existing in the oscillating circuit.

(*Communicated*): In connection with the method of measuring the voltage factor given in the paper, it may be of interest to note that patent specification No. 5 373 of 1915 describes the use of a saturated diode in series with a triode for the purpose of amplification. On seeing this specification, I used in 1919 the arrangement shown in Fig. 1 of the paper for the measurement of the voltage factor of different types of triodes. Records show that B.T.H. type "R" and type "B" triodes gave maximum factors of 9·6 and 20·0 respectively. On page 435 the authors refer to the accumulation of a negative charge by an insulated grid. This effect may be shown in a rather striking manner by connecting a well-insulated variable condenser between grid and filament, and reducing its capacity suddenly from maximum to minimum value. The result of this reduction of capacity is to lower the potential of the grid enormously, with the result that the anode current is cut off completely, and may remain at zero for minutes if the insulation is good. If the normal potential of the insulated grid is −1 volt, and the ratio of maximum to minimum capacity of the condenser is, say, 70, the effect is to reduce the grid potential to −70 volts.

Mr. L. B. Turner: The authors have presented certain interesting theoretical possibilities, and have demonstrated that it is practicable to realize them: they have bridged the wide gulf between the diagram on paper and the apparatus in the laboratory. The arrangement which I think the most novel in principle is that in which the change of potential to be detected or magnified is applied across the filament of one of two tubes connected in series. The large magnification obtained, of the order of 1 200, recalls the achievements of retro-active triode circuits, but here the large magnification is obtained without an approach towards instability by retro-action in the usual sense. I think the authors put forward this arrangement, as indeed the others described, perhaps as of scientific rather than engineering significance, at least at this stage; but in comparing it with the usual triode amplifying devices one must bear in mind that the momentous property of the amplifying triode (with negatived grid), viz. that of responding to a change of potential unaccompanied by a corresponding change of energy consumption, is here sacrificed. The change of potential, when applied across the filament instead of the grid, can be maintained only at the cost of a large expenditure of power. From the engineering or applied standpoint, also, the replacement (Figs. 1 and 5) in the anode circuit of the ordinary form of resistance (whose slope-resistance equals its steady-current resistance) by a saturated thermionic tube (whose slope-resistance is infinite) does not seem to be advantageous. With a single amplifying triode, there may be a quite moderate gain in amplification or saving in the high-tension battery, but only at the cost of an additional filament battery; and in the series arrangement of multi-triode amplifier suggested in Fig. 5, every additional triode involves an additional filament battery and an increase in the high-tension battery. The authors have compared Fig. 7 with the oscillatory valve relay. In the latter device, the object of providing the oscillation was to enable a small change of grid potential to engender a large change of anode current. Where the signal is in the form of a mechanical motion making or breaking an electrical contact, the necessity for the generation of oscillation vanishes. Thus, in Fig. 7, it would seem that an anode change of any desired amount could be effected without recourse to any oscillatory circuits merely by selecting a suitable grid battery. The small mechanical motion of P may be caused to effect a large change of grid potential as easily as a small change.

Mr. J. Scott-Taggart: In connection with the latter portion of the paper, the reference to the relay action produced by having an absorbing oscillation circuit coupled to the output circuit of an oscillating valve interested me very much, as I have been carrying out some experiments on these lines. The actual method of operating the relay consisted in including it either in the grid system of the valve or in the anode circuit. When the wave-length changed I noted that it was always accompanied by a large change, or at any rate by a change which could be made large, in the anode current, and this change could be arranged to operate the relay. Another method which I tried was to have an oscillatory circuit tuned to the changed wave, and a detector and relay in circuit with that, so that when the new wave-length came into force it energized the coupled circuit and operated the relay. The arrangement, however, was not altogether as sensitive as might have been expected. There certainly was a distinct, quick change of wave-length with consequent change of current, but the device seems to lack practical utility because of the great difficulty of getting it back to its original state. So that, although the arrangement may be satisfactory for indicating a sudden impulse, there seems to be great difficulty in applying it to the reception of signals. In fact, I could not get it back to its original condition after being "trigged," unless I replaced the tuning condenser. Perhaps the authors have devised a manner in which this can be done conveniently.

Admiral Sir Henry Jackson: I am not going to

criticize the paper, but I think it is the first time I have heard a lady lecture in this room, and I should like to congratulate her on the successful way in which she carried out those very delicate and very difficult demonstrations of the experiments. I hope it will not be the last time the Wireless Section will hear a lady lecturer; she has certainly given us plenty to think of.

Mr. B. Davies (*communicated*): The light-contact relay described in the third section of the paper is specially interesting to telegraphists. When it is considered that in certain telegraphic circuits the moment of the force available for contact-making is often well below 15 dyne-cm., it becomes clear that any device which will render the closing of such circuit a precise and stable operation will be welcomed. The experience I have had in the endeavour to obtain good contact between two metals by the help of a valve in the manner shown in Fig. 6 has resulted in proving that the mechanical pressure needed to make a stable contact can be quite considerable—at least in the case of platinum. I have seen two freshly-cleaned platinum wires brought together and failing to make contact when subjected to a pressure of 100 dynes. When contact is effected, the current obtained from the thermionic valve is often of similar irregular character to that through a carbon microphone. I do not know how much of this irregularity in the case of platinum is due to minute mechanical tremor. The failure to make contact is obviously due to a surface condition. Contact elements of silver act differently; the contact being effected more readily than with platinum, and a steady state is attained at a much lower mechanical pressure. Surface action is certainly less prominent with silver than with platinum. I had occasion recently to use a rotary interrupter for dealing with weak currents. I avoided the use of a copper rotor and copper brush because their contact resistance was not stable. I ultimately used a silver rotor with a hard phosphor-bronze brush. The combination gave a very uniform current and a stable condition. Eureka, which usually acts as quite a good contact maker, was tried as a brush against the rotating silver in the first instance, but was unsatisfactory owing to thermo-electric action. There is need for research on contact making. A line of attack is possibly indicated in Dr. Owen's papers* on "Rectifying Contacts," and Dr. Eccles's "Theory of Radio-telegraphic Detectors."† It is also quite possible that photo-electric action may be helpful in the quest. After a surface is freshly cleaned, its photo-electric sensibility falls off very rapidly with time, even in the case of platinum. The contact-making property and photo-electric sensibility may be related to one another.

Mr. H. R. Rivers-Moore (*communicated*): Reference is made on page 435 to the application of a magnetic field to check the flow of electrons across the tube. Can some further details be given? If a valve with straight filament and cylindrical electrode was used it would seem that a field applied across the cylinder would have a balanced effect in the two halves, and no result as a whole. If applied axially along the cylinder, all the electron streams would be at right angles to the lines of magnetic force, but again they would be in opposite directions on opposite sides of the filament. I am assuming that electron streams in a magnetic field behave as currents in movable conductors and are acted upon accordingly. Is there any other effect?

Dr. W. H. Eccles and **Miss W. A. Leyshon** (*in reply, communicated*): We wish first to thank Mr. Clinker for showing his instructive and suggestive experiment and for confirming our account of a method of measuring the voltage factor of triodes.

In reply to Mr. Turner we are aware that the necessity for the use of a number of separate filament batteries in the series arrangement of triodes, instead of a single large battery for filaments in parallel, is perhaps a nuisance, but this is of less importance when triodes with very fine filaments are used, such as those recently produced by the General Electric Company. In regard to the use of an oscillatory circuit in the light-contact relay apparatus, the object is, as indicated in the paper, to make the relay self-restoring or non-restoring at will, by a small change of circuit conditions. This cannot be accomplished by using the anode current changes in the way he suggests.

In reply to Mr. Scott Taggart it is possible to make the capacity form of relay self-restoring by suitably adjusting the coupling and the filament temperature. We find, however, at present that with this adjustment the actual change in anode current produced by a small change of capacity is always much smaller than when the relay is used in the non-restoring condition.

In reply to Mr. Davies we have found that when using relatively high voltages—say 100 volts—on contacts between ordinary metals, there is no difficulty in securing contact of the degree we desire; and, provided that the current permitted to flow across the contact is only a fraction of a micro-ampere, stiction and other diseases of contacts disappear.

In reply to Mr. Rivers-Moore, the experiment shown was carried out with the lines of force of the applied magnetic field approximately parallel to the filament of the diode. The result of the application of the magnetic field is presumably to alter the paths of the electrons in a spiral manner and, consequently, to return some of them to the filament. Hence, with a constant applied anode voltage the current through the tube is diminished by the application of the magnetic field.

* *Proceedings of the Physical Society*, 1916, vol. 28, p. 173; and vol. 29, p. 33.

† Ibid., 1913, vol. 25, p. 273.

THE RADIATION RESISTANCE OF VARIOUS TYPES OF ANTENNA CONSTRUCTION.*

By A. PRESS, Member.

(*Paper received* 29 *June*, 1920.)

SUMMARY.

The loss of energy by electromagnetic radiation, quite apart from absorption effects, is likened to a compound hysteresis loss. This is because both the electric field and the magnetic field absorb energy from the system, but the quantum of energy returned is rather less than that taken up to charge the respective fields. After developing the mathematics for elliptical hysteresis, the antenna is treated as a transmission line loaded hysteretically because of radiation effects. The true voltage and current distribution values as a function of the antenna length then follow. As a result of this analysis the trough in the antenna resistance characteristic as a function of the impressed frequency is explained. The rising branch is to be described to the capacitative radiation, quite apart from pure absorption effects. The falling branch is then seen to be due to the magnetic component of radiation. The minimum in the characteristic curve corresponds to the radiation condition in which the capacitative equals the magnetic hysteresis effects. The hysteresis constants per unit of length as obtained from the graphs are then to be made the means of comparing different antenna systems.

It has been customary to develop formulæ for the voltage and current distributions of antennæ based on the theory of power-transmission lines. Such formulæ have hitherto taken no cognizance of radiation.† It will be the province of the present paper to indicate how antennæ can be compared from a radiation standpoint.

Elsewhere I have shown how the Maxwellian instantaneous potentials, electric and magnetic, for all space, give rise to diverging lines of force, electric and magnetic. Hence the hysteresis interpretation given in the paper.

Judged broadly, the source gives energy to the medium about the antenna system. This energy of the fields, electric and magnetic, is then either completely returned to the source or is dissipated as heat, mechanical power, or electromagnetic radiation. The two fields in question being the electric and the magnetic, it is safe to liken the loss of power by radiation to a hysteresis equivalent. Thus, over one-half of the cycle the two fields offer a resistance to being charged, and on the second half return a quantum of energy rather less than was introduced into the medium. The difference in quantum corresponds to the radiation of the field energies, electric and magnetic. This calls for a consideration of complex inductances and capacitances.‡

We regard the voltage as determined by the relation

$$n\frac{d\phi}{dt}10^{-8} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The flux ϕ can then be expressed in terms of the current i, which latter would be in absolute time phase with the magnetic intensity H. If hysteresis is present, the flux ϕ, and therefore the density B, will not be in phase with the magnetic intensity H and will therefore be out of phase with the current i. Thus, assuming elliptical hysteresis, we have

$$\left.\begin{aligned} H &= H' \sin pt\,;\ p = 2\pi f \\ B &= H'(\mu_1 \sin pt - \mu_2 \cos pt) \end{aligned}\right\} \quad . \quad . \quad (2)$$

Then, since it is possible operatively to express the cosine in terms of the sine, we have

$$\frac{d}{dt}\sin pt = t_1 \sin pt = p \cos pt$$

$$\frac{d^2}{dt^2}\sin pt = t_1^2 \sin pt = -p^2 \sin pt$$

It will be found possible in general to write

$$t_1 = pj\,;\ j = \frac{1}{p}\cdot\frac{d}{dt}\,;\ j^2 = -1 \quad . \quad . \quad . \quad (3)$$

with j as a generalized form of imaginary number. That is

$$t_1 \sin pt = pj \sin pt = p \cos pt$$
$$j \sin pt = \cos pt \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Introducing the operational form (3) in (2) we have

$$\left.\begin{aligned} H &= H' \sin pt \\ B &= H'(\mu_1 - \mu_2 j)\sin pt \end{aligned}\right\} \quad . \quad . \quad . \quad (5)$$

From this we gather, putting $B = \mu' H$, that

$$\mu' = \mu_1 - \mu_2 j \quad . \quad . \quad . \quad . \quad . \quad (6)$$

In the form (6), therefore, the permeability μ' is complex and fully determines the condition that the hysteresis be present. It should be remembered, however, that j in (6) is to be interpreted from a generalized number point of view in the light of (3) above.

Had we considered a simple tore, then from (1) we have the relationship

$$\phi = AB$$

where A is the cross-section of the tore. Making use now of (6) we have

$$\phi = A\mu' H$$

* The Papers Committee invite written communications (with a view to publication in the *Journal* if approved by the Committee) on papers published in the *Journal* without being read at a meeting. Communications should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

† The methods involving the use of so-called "deferred potentials" are being criticized elsewhere.

‡ For an exposition of complex reactances see the author's "Harmonic Algebra," Engineering Publications of the University of California, September 1919.

but since on the Heaviside system we have, in practical units, $H = ni/10l$

$$\phi = \frac{A\mu' ni}{10l} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where l is the mean perimeter of the tore. That is

$$e = \frac{A\mu' n^2}{10^9 l} \cdot \frac{di}{dt} = L'' \frac{di}{dt} \quad . \quad . \quad . \quad . \quad (8)$$

it being understood that L'' is complex because of μ'.

With a tore having a vacuous core we should have, in Heaviside practical units

$$L_0 = \frac{An^2}{10^9 l} \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

from which we can write

$$L'' = L_0(\mu_1 - \mu_2 j) = (L_1 - L_2 j) \quad . \quad (10)$$

$$e = pL_0(\mu_2 + \mu_1 j)i \quad . \quad . \quad . \quad . \quad . \quad (11)$$

However, in terms of reactances, we have for a vacuous core

$$pL_0 = X_0 \,;\; X' = X_0(\mu_2 + \mu_1 j) \quad . \quad . \quad (12)$$

$$e = X_0(\mu_2 + \mu_1 j)i = X'i \quad . \quad . \quad . \quad . \quad (13)$$

It will be observed from (10) that equation (13) substantially introduces a resistance term $\mu_2 X_0$ in phase with the current, to allow for the hysteresis loss. The resistance

$$\mu_2 X_0 = \mu_2 2\pi f L_0 \quad . \quad . \quad . \quad . \quad . \quad (14)$$

expresses the hysteresis resistance as a function of the frequency.

Turning now to the capacitance we have the formula

$$Q = \kappa C_0 e \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

where C_0 refers to the capacity in a vacuum. If, however, the charge Q lags behind the E.M.F. it can be expressed as a complex as was the permeability μ'. That is, let $e = E \sin pt$

then $$Q = C_0 E(\kappa_1 \sin pt - \kappa_2 \cos pt) \quad . \quad . \quad (16)$$

Then, employing (3) above we have

$$Q = C_0 E(\kappa_1 - \kappa_2 j) \sin pt$$

$$Q = C_0(\kappa_1 - \kappa_2 j)e \quad . \quad . \quad . \quad . \quad . \quad (17)$$

In other words, in conformity with (15) we can write

$$C' = C_0(\kappa_1 - \kappa_2 j) \quad . \quad . \quad . \quad . \quad (18)$$

regarding the capacity as complex. Or in conformity with (6), we have

$$\kappa' = \kappa_1 - \kappa_2 j \quad . \quad . \quad . \quad . \quad . \quad (19)$$

To derive the capacitance in ohms, we have for the current

$$i = \frac{dQ}{dt} = C' \frac{de}{dt} = pjC'e$$

$$e = \frac{\kappa_2 - \kappa_1 j}{pC_0(\kappa_1^2 + \kappa_2^2)} i \quad . \quad . \quad . \quad . \quad (20)$$

Thus, if Y' is the capacitance in ohms, we have

$$\left.\begin{aligned} Y' = Y_0(y_1 - y_2 j) = Y_0\left\{\frac{\kappa_2}{\kappa_1^2 + \kappa_2^2} - j\frac{\kappa_1}{\kappa_1^2 + \kappa_2^2}\right\} \\ \text{where} \qquad y_1 = \frac{\kappa_2}{\kappa_1^2 + \kappa_2^2} \,;\; y_2 = \frac{\kappa_1}{\kappa_1^2 + \kappa_2^2} \end{aligned}\right\} \quad (21)$$

It is to be observed from (19) that the electrical hysteresis loss is allowed for as a series resistance of the value

$$\frac{\kappa_2}{\kappa_1^2 + \kappa_2^2} Y_0 = \frac{\kappa_2}{\kappa_1^2 + \kappa_2^2} \frac{1}{2\pi f C_0} \quad . \quad . \quad (22)$$

Thus the quantities μ_2 and κ_2 are to be regarded as functions of the frequency if desired.

Turning now to the differential equations with reference to a horizontal antenna system, we have at x the voltage to earth e. At $(x + dx)$ the voltage to earth is $e + \frac{de}{dx}dx$. The difference is made up by the ohmic conduction drop $\rho dx i$ and a complex reactance drop $(L''dx)\frac{di}{dt}$. That is

$$\frac{de}{dx}dx = \rho dx i + L''dx\frac{di}{dt} \quad . \quad . \quad . \quad . \quad (23)$$

It will be found convenient to write (23) in the form

$$\frac{de}{dx} = L'\frac{di}{dt} \quad . \quad . \quad . \quad . \quad . \quad (24)$$

giving the result

$$L't_1 = \rho + L''t_1 \quad . \quad . \quad . \quad . \quad . \quad (25)$$

$$L' = L_1 - j(\rho/p + L_2) \quad . \quad . \quad . \quad (26)$$

Again, with the current i at x, we have $i + \frac{di}{dx}dx$ at $(x + dx)$. The increment of current di passes through the complex capacitance $C'dx$ with the voltage e of the line. That is

$$e = \frac{\frac{di}{dx}dx}{C'dxt_1} \,;\; \frac{di}{dx} = C'\frac{de}{dt} \quad . \quad . \quad . \quad (27)$$

Combining now the two equations (24) and (27) it follows that

$$\frac{d^2e}{dx^2} = L'C'\frac{d^2e}{dt^2} \quad . \quad . \quad . \quad . \quad (28)$$

$$\frac{d^2i}{dx^2} = L'C'\frac{d^2i}{dt^2} \quad . \quad . \quad . \quad . \quad (29)$$

Formally, therefore, the solution is the same in both cases.*

Since we have $t_1 = pj$ the equations (28) and (29) can be written in the form

$$\frac{d^2i}{dx^2} = -p^2L'C'i = q^2 i \quad . \quad . \quad . \quad (30)$$

* There is an important difference between "formal" solutions in operational mathematics, and general algebraic solutions. Formal equivalence is not necessarily algebraic equivalence. (See *E.M.T.*, vol. 2, p. 481.

where $q^2 = -p^2L'C'$

The solution is therefore of the form

$$i = A \sinh qx + B \cosh qx \quad . \quad . \quad . \quad (31)$$

With the usual type of antenna of aggregate length h we have

$$i = 0 \text{ when } x = h \text{ (see Fig. 1)} \quad . \quad . \quad (32)$$

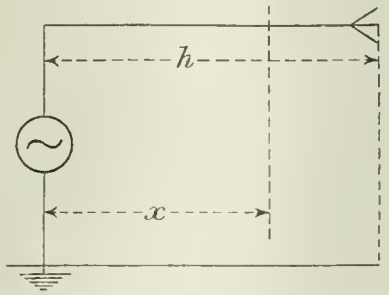

FIG. 1.

Substituting these conditions in (31) above we get

$$B = -A \tanh qh \quad . \quad . \quad . \quad . \quad (33)$$

with the result that

$$i = A(\sinh qx - \tanh qh \cosh qx) \quad . \quad . \quad (34)$$

To arrive at an equation in terms of e we have, by (27),

$$e = -\frac{j}{C'p}\frac{di}{dx} \quad . \quad . \quad . \quad . \quad (35)$$

$$e = -j\frac{Aq}{C'p}\left\{\cosh qx - \tanh qh \sinh qx\right\} \quad (36)$$

To evaluate A we shall assume

$$e = v_0 \sin pt \text{ for } x = h \quad . \quad . \quad . \quad (37)$$

This will enable a comparison to be made with the low-frequency or non-radiating type of transmission-line antenna. Substituting, we have

$$v_0 \sin pt = -j\frac{Aq}{C'p}\left\{\frac{1}{\cosh qh}\right\} \quad . \quad . \quad (38)$$

$$A = \frac{C'pj}{q}\cosh qh \,.\, v_0 \sin pt \quad . \quad . \quad . \quad (39)$$

Substituting this value of A in (36) we have

$$e = \cosh q(h - x) \,.\, v_0 \sin pt \quad . \quad . \quad (40)$$

Again, for the current we have, by (34),

$$i = -\frac{C'pj}{q}\sinh q(h - x) \,.\, v_0 \sin pt \quad . \quad (41)$$

so that finally

$$i = \frac{C'p}{jq}\sinh q(h - x) \,.\, v_0 \sin pt \quad . \quad . \quad (42)$$

It follows in consequence that the admittance at any point x is given by

$$Z_x^{-1} = \frac{C'p}{jq}\tanh q(h - x) \quad . \quad . \quad . \quad (43)$$

This admittance is proportional to the current. The charging admittance at the base of the antenna is therefore

$$Z_0^{-1} = -jphC'\frac{\tanh qh}{qh} \quad . \quad . \quad . \quad (44)$$

Thus from (43) it is clear that only when q, C' and L' are real does the power factor or phase angle between voltage and current remain independent of the position taken along the antenna. From (40) it appears that the voltage-distribution curve lies on a rotating helical sheet, winding about the antenna as an axis, whereas in the non-radiating or low-frequency type the locus lies in a rotating plane passing through the antenna as axis.

To bring out the relationship with respect to the inductivity we must consider an approximation by means of series of the cotanh development.

Since

$$\frac{\tanh x}{x} = \frac{1}{x}\cdot\frac{\sinh x}{\cosh x}$$

we see that for large values of h

$$\frac{\tanh x}{x} \equiv \frac{1}{x}$$

For small values, on the other hand, we have, taking the initial two terms

$$\frac{\tanh x}{x} \equiv 1 - \frac{1}{3}x^2$$

in which case the value varies around unity. Thus for the charging impedance Z_0 we have substantially, from (44)

$$Z_0 \equiv j\left\{\frac{1}{C'hp} - \frac{1}{3}L'hp\right\} \quad . \quad . \quad . \quad (45)$$

Strictly speaking, for the practical case (44) should be employed instead of (45).* On the other hand, for large values

$$Z_0 \equiv \frac{jqh}{C'hp} \quad . \quad . \quad . \quad . \quad . \quad (46)$$

To evaluate we note

$$q^2 = -p^2L'C' = -p^2L_1C_1\left(1 - \frac{L_2}{L_1}j\right)\left(1 - \frac{C_2}{C_1}j\right)$$

However, for an antenna of negligible resistance so far as the wire is concerned

$$p^2L_1C_1 \equiv \frac{2\pi}{\lambda} \quad . \quad . \quad . \quad . \quad . \quad (47)$$

whence we have

$$q = j\frac{2\pi}{\lambda}\left(1 - \frac{L_2}{L_1}j\right)\left(1 - \frac{C_2}{C_1}j\right) \quad . \quad . \quad (48)$$

In this case we can write for convenience

$$\frac{L_2}{L_1} = n_1\,; \quad \frac{C_2}{C_1} = n_2 \quad . \quad . \quad . \quad . \quad (49)$$

* By a well-known hyperbolic transformation we have

$$\frac{\tanh qh}{qh} = U + jV$$

so that, from (48), the general form (44) becomes available.

and the question arises: What is the magnitude of qh in view of n_1 and n_2? It will be shown below how to obtain an estimate of the n values by actual test on an antenna construction. If then we assume that n is large compared with unity we have, practically

$$\left.\begin{aligned} q &= \pm \frac{2\pi}{\lambda}(n_1 n_2) = \pm \frac{2\pi n}{\lambda} \\ qh &\equiv \pm \frac{2\pi n h}{\lambda} \end{aligned}\right\} \quad . \quad . \quad (50)$$

However, under these circumstances

$$C' = C_1 - C_2 j = C_1\left(1 - \frac{C_2}{C_1} j\right) \quad . \quad . \quad (51)$$

so that as a first approximation we can write

$$Z_0 \equiv \frac{2\pi n h}{\lambda C_1 n_2 h p} \equiv \frac{2\pi h}{\lambda} \frac{1}{2\pi f (C_1 h)} \quad . \quad . \quad (52)$$

When interpreted, the above formula takes on a very important aspect. It implies that at very high frequencies the entire antenna capacitance, instead of being reactive as heretofore assumed, should be taken as ohmic. This simplifies the matter of antenna calculations very considerably. As the wave-length becomes greater the earlier formula (44) will have to be resorted to, since (45) is only roughly true.

Turning to the earlier development for large amounts, let

$$\frac{1}{C'} = \frac{1}{C_{22}} - \frac{1}{jC_{11}}$$

then we find that we must have

$$C_1 = \frac{C_{11}^2 C_{22}}{C_{11}^2 + C_{22}^2} \; ; \; C_2 = \frac{C_{11} C_{22}^2}{C_{11}^2 + C_{22}^2}$$

Introducing these values in the above formula for the charging impedance, we have after a few simple reductions

$$Z_0 = \left(\frac{1}{hpC_1} + \tfrac{1}{3} hpL_2\right) + j\left(\tfrac{1}{3} hpL_1 - \frac{1}{hpC_2}\right) \quad (53)$$

In other words, as a function of the frequency we should have for the ordinary resistance including the antenna radiation resistance

$$\Omega_r = \frac{1}{hpC_1} + \tfrac{1}{3} hpL_2 \quad . \quad . \quad . \quad . \quad (54)$$

It will be observed that the above formula explains the character of the usual experimental curve, Fig. 2.

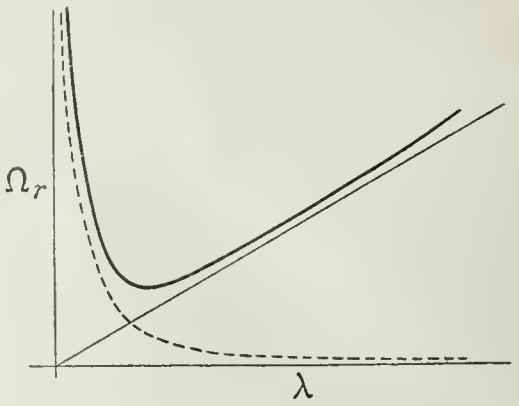

FIG. 2.

From the graph we are therefore in a position to determine the electrical hysteresis component as well as the magnetic hysteresis component. Knowing that a minimum exists as a function of the wave-length, we can write the equation in the form

$$\Omega_r = \frac{\alpha}{p} + \beta p \; ; \; \frac{d\Omega_r}{dp} = 0 \text{ gives } \frac{\alpha}{p} = \beta p \quad . \quad (55)$$

Thus, operating to find a minimum with respect to the wave-length, the condition requires that for minimum radiation resistance the magnetic radiation or hysteresis resistance should equal the electrical radiation or hysteresis resistance. At the minimum frequency both resistances are equal and measured by half the ordinate value. An analysis of experimental tests is given below.

From the data furnished by L. W. Austin in the Bulletins of the Bureau of Standards we have the following:

Antenna Type	C_1, microfarads	Height, metres	Critical Wave-length	Resistance, ohms	C_2 from $\frac{1}{pC_2} = pL_2$	$n_2 = \frac{C_2}{C_1}$
Signal Corps (Flat)	0·00171	39·6	1 500	6	0·25	146
Capitol (Flat)	0·00232	44·2	1 000	7	0·142	62
U.S.S. "Maine" (Flat)	0·00125	38·4	1 000	2	0·5	400
U.S.S. "Massachusetts" (Flat)	0·00110	39	1 000	2·5	0·4	363
Bureau of Standards (Harp)	0·00126	54·8	750	13	0·057	46
Washington Naval Yard (Harp)	0·00073	54·8	900	5	0·18	246
Arlington (Flat)	0·009	—	8 000	1·2	6·6	740
Bureau of Standards (Indoor, Flat)	0·00029	—	1 000	18	0·055	190*

* J. M. MILLER: "Electrical Oscillations in Antennas aod Inductance Coils," *Proceedings of the Institute of Radio Engineers*, 1919, vol. 7, p. 299.

The explanation of the rising branch of the radiation resistance characteristic is therefore to be ascribed to the capacitative radiation and absorption. The falling branch of the curve must then be due to the magnetic component of radiation, rather than to the electric.

PROCEEDINGS OF THE INSTITUTION.

660TH ORDINARY MEETING, 17 FEBRUARY, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 10th February, 1921, were taken as read, and were confirmed and signed.

Messrs. H. Hobson and P. S. Pitt were appointed scrutineers of the ballot for the election and transfer of members and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

Members.

Ackland, Eustace William.
Alexander, Richard.
Bullock, Sydney Charles.
Grigor, Robert.
Mowat, Francis Douglas, Comm. R.N.
Vincent, Joseph Herbert, M.A., D.Sc.

Associate Members.

Arden, George Douglas.
Ashton, Harry Ernest.
Atkins, Walter.
Balchin, George.
Barbour, George Francis.
Barnes, Edgar James.
Bell, Reginald Wells S.
Bell, Robert Lancaster, B.Sc. (Tech.).
Bellwood, George Francis.
Brown, Charles William.
Burbridge, Walter Charles.
Cobbe, Thomas.
Cowburn, William.
Davies, George Frederick.
Day, William.
Devereux, Thomas.
de Wardt, Robert George.
Dudgeon, Hector McPhail.
Eaton, Arthur.
Ellson, Fergus Alexander, B.Sc.
Fletcher, Albert Emery.
Fletcher, John Francis.
Giffen, Alexander Ewing.
Giles, Archibald Moffatt, B.Sc.
Graham, Colin.
Green, Charles Frederick.
Greening, Alfred Charles.
Hancox, Samuel Herbert.
Hill, George Arthur D.
Honan, John Joseph.
Ings, Sidney Herbert.
Jackson, John Morton.
Lang, John.
Leigh, Charles.
McIntyre, John.
McMullen, James Fleming.
McMurray, Charles Henry.
Macpherson, Hugh.
Miller, Percy John.
Morgan, John.
Nancarrow, Frederick Ernest.
Nelson, James Egar.
Neville, Sidney, B.Sc. (Eng.).
Parker, Harold Herbert.
Pennington, William.
Perry, Edmund Ernest.
Pyle, Frank.
Ridd, Percy John.
Robinson, Robert Thomas.
Scott, Walter.
Smith, William Wild.
Spencer, Walter.
Stock, William George.
Stone, Alfred Ernest.
Taylor, Alfred Rickard, M.A.
Torikai, Risaburo.
Tranmer, William Henry.
Turpin, Alfred Edward.
Wardlaw, Herbert Hope.
Wise, Frank Henry.
Woollard, Francis.

Graduates.

Alexander, Thomas Robertson.
Andrew, Hugh Frederick.
Bose, Saral Kumar.
Bown, Frederick James.
Clinch, William Norman C.
Dalal, Shavakshah Nasservanji.
Dandy, Ernest Charles.
Dixon, Benjamin J.
Fox, Walter.
Gopaliengar, Sisha.
Hickin, Sydney Botevyle.
Hunter, Drummond William S.
Jennings, Bruce Charles.
Johns, Llewellyn.
Jones, Robert Sidney.
Lowe, Thomas Clelland.
Macadam, Ivison Stevenson.
McKinnon, Hugh Train.
Payne, George Inwood.
Pealing, John Alfred L.
Porter, Ernest Bellamy.
Shillitoe, John.
Thadhani, Gopaldas Tharumal, B.Sc.
Todd, William Montgomery.
Wallis, Albert Edward.
Whaite, Samuel Charles.
White, Frank B. Howard, M.C., B.A.

Students.

Andrews, John Henry S.
Anslow, Cecil Leslie.
Bacon, Lionel Percy S.
Bamford, John.
Battersby, George Herbert.
Belchem, Robert Henry.
Bellamy, Fenton George.
Benger, Harold A.
Bird, James, B.Sc.
Bishop, Harold, B.Sc. (Eng.).
Bleddyn, John Caradog.
Boldy, Tigran David.
Bonnyman, John.
Broadbent, Ernest William.
Bromley, Frank Guy.
Brown, William Henry.
Bullen, Frank Hugh.
Camidge, William Gordon.
Cassé, Clifford William.
Charles, Edward Kay.
Clark, James d'Argaville.
Clotworthy, Stanley Edward.
Clough, Wallace Oswald.
Cooper, Sapper Claude Victor, 1859300.
Cozens, Christopher John, M.C., B.Sc.
Crotty, Arthur James.
Davies, Frederick Henry.
Dawkin, Ernest Mansel.
Dell, Robert.
Desai, Becharbhai Purushstamdas, B.A., B.Sc.
Dixon, Hugh Hume.
Edwards, Wilfrid Gordon.
Ezard, Gerald.
Gardner, Frederick Thomas.
Geach, Ernest Arthur.
Ghose, Khitish Chamdra.
Graham, Douglas Harry, B.Sc. (Eng.).
Griffin, Samuel Royston.
Hailey, Guy.
Hanney, Eric Adie.
Harris, Henry Cecil.
Harris, Lionel Herbert.
Harrison, Clifford.
Harwood, Arthur Edwin J.
Higson, Douglas.
Holt, Donald.
Horwood, Faulknor Robert F.
Hunter, Charles Moore.
Jaap, John.
Leng, Leonard John.
Leslie, The Rt. Hon. Lord.
Mackinnon, Lachlan.
Marks, Frank Edward T.
Marsden, Wilfrid Kenneth.
Mercer, Joseph Roy.
Mildred, Frank Adamson.
Muir, Ernest Charles.

Students—continued.

Murgatroyd, Eric Hall.
Murray, George Ronald A.
Nicholson, Frederick Walter.
Nicklin, Arthur William.
Noddings, Wilfrid Bellars.
Palmer, Reginald William.
Parkinson, Darwen.
Parrish, Joseph John A.
Piper, Benjamin James.
Regis, John.
Robinson, Norman Hutton.
Robinson, Reginald.
Rogers, Robert Arnold.
Roskilly, Vivian Walter.
Ross, Walter Donald W.
Searle, George Rex.
Shires, George Eric.
Short, Leonard Highton, M.C.
Small, Allan Jamieson.
Smith, Frederick Daniel.
Speakman, John Edward D.
Spence, Leonard Rolls B.
Spence, Samuel.
Thomas, Wilfrid Archibald.
Tickle, Clifford.
Turney, Trevor Hugh, B.Eng.
Tyler, Leonard Benjamin.
Tyrrell, Charles Frederick.
Usher, Frederick Francis C.
Walder, Ernest Derrick L.
Wallis, Alfred Stanley.
Wann, Andrew Stewart.
Watkinson, Joseph Edwin.
Webb, Lawrence Herbert.
Wells, Brian Lewis.
Wells, Ewart Henry, B.Sc. (Eng.).
White, Leslie Frederick.
Williams, Charles Branton.
Wilson, Allan James.
Wolf, Walter Albert L.
Womack, Harold Athelstan.
Woods, Edward Farrington.
Wright, Robert Stewart.
Wrighton. James.

TRANSFERS.

Associate Member to Member.

Burkinshaw, Henry.
Drury, Guy Lyndon.
Duffett, William Henry.
Hilyer, William John, B.Sc. (Eng.).
Lund, Charles William.

TRANSFERS—*continued.*

Associate Member to Member.

Mitton, Frederick Ernest.
Pleasance, Walter.
Sloog, Hermann.
Stewart, George Wigram.
Styles, William Ellerd.
Swingler, George Henry.
Thompson, John Harold.

Associate to Member.

Hazzledine, Henry Thornton.

Graduate to Associate Member.

Gilbert, Herbert Williams.
Stevens, Percy Edward.
Wolf, Edmundo, Lieut.-Engineer.

Student to Associate Member.

Wilson, Joseph Arnold.

Associate to Associate Member.

Rodwell, Henry Kirby.

Student to Graduate.

Beaumont, Leonard.
Edwards, John Marshall, B.Sc. (Eng.).
Kelly, John Philip, B.Sc.
Russell, Ernest Stuart.
Wilkinson, Harold Claud.

Professor E. Wilson, Member, delivered the first of a series of lectures on "Magnetic Susceptibility of Low Order" (see page 319).

The meeting terminated at 7.30 p.m.

661ST ORDINARY MEETING, 24 FEBRUARY, 1921

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 17th February, 1921, were taken as read, and were confirmed and signed.

Professor E. Wilson, Member, delivered the second of a series of lectures on "Magnetic Susceptibility of Low Order" (see page 319).

The meeting terminated at 7.10 p.m.

662ND ORDINARY MEETING, 10 MARCH, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 24th February, 1921, were taken as read, and were confirmed and signed.

A list of candidates for election and transfer approved by the Council for ballot was taken as read and was ordered to be suspended in the Hall.

Professor E. Wilson, Member, delivered the third of a series of lectures on "Magnetic Susceptibility of Low Order" (see page 319).

A vote of thanks to the lecturer, proposed by the President, was carried with acclamation and the meeting terminated at 7.10 p.m.

13TH MEETING OF THE WIRELESS SECTION, 16 MARCH, 1921.

(Held at the Institution of Mechanical Engineers.)

Dr. W. H. Eccles, Chairman of the Wireless Section, took the chair at 6 p.m.

The minutes of the meeting of the Wireless Section of the 15th December, 1920, were taken as read, and were confirmed and signed.

The following papers were read and discussed:—"The Effect of Electron Emission on the Temperature of the Filament and Anode of a Thermionic Valve," by G. Stead (see page 427); and "Some Thermionic Tube Circuits for Relaying and Measuring," by Dr. W. H. Eccles, Vice-President, and Miss W. A. Leyshon (see page 433).

The meeting terminated at 7.50 p.m.

MAGNETOS FOR IGNITION PURPOSES IN INTERNAL COMBUSTION ENGINES.

By E. A. Watson, Associate Member.

(*Paper first received* 20 *October*, 1919, *and in final form* 26 *October*, 1920; *read before* The Institution 14 *April*, *before the* North-Western Centre 22 *February*, *and before the* South Midland Centre 27 *April*, 1921.)

Summary.

The paper considers in the first instance the conditions which have to be fulfilled by ignition magnetos on internal combustion engines. These conditions are extremes of speed, high and low, voltage required, and adverse circuit conditions tending to reduce the voltage.

The magneto to be satisfactory must give a spark at sufficiently low speed to enable the engine to be started readily, and must give regular operation at high speeds under adverse conditions. The former condition is the least easy to obtain. The speed at starting depends on the class of service for which the magneto is intended, and data are given for various types of motor vehicles. The effect of adverse conditions, such as leaky plugs and wiring, and the capacity of the plug are dealt with. A method of estimating the capability of a magneto for dealing with leaky circuits is described, and characteristic curves given by various machines are illustrated.

The paper next deals with the principles of operation of a magneto. The cycle of operations accompanying each spark is considered in successive steps. These may be divided into:—(1) Storage of energy in the magnetic field; (2) the collapse of this field, and the transference of the energy to the secondary circuit.

The relation between the properties and the dimensions of the magnets, and the energy which can be stored in the magnetic field, is considered in detail, attention being drawn to the importance of the shape of the *BH* loop, and its effect upon the energy which can be stored.

It is shown that the most important property of the magnet is the maximum value of *BH*. Magnetic data for various steels are then given, and also the magnetic content which is usual for magnets on magnetos intended for various classes of service.

The principles governing the proportions of the magnet, namely length and cross-section, are considered in detail. The effect of the armature resistance is then considered, and it is shown that for the best results it is important to keep the resistance of the primary winding as low as possible, and secure the best possible linkage between the primary winding and the working flux.

The effect of the condenser as a spark suppressor is considered, and a brief account is given of Professor Taylor Jones's theoretical investigation of the effect of condenser capacity.

The laws governing the current in the secondary circuit, and the rate of flux-change after the passage of the first spark, are next outlined, and the importance of spark extinction is pointed out. Experimental results are given showing the way in which incomplete spark extinction reacts upon the performance of the machine.

Brief reference is then made to some of the mechanical points involved in the design and construction of magnetos.

Introduction.

The continually increasing importance of the internal combustion engine in all its forms, and the fact that the vast majority of these employ some form of electrical ignition apparatus, make no apology necessary for the subject of this paper. In the past, the design and production of such apparatus has been somewhat neglected by electrical engineers in this country as a subject hardly attaining to the dignity of electrical engineering, although, as the author hopes to show, it is one which requires just as careful and scientific treatment both in design and origination, and in carefully controlled manufacture, as any other branch of the industry to which it belongs. The subject is in reality one of very great interest, and it may be said of very considerable complexity, as the problems presented are in many ways different from those with which electrical engineers are usually called upon to deal. Consequently, the established methods of dealing with these problems are in many cases inadequate and unsatisfactory, and fresh and somewhat unfamiliar methods have to be adopted.

Conditions to be Fulfilled.

The function of a magneto is to supply at perfectly regular intervals a sudden electrical impulse in the form of a spark between a pair of points in the engine cylinder. These points are usually about 0·5 mm. apart and are, at the moment when the spark is required, surrounded by gas at a pressure which may reach as high as 6 or 7 atmospheres. It is impossible in the scope of this paper to inquire into the relation between the electrical characteristics of the discharge and the ignition of the gas. Very much work has been done upon this subject, and much yet remains to be carried out. It may be stated, however, that all workers are agreed that, with any of the gaseous mixtures met with in practice, provided a spark is produced between the plug points, ignition is bound to occur. The problem therefore resolves itself into stating that the magneto must be capable of giving a spark under the most adverse conditions, and that the spark must always occur at the correct moment with relation to the position of the piston, i.e. the spark must be properly synchronized. The former problem is primarily electrical, while the latter is a purely mechanical one.

Adverse conditions.—The adverse conditions likely to be met with are:—

(1) Extremes of speed.
(2) Leakage on sparking plugs, wiring and in the magneto itself.
(3) Capacity of leads, plugs and terminals, all of which tend to lower the voltage generated and prevent a spark occurring.

In order to produce a spark, a definite minimum voltage must be generated by the machine. The value of this will depend upon : (*a*) The gap between the plug electrodes ; (*b*) their configuration ; (*c*) the pressure and temperature of the gaseous mixture surrounding them.

Examination of the characteristics of representative sparking plugs appears to indicate that, in order to ensure satisfactory starting, the magneto must be capable of giving a peak voltage of 10 000 volts when the engine is turned by hand or by the electric starter when one is fitted. This speed may be taken as being approximately 100 r.p.m. on the engine in the case of cars of small and moderate sizes. It is, of course, much less in the case of lorry and tractor engines where self-starters are not fitted. The corresponding magneto speed will depend upon the ratio of the gearing between the engine and magneto. In the case of four-cylinder engines, the usual practice is to employ a magneto giving two sparks per revolution when running at engine speed, the four sparks in each cycle being distributed to their respective cylinders by means of the distributor which is usually combined with the magneto. Two-cylinder engines generally employ a magneto running at half engine-speed, an exception being made in the case of two-stroke engines in which the magneto will naturally run at engine speed, and in four-stroke engines with cranks at 180° and cylinders in line, in which case the magneto is run at engine speed with an idle spark in the exhaust stroke. In the case of the half engine-speed magneto, if the cranking speed of the engine is 100 r.p.m. the speed at which the magneto must function will be only 50 r.p.m., and this, as we shall see later, is not very easy of fulfilment. Consequently, various devices are employed to overcome this difficulty, such as geared starters enabling the engine to be rotated more rapidly, and so-called impulse starters which rotate the magneto rapidly through a spring coupling at the moment when the spark is required.

Typical speed/voltage curves are shown in Fig. 1 for :—

(1) A four-cylinder car magneto.
(2) A large motor-cycle magneto normally used on single-cylinder four-stroke engines. In this case the magneto will run at half engine speed, and the engine must run at 200 r.p.m. to produce 10 000 volts. Such engines are, however, always started by means of a " kick starter," which enables a much higher speed to be obtained than when cranking by hand.
(3) A small motor-cycle magneto usually fitted to small two-stroke engines. The magneto runs in this case at engine speed, and the engine can be rotated rapidly by a kick starter or by pushing the machine.

The upper limit of speed is very rarely encountered, except in the case of aeroplane engines. In this case, with some cylinder arrangements the normal speed of the magneto is very high. For example, a two-spark magneto employed on a 9-cylinder engine will have to run at $2\frac{1}{2}$ times engine speed. The conditions as regards voltage required are usually less severe here than at low speeds, as the temperature is considerably higher, and at extreme speeds the gas pressure in the cylinders is reduced, due to throttling in the induction system. A figure of 8 000 volts is certainly an outside one, even for high-compression engines with well-designed induction systems, and 5 000 to 6 000 volts is a more usual figure. On the other hand, adverse conditions due to plug leakage are generally more likely to occur at high speeds than at low ones. As a matter of practical experience, the high speed limit is much less difficult to meet than are the low speeds, and rarely causes trouble to manufacturers. Such problems as are associated with it are usually purely mechanical and connected with the contact breaker, which has to interrupt the circuit, rather than with the magnetic or electrical system of the machine.

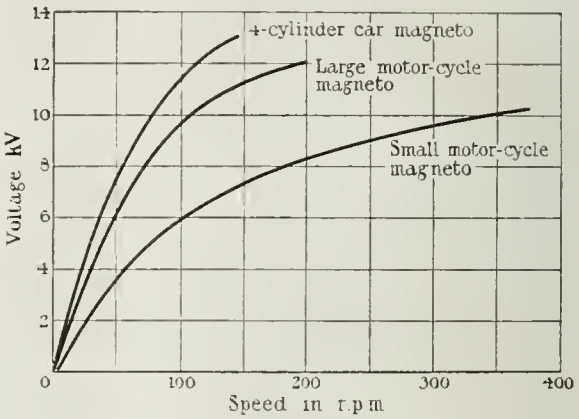

FIG. 1.—Performance of various magnetos at low speeds.

The standard testing speed for aeroplane magnetos during the latter portion of the war was 3 500 r.p.m., and most firms have retained this, at all events for magnetos running at engine speed or above. It is very rarely indeed that a four-cylinder petrol motor exceeds this, so that the modern magneto has little difficulty in complying with requirements in this direction.

Adverse conditions due to leakage are less easily defined and less easily estimated in a quantitative manner. Such leakage may be due to various causes, as for example, moisture deposited in a film on the surfaces of plugs, or on the insulation of the magneto itself ; dirt on the insulation of the magneto, due to wear of carbon brushes or similar causes ; defects in cables and wiring ; carbon deposit, charred oil, etc., on the interior of the sparking plug ; and leakage through the body of the insulator of the plug itself. All but the last of these are effective at starting when everything is cold and when the magneto running slowly produces a comparatively feeble spark. The last is generally only evident when the insulation of the plug has been heated, and depends very greatly upon the material used for the insulation. Some porcelains show a well-marked leakage at quite low temperatures. Mica, on the other hand, shows

practically nothing up to 500° C. With satisfactory sparking plugs the last may be left out of account, as a temperature sufficient to render the insulation conducting will only be reached on a very badly designed engine or on one which is being run under incorrect conditions of cooling. The temperature which will cause serious leakage in a good plug will certainly be sufficient to cause trouble to develop through pre-ignition, before any failure would arise due to electrical leakage.

Capability of a Magneto for Sparking a Leaky Circuit.

This will clearly be to some extent a function of the energy which the magneto can supply in the spark discharge, but in speaking of this it is necessary to try to get a clear idea of what is meant exactly by the term "energy of the discharge." As we shall see later, the spark discharge consists of two portions, first a capacity spark in which the energy stored in the capacity of the leads, terminals, etc., discharges practically instantaneously across the gap, and following upon this a flame-like spark or arc, the duration of which is controlled by the electrical constants of the machine.

The total energy in the discharge may be measured by means of various different types of calorimetric methods, a number of which have been evolved by different experimenters, notably, Mr. J. D. Morgan, Dr. G. E. Bairsto, and the National Physical Laboratory.

Another method has been designed by the author, in which the magneto is connected to a large condenser through a valve or small spark-gap which prevents the condenser discharging backwards into the magneto. The condenser is shunted by means of a high-resistance leak, and the energy may be obtained either by means of an electrostatic voltmeter across the terminals of the condenser, or by a thermal milli-ammeter in series with the resistance. In the former case the power input is V^2/R, and in the latter it is I^2R. Knowing the speed of the machine, the spark energy can be readily calculated. Many measurements have been taken by this method, and useful results obtained, but the figure which it gives does not strictly represent the capability of a magneto for dealing with a leaky circuit. Clearly this is more a function of the maximum current which the magneto can supply than of the total spark energy, the condition being that the machine shall furnish the leakage current of the system at the same time that it charges all the terminal capacity up to the sparking voltage. A machine giving a small current lasting for a long time will be much less suitable for this purpose than one giving a heavy current for a shorter period, even though the total energy might be greater.

Direct measurement of the current which the magneto can yield is difficult, owing to the transient nature of the discharge and the small values of the currents involved. Rough approximations can be made by means of an oscillograph and also by means of a Joubert disc, but the simplest and most satisfactory method of comparing different machines is by shunting their terminals with a high resistance in parallel with a spark-gap. The value of the resistance necessary to stop sparking is then employed as a criterion of the capability of the machine for dealing with a leaky circuit. Such a test is generally called a "utility test," and the conductance or resistance required to quench the spark is sometimes referred to as the "utility" of the magneto. The only practical difficulty in carrying out this test is to find a high resistance of the necessary value (0·2 to 1 megohm) which will satisfactorily take the discharge of the magneto and remain constant in value while at the same time being free from self-induction, portable and fairly cheap to manufacture. A liquid resistance is fairly satisfactory for laboratory purposes where it can be readily and repeatedly standardized, but is not sufficiently robust for works' use. Carbon rod resistances are quite useless owing to their high temperature coefficient, and wire resistances are very expensive and difficult to obtain truly non-inductive.

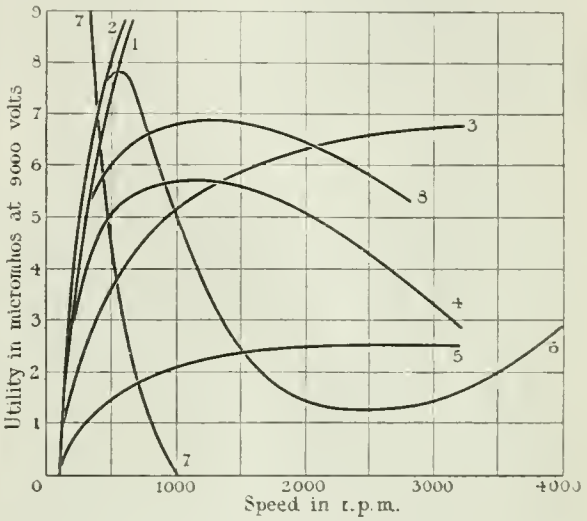

Fig. 2.—Comparative utility curves.

The author has obtained very satisfactory results by using a step-down transformer working in conjunction with a resistance on the low-tension side. The transformer is a small shell type made of a magneto armature core fitting in a laminated iron ring, and the coupling of the circuits is fairly close. The resistance is made variable in order to extend the range over which measurements can be taken. While such an instrument is not absolutely accurate under all conditions, and is not completely independent of wave-form owing to the fact that the high- and low-tension windings are not perfectly coupled, it is found to give quite consistent results when comparing different types of machines and particularly when comparing different machines of the same type.

Fig. 2 gives a series of utility figures obtained on various types of magnetos, some of which were in use prior to the war and some of which have been developed recently. It will be noticed that there is a very great diversity between these, the utility of some being very much in excess of that of others. The majority of

these gave satisfactory service, showing that in many cases the machine had a very large margin to spare over what was actually required of it. The machine shown in curve 6 actually did give trouble in service. In this case the low utility at high speeds was due to a defect in the design, which was overcome, and the machine then gave the results shown on curve 1, after which no further trouble was experienced. The machine represented by curve 5 also gave trouble, but this was due as much to bad mechanical design and poor workmanship as to its electrical properties. It was finally abandoned in favour of machine No. 8. Curve 7 is for a battery ignition set and, it will be noted, is just the reverse of that given by the magnetos. The utility here is a maximum at the lowest speed and falls off rapidly as the speed increases. Battery sets are always good starters, but usually fail to function at very high engine speeds, unless very great care is taken in their design. At the present time there is no generally accepted standard of utility which a magneto must fulfil. The author's firm have adopted the figure of 4 micromhos, and all high-speed testing gaps are shunted with a leak of this value. No case has yet been observed of a machine which passes this test giving trouble on an engine.

Principles of Operation.

The best method of treating a magneto is to consider it as a high-tension alternator in which a special artifice is employed to obtain a rapid movement of the flux relative to the armature winding at one particular instant, while keeping the speed of rotation of the armature down to a low figure. The same artifice also serves to make the rate of movement of the flux to a certain extent independent of the speed at which the armature is revolving. This artifice consists in the employment of a winding, generally known as the primary, which is short-circuited during a portion of the revolution. Currents are induced in this short-circuited winding which tend to prevent any change in the flux linking it, and which consequently produce distortion of the magnetic field. At a definite moment the primary circuit is interrupted by the contact breaker and, as the distorting current then ceases, the magnetic flux rapidly returns to its original position. In so doing it cuts both the primary and secondary windings, the latter of which is closely linked with the former by being wound upon the same core. The rate of movement of the flux back to its original position can be made very rapid, provided that the circuit is well laminated, and in consequence a high voltage is induced in the windings which it cuts. In a few magnetos the primary and secondary windings are not coupled together by being wound upon the same core but are coupled electrically, there being in this case three windings, one on the armature of the magneto, of relatively few turns of thick wire, a similar one wound on a separate small transformer incorporated with the machine, and a high-tension winding wound on this transformer and linked with it. The impulse generated in the armature winding is in this case transformed up by the transformer to the voltage necessary to produce a spark.

In order to secure a sparkless break, a condenser is connected across the points of the contact breaker. The precise function of the condenser will be considered later.

The Magnetic System as a Source of Available Energy.

If we consider the cycle of operations of such a machine we shall see that it may be divided into two principal parts as follows:—

(*a*) A period during which work is done upon the magnetic field in distorting it, and energy is stored therein.

(*b*) A period during which this stored energy is returned to the circuit as a spark.

In attempting to predetermine the performance of a magneto it will be first of all necessary to inquire into the relation between the properties of a permanent magnet and the energy which can be stored in the field furnished by it. There are several possible arrangements of the magnetic circuit, all of which are in common use. The most notable at the present time are (1) the ordinary revolving H armature, (2) the sleeve type, (3) the polar inductor type.

In (1), an H armature carrying primary and secondary windings rotates between the poles of a permanent magnet. In (2), a revolving sleeve rotates coaxially with a stationary H armature, producing a reversal of the flux as the armature rotates. Class (3) comprises a variety of machines, in all of which a revolving inductor controls the passage of flux through a fixed armature circuit.

The first class necessarily gives one complete flux reversal to each revolution. The second gives two complete reversals, while the third gives any number which may be desired, within certain limits.

The majority of magnetos belong to the first class, although some of the very earliest ones were in the second, and a large number of magnetos of this type by the Bosch, British Thomson-Houston Co. and other firms have been put into service. The third class is chiefly exemplified by the American Dixie magneto, followed later by the B.T.H. type AV machine, while other examples are found in the American Berkshire magneto and in several new forms of aeroplane magnetos which have recently been developed by the B.T.H. Company, and Messrs. Thomson-Bennett, Ltd.

Although these machines differ greatly in construction, the principle is the same in all, and is found in its simplest form in the ordinary H-armature machine of class (1). It is this machine then to which we shall in the first instance confine our attention. Such a machine may be regarded as a two-pole alternator, with a concentrated winding carried in one large slot. If such a machine be run on open circuit, a wave-form is obtained similar to that in Fig. 3, having a very high maximum value for a relatively small R.M.S. voltage. It will be seen at once that the treatment of such a wave by the ordinary laws governing alternating currents will be very difficult, and it is still further complicated by the fact that the inductance of the

rotating armature is not constant, but varies greatly in different positions owing to the large size of the single slot and tooth compared with the dimensions of the pole-piece. We shall therefore make no attempt to follow the ordinary laws of alternating current, but will deal with the machine from first principles.

To begin with, let us imagine the H armature lying with its axis across the pole-pieces in the position of minimum reluctance. Let us take this position as our zero position corresponding to an angle of 0°. We have first of all to determine the flux which the magnet is capable of driving through the armature when in this position. It is not proposed in this paper to deal *in extenso* with the theory of permanent magnets, but in order to make our reasoning complete a few words are necessary.

The relation between the flux B and the magnetizing force H for any piece of steel can be expressed by means of the well-known BH or hysteresis loop, but in the case of a permanent magnet only a small section of this loop is of interest. This section, which we will call the demagnetization curve, is the portion lying between the points $B = B_{rem.}$ and $H = H_c$, i.e. it is the section of the loop traversed when the flux in the magnet is brought down by means of a

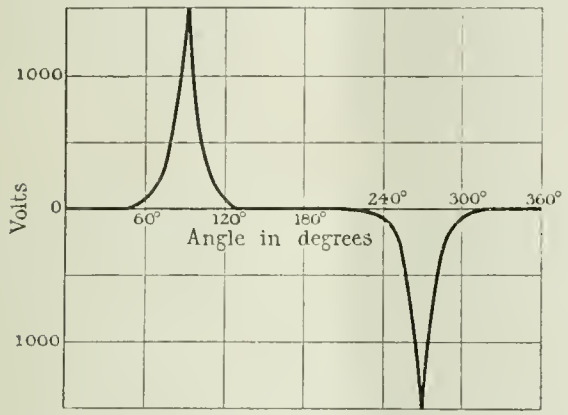

FIG. 3.—Open-circuit voltage in magneto secondary.

negative magnetizing force from that corresponding to the remanence, to zero.

The flux which the magnet is capable of producing in the armature of the magneto when in the zero position may be found by the construction shown in Fig. 4, in which is plotted to the same scale a curve representing the magnetomotive force required to drive various fluxes through the armature and air-gap. Since the ordinary BH curve expresses the relation between flux and magnetizing force, i.e. magnetomotive force per centimetre of magnet length, whereas in the case of the armature reluctance we are dealing with the total magnetomotive force required, we must either multiply the figures for H in the magnet by the mean length between the pole-shoes, or divide the M.M.F. figure for the armature by the same amount. The latter method will be found convenient and will be followed throughout this paper. Similarly, the actual flux in the machine will in every case be divided by the cross-section of the magnet so as to be reduced to a flux density instead of a total flux. Energy figures thus obtained will therefore require multiplying by the useful volume of the magnet in order to give the actual energy available in the machine.

Referring again to Fig. 4 we see that the two curves intersect at a point P, and that P represents the flux which will be produced by a freshly magnetized magnet in the armature when in its zero position. Now let us suppose the armature to rotate from this position. In order to store up energy in the magneto field the flux passing through the armature must be distorted, i.e. the magnetomotive force associated with it must be increased. This is done by closing the primary or constraining circuit so that any flux change in the armature core will produce a current which by Lenz's law will oppose this change. If the primary winding

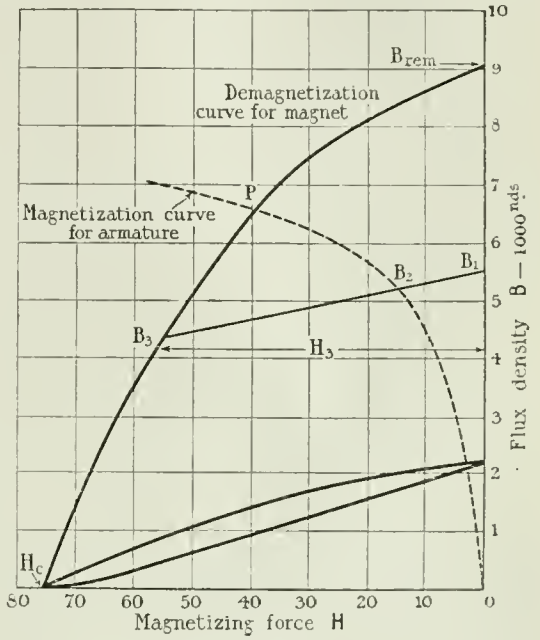

FIG. 4.—Construction for working flux in magneto.

were of zero resistance this current could be produced and maintained without any flux change, so that the whole of the flux linking by primary in the zero position would be distorted and dragged forward with the rotating armature without any diminution. Let us for the present assume this to be possible, and see what will happen to the magnet.

As the armature rotates, the reluctance of the path traversed by the flux will increase and, since the flux is constant, the magnetomotive force will increase also, i.e. the value of H, the demagnetizing force acting on the magnet, will increase, and since the magnet is incapable of maintaining the same B with an increased H, the point P must move to the left along the curve, and the flux given by the magnet must diminish. The flux in the armature itself cannot change, as the armature core is linked with a short-circuited winding of zero resistance, but the flux in the magnet will do so, or, in other words, a certain number of lines of force will collapse in the magnet itself, thereby diminishing the

number of lines distorted by the armature and reducing the corresponding M.M.F. until a steady condition is obtained, when the value of H is sufficient to supply the necessary B. Let us suppose the point B_3 in Fig. 4 represents the condition when the armature is in its position of maximum reluctance, so that B_3 represents the maximum demagnetizing force which the magneto will have to deal with, and let us suppose that at this point the constraining circuit is broken and the armature again moved to its position of zero reluctance. We have traced the reduction in flux from the point P to the point B_3 under the influence of the M.M.F. required to distort the field. Will the flux now that this M.M.F. is removed return to the point P, will it remain at B_3, or will it take some intermediate value ?

Experiments on this point show that, if starting at the point B_3 we carry the demagnetizing force H through any small cycle, provided the original value of H is never exceeded, the value of B will follow a small loop less steeply inclined to the H axis than the original one, and lying wholly within it. The area enclosed by this loop is very small, and for values of H between the point B_3 and the B axis the loop may be represented by a straight line B_3B_1 in Fig. 4. Between these points and for any value of H up to the point B_3, the magnet may be said to be truly elastic, but if H exceeds this value a magnetic yield will take place and the point B_3 will move further down the curve. The line B_3B_1 then represents the changes which will take place as the armature rotates. When the maximum demagnetizing force has once been applied it will be seen that the original flux given by P will never again be reached, but that the flux in the zero position will now be given by B_2, the point of intersection of the subsidiary loop B_3B_1 and the curve for armature reluctance.

The steady flux in the magneto after the magnet has settled down will therefore be dependent upon both the properties of the magnet and the armature reaction to which it is subjected, so that the flux can be controlled by varying the armature reaction, i.e. the proportions of the armature and pole-tips, point of interruption of the primary circuit, and the amount of copper in the armature winding. It is not flux which we are primarily concerned with, however, but available energy stored in the magnetic field. We must therefore find the conditions which will make this energy a maximum.

As the armature rotates from the zero position to that at which the primary circuit is broken, we have for any short interval of time

Mechanical work done = Energy added to magnetic field + electrical work done

The electrical work done is simply the I^2R loss in the short-circuited winding, plus any iron loss due to flux-changes in the armature or pole-shoes. Let us for the present neglect the latter and assume a winding of zero resistance, so that there is no I^2R loss. In this case all the mechanical work done is used in distorting the field, and there is no change in the flux linkage of the armature as it rotates. If we examine the magnetic circuit for various positions of the rotating armature, we see that the chief portion in which energy is stored is that between the tips of the armature and pole-shoes, and, assuming that there is no loss of lines in the armature core, and no collapse of lines in the magnet itself, the total number of lines of force stretched out across this portion of the field will be equal to twice the working flux. Now the energy added in any short period is equal to

$$\frac{1}{4\pi} H \,.\, dB$$

So that the total energy added to the field is

$$\frac{1}{4\pi}\int_0^{2B_1} H \,.\, dB$$

H will in general be a complicated function of B, for the reason that the reluctance of the magnetic field will vary as the armature rotates during the cycle we are considering. It will not, however, affect the final conditions if we assume the armature locked in the condition of maximum flux distortion, and this distortion produced by passing current through the armature from an external source. Clearly, provided the same condition of the field is ultimately reached, the way it is arrived at can have no bearing upon the energy which is stored.

Denoting the reluctance of the distorted field in this terminal position by K, we have

$$K = \frac{H_3}{2B_1}$$

where H_3 is the value of H corresponding to the maximum distortion, and B_1 is the working flux.

Assume the armature held fixed, i.e. K constant, and current passed through the primary winding, then at any moment $H = KB$, so that we have

$$\text{Energy} = \frac{K}{4\pi}\int_0^{2B_1} BdB = \frac{K}{8\pi}(2B_1)^2 = \frac{H_3B_1}{4\pi}$$

The energy stored in the distorted field is therefore equal to $1/(4\pi)$ times the area enclosed by the rectangle obtained by drawing perpendiculars from the point on the curve corresponding to H_3 to the two axes, and the maximum energy will be stored when H_3 is so chosen as to give an optimum value to this quantity.

A graphical construction will help to make our reasoning clearer and also enable us to consider conditions rather more complicated than the simple one we have just dealt with.

In Fig. 5 the point A corresponds to the maximum demagnetizing force H_3. AD represents a subsidiary loop passing through A and meeting the B axis in D. AE is perpendicular to OB, and AN to OH.

Then the initial flux present in the armature in the zero position is given by OD, and the total flux which would be stretched out if no change took place in either the magnet or armature core will be 2OD.

Draw OG = OD, then of the total flux DG which would be stretched out if no change took place in the magnet, the flux represented by ED is forced back

into it by the demagnetizing force AE, and the flux actually stretched across the air-gap is given by EG.

Let us assume the reluctance of the armature in the zero position to be zero. Then flux EG is added to the air-gap at a value of H increasing from zero to a maximum given by ON, so that the energy stored in the air-gap is given by $1/(4\pi)$ times the area of the triangle AEG. Similarly, the energy stored in the magnet is given by the area of the triangle AED, so that the total

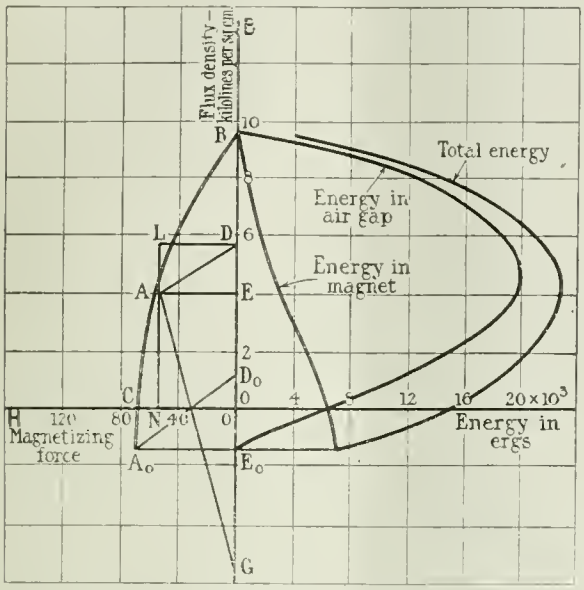

FIG. 5.—Construction for available energy.

energy stored is given by the area of the triangle ADG which, since OD is equal to OG, is equal to the rectangle DONL.

Now let us follow the changes in the energy stored in both these places as the point A moves along the curve BC. When A coincides with B there will be no flux distortion and storage of energy either in the magnet or in the air-gap, and the conditions would correspond to a magneto to which the poles of the magnet were bridged by a heavy keeper of no reluctance.

When the point A takes up a position A_0 such that $OD_0 = OE_0$, E and G will coincide and there will be no flux storage in the air-gap. This would correspond to the hypothetical case of the magneto being operated in a medium of zero permeability. All the flux energy stored, represented by the area $A_0D_0E_0$, will be in the magnet itself. Clearly there will be some intermediate point for which the total stored energy represented by the area of the rectangle DONL is a maximum, and in Fig. 5 are drawn two curves, one for the energy stored in the air-gap, the other for that stored in the magnet and air-gap combined. It will be seen that one curve has a maximum for $A = 4\,800B$, while the other has its maximum value at about 4 200. Theoretically, the energy available is given by the greater of the two curves, i.e. by the total energy stored in the air-gap and magnet, but it is doubtful whether that stored in the magnet is of very much use, as it cannot be given up to the circuit sufficiently rapidly, due to the eddy currents which would necessarily be produced in the steel of which the magnet is composed.

Fig. 6 shows the actual flux-changes taking place in the magnet of a magneto when running, and it will be seen that the flux-change lags greatly behind the point of cessation of primary current, showing the retarding effect of the eddy currents which are produced. We may therefore take the area of the triangle AEG as representing the energy available for producing a spark, and this is very nearly equal to the area of the rectangle AEON, or to the product of B and H at the point A. We see then that our aim should be to make this quantity as large as possible, and this may be done in two ways, firstly by choosing the proportions of the machine so that A occurs at the optimum point, and secondly by choosing the magnet so as to give the largest maximum value of BH for this optimum value. The value of BH and the curve connecting BH and

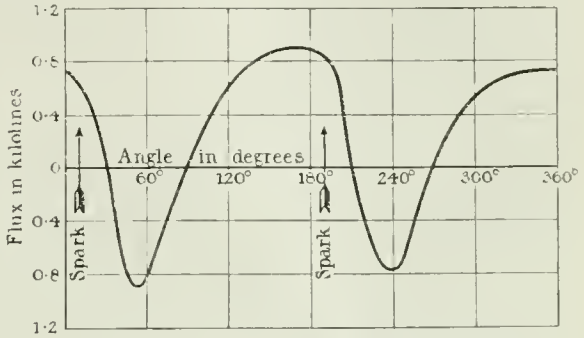

FIG. 6.—Flux-changes in arch of magnet.

H are therefore important criteria in the choice of a permanent magnet.

MAGNETIC DATA FOR VARIOUS STEELS.

Until a year ago the range of magnetic properties of available steels was somewhat restricted. Tungsten steels were available, containing about 6 per cent of tungsten with about 0·6 per cent carbon. With these steels a figure for $(BH)_{max.}$ of from 220 000 to 300 000 was obtainable, the general run of ordinary commercial magnets being approximately 260 000. By varying the heat treatment, this value of $(BH)_{max.}$ could be associated with either a comparatively high $B_{rem.}$, 10 000 to 11 000, and a low H_c of 55 to 60, or with a lower $B_{rem.}$, about 9 000 to 9 500, and a higher H_c of 65 to 75. Either magnet could be made to give equally good results, provided it was used in a machine which was properly proportioned to work at the point of $(BH)_{max.}$ Some of the existing designs naturally suited one class of magnet more than the other, and for this reason some firms preferred the one class of magnet while some favoured the other.

In addition to the tungsten steels great use is made in America of chromium steels which, although not quite equal to tungsten, give results considerably better than the plain carbon steels. A good chromium-steel magnet will give a $(BH)_{max.}$ of 240 000 to 250 000, against a corresponding figure for tungsten of 280 000 to 300 000, while a plain carbon steel will not usually exceed

200 000. These steels were also used in Germany during the war, when tungsten was unobtainable in that country.

During the last 12 months, however, the range of steels available has been enormously extended by the introduction of the new cobalt steels. From these steels, which contain cobalt alloyed with iron in various proportions in addition to other elements such as tungsten, chromium, etc., a very wide range of figures can be obtained. With 35 per cent of cobalt, values of $(BH)_{max.}$ as high as 900 000 can be reached, the value of $B_{rem.}$ being from 10 000 to 10 500 with a value of H_c of about 200 to 240. Such a steel is naturally expensive owing to the high price of cobalt, but on the other hand a very much smaller amount of it is required, and the higher coercive force opens up great possibilities in the way of simplification of design, owing to the reduction in magnet length permitting straight bars to be used in place of the horseshoe magnets hitherto employed. These steels have not yet been available for a sufficient length of time to allow their influence on design to become apparent, but there is little doubt that they will lead to very considerable developments during the next few years.

Knowing the energy content of the magnet steel to be employed, we can, if we know the energy output required of the machine and the efficiency of utilization of the available energy, arrive at an estimate of the total amount of magnetized steel which must be provided. The energy output required from the machine is, however, somewhat indefinite. We have already seen that the utility of the machine is by no means proportionate to the energy output, and the present trend of design is to maintain or to increase the utility while reducing the energy output. Hence the size of magnets provided in machines of a given performance has been considerably reduced. We do not propose, therefore, to state any definite figures of energy output but merely to give data which represent present practice with machines of modern design.

Table 1.

Class of Service	Magnetic Contents $(BH)_{max.}$ Volume
Heavy commercial vehicles ..	45 to 70 × 10^6
Light and medium cars	35 to 45 × 10^6
Heavy motor cycles	30 to 45 × 10^6
Light motor cycles	20 to 30 × 10^6

Proportions of Magnet.

The preceding data allow us to form an estimate of the total volume of the magnet, but do not indicate what proportions it should have. The magnet might, for instance, be of very large section capable of giving a large flux but only short in length, or on the other hand its cross-section might be small and its length great. In the former case the working flux in the machine will be large, but the amount of distortion to which it is subjected must, owing to the short length of the magnet, be small, as otherwise the magnet will be working beyond the point of $(BH)_{max.}$. Such a magneto will have a long armature of large cross-section and will consequently use more iron and copper than a machine with a smaller working flux distorted to a greater extent; carrying this to excess will result in a long magnet which will occupy too much space, and difficulty will, in the limiting case, be found in producing sufficient distortion of the flux to reach the point of $(BH)_{max.}$. In this connection it must be remembered that the normal working of the magneto is not the worst condition which the magnet may be called upon to meet. Specially severe conditions may be introduced by:—

(1) The armature being permanently short-circuited when running at a high speed. This is a condition which is frequently met with. It may be due to: (*a*) short-circuiting of the sparking plugs by charred oil, carbon deposit, etc., or (*b*) the normal operation of the cut-out which is used for stopping the engine. The usual way of carrying this out is to short-circuit the primary winding of the machine. This is usually done after the engine has been slowed down, but may occasionally happen at a high speed. It should be noted that the effect of short-circuiting is practically instantaneous. A few seconds' running under these conditions has far more effect on the magnet than long periods of normal service.

(2) The armature being removed from the tunnel without the use of any keeper. This is a condition always liable to be met with when the machine is dismantled for examination or overhaul. It is a matter of fairly common knowledge that, if the armature is withdrawn from the magnets, a keeper should be placed across the poles, but few people realize that the keeper must be placed across the poles before the armature is withdrawn. To place it across afterwards is quite useless.

Both these conditions really amount to very much the same thing, and it may be laid down as a condition to be fulfilled by the design that the removal of the armature shall not take the magnet appreciably beyond the point of $(BH)_{max.}$. This then gives us a relation between the gap between the poles of a magnet, the magnetic constants of the material and the length of the magnet employed. As the magnetic constants will vary with the material employed, it is useful to express the proportions of the magnet in terms of the magnetic potential between its extremities. This is zero when the magnet is short-circuited, and should never exceed the value corresponding to the optimum value of BH. The value of this quantity ranges from about 600 for very small motor-cycle magnetos, to 1 200 for machines intended for lorry and similar work. Ordinary tungsten steels have the optimum value of BH at about $H = 40$, corresponding to a length of from 15 to 30 cm.

Effect of Armature Reluctance.

In the preceding calculation the effect of armature and air-gap reluctance has been neglected with a view to simplifying the problem. It will be clear that the

general effect of both armature and air-gap reluctance will be to reduce the available amount of stored energy; for the energy available will be that stored in the field when distorted, less that stored in the field after its return to its normal position. This latter is stored partly in the armature core and partly in the air-gap between it and the pole-pieces. Both these must therefore be reduced to the least value possible. For this reason, therefore, the air-gap is generally kept at a low figure, usually about 0·1 to 0·15 mm. measured radially. The universal employment of ball-bearings for the armature spindle facilitates the maintenance of such a small gap. The armature reluctance can be kept low by using material of high permeability, and taking care not to work at too high a density, but here a compromise is necessary in that if the density chosen is too low the size of the core and the length of the primary winding are increased, thus reducing the constraint of the primary winding upon the flux, and reducing the amount of flux distortion at low speeds. The usual density in most machines is from 12 000 to 15 000 lines per square centimetre, and the corresponding magnetic potential between the pole-faces when the armature is in its zero position is approximately 100 C.G.S. units, or, say, from 8 to 16 per cent of the value corresponding to storage of maximum energy.

Effect of Armature Resistance.

The effect of armature reluctance may be relatively small and will only show itself by a reduction of output below that which is theoretically possible, but the effect of armature resistance cannot be neglected and has a very important bearing upon the operation of the machine, more especially at low speeds. If the resistance of the armature were zero we should expect the spark energy to be constant at all speeds, as the amount of flux distortion would then be independent of the speed. In a machine built for variable timing, i.e. in which the point at which the primary current was broken could be varied in respect to the pole-tips, one should expect the maximum spark energy corresponding to a fully retarded spark, i.e. to the position of maximum distortion. Actually, at low speeds especially, this is not the case, owing again to the effect of the primary resistance. The effect of the primary resistance is to cause a flux-change to occur in the armature core as it rotates in the act of distorting the field. This flux-change induces in the primary winding a small E.M.F. which maintains the current producing the distortion. The E.M.F. necessary for a given flux distortion will be independent of the speed and of the time elapsed since the beginning of the cycle, and so will therefore be the rate of the flux-change. Consequently, for low speeds of rotation and long cycles the total flux-change will be very much less than for short ones, so that the flux actually distorted at the point of break will diminish as the speed is reduced, and with this flux diminution there will be a corresponding diminution in the stored energy. Consequently the spark energy will rise as the speed increases up to a maximum value at which the amount of flux leaking back is small compared with the total. This maximum value is generally obtained at about 2 000 r.p.m., and is clearly shown in Fig. 7, which gives curves taken for representative machines.

A method of calculating the amount of flux distortion at any speed and armature position is given in the Appendix. The conclusions reached are that in order to get the best performances at low speeds without any sacrifice in other directions, the cross-section of the primary winding must be as great as possible and its length of mean turn a minimum. It is shown also that it is impossible to design a machine which will give a good performance at low speeds over a large angle and will still give ample spark energy in all positions at high speeds without exceeding the optimum value of *BH* for the magnet. Put briefly, it may be stated that the reluctance of the flux path when in a position of maximum distortion (fully retarded) must be such that the M.M.F. corresponding to the passage

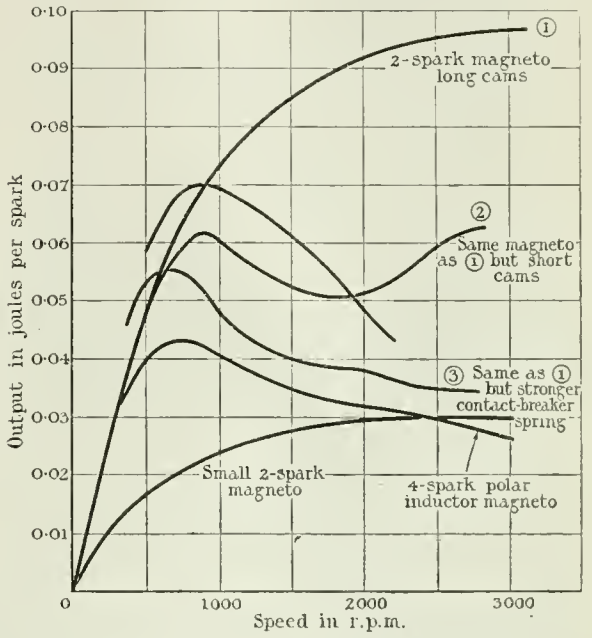

Fig. 7.—Typical magneto spark-energy curves.

of the full working flux across it must not exceed that equivalent to H_3. In the "fully advanced" position the flux distortion will be much less, and the energy less also. The ideal for high speeds would be for the flux to be suddenly distorted to its maximum amount and then left in this condition for the remainder of the range of operation of the machine. This would (assuming it were possible) give a uniform performance at high speeds, but it would give a very poor performance at low speeds in the retarded position. If, on the other hand, a uniform performance at low speeds is aimed at by making the reluctance of the flux path increase with the angle so as to make the potential remain approximately constant, the increase in reluctance compensating for the reduction of flux due to armature resistance, then at high speeds the magnet will be overstrained in the retarded position unless the flux distortion in the advanced position is kept small.

Since these effects are the direct outcome of the resistance of the primary winding, it is clearly essential that the cross-section of this should be as large as possible compatible with providing sufficient space for the secondary turns. For this reason also, machines employing a separate transformer have an important advantage in that the whole of the space on the armature can be filled with short-circuited winding.

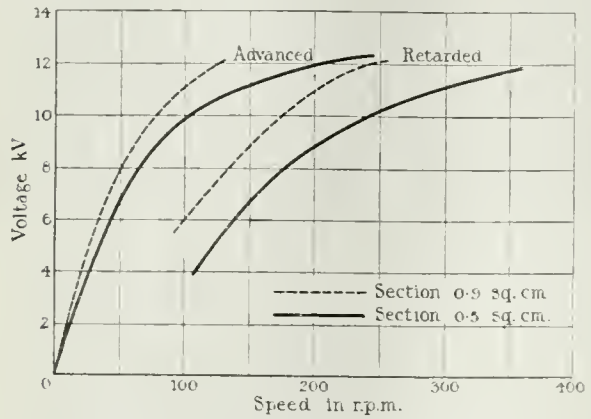

FIG. 8.—Effect of two different sizes of copper cross-section in primary winding. Standard pole-tips.

Figs. 8 and 9 show the way in which the performance of a magneto at low speeds varies with changes in the copper cross-section of the primary winding, and also with the shape of the pole-shoes. The curves in Fig. 8 are for pole-shoes of normal shape, while in the curves in Fig. 9 the pole-shoes have been extended so as to give a more uniform performance. Fig. 10 shows the

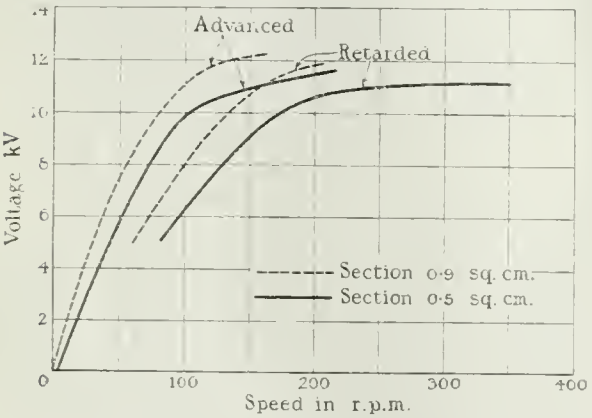

FIG. 9.—Effect of two different sizes of copper cross-section in primary winding. Extended pole-tips.

way in which the position of maximum utility varies with the speed and angular position of the armature. At low speeds the maximum value occurs very near the mid-position of the armature. At high speeds its position moves round in the direction of rotation.

In order to obtain a variable point of ignition, a better arrangement on theoretical grounds is to fix the position of the armature with relation to the pole-shoes at which the spark occurs, and obtain any necessary variations in timing by altering the angular relation of the magneto to the engine. This may be done by several forms of special coupling, the underlying principle being a member joining the driving and driven shafts and carrying two keyways, one straight and the other inclined. Axial movement of the member causes a change in angular relation of the shafts. Another device consists in swinging the whole magneto about its axis, while in others the magnet and pole-shoes are swung. A device largely used by both English and German machines consists in the employment of a sleeve which carries the pole-shoes and which can be rotated, the magnet itself remaining fixed. This device gives very good results, but is somewhat expensive. In a further development this sleeve is geared to the

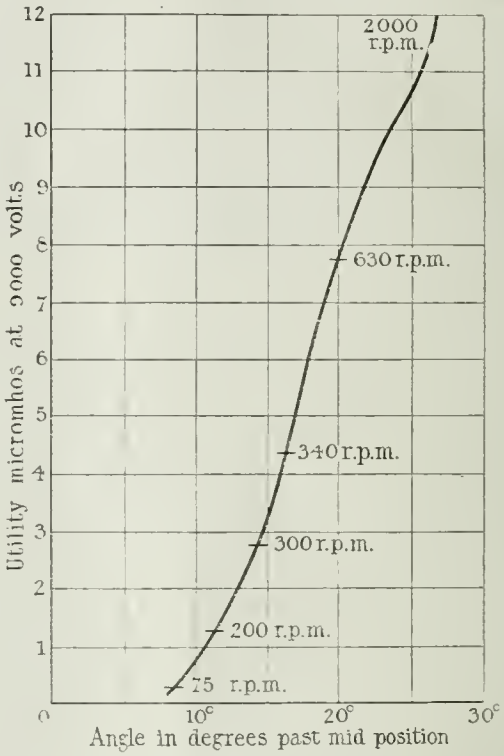

FIG. 10.—Showing variation in position of maximum utility with speed of machine.

control lever in such a way that, as the magneto is retarded, the pole-shoes approach the armature tips and recede from them when advanced. This gives a very good characteristic in that it gives the best performance at low speeds when fully retarded, thereby making starting considerably easier. It has not so far been adopted on any large scale, possibly on the score of complication and expense.

Another common device to improve the performance of the machine at low speeds is the impulse starter coupling. There are very many of these in use, but they all utilize the principle of a spring connection between the magneto and its driving shaft. At low speeds a catch arrests the motion of the armature, and the shaft in rotating winds up a spring and, after rotating

through a definite angle, trips a catch permitting this spring to rotate the armature rapidly. There is thus a two-fold action. First, the speed of rotation of the armature is sufficiently great to give a satisfactory spark at the lowest engine speeds; and secondly, there is an automatic retardation of the ignition corresponding to the angle through which the driving shaft is rotated before the catch is tripped. At high speeds the catch is thrown out of operation, and the magneto and its driving shaft rotate uniformly together. In some types the disengaging of the catch is done by hand, while in others it is automatically effected by centrifugal action.

Transformation of Magnetic to Electrical Energy.

Having traced the storage of energy in the stretched magnetic field, let us consider the transformation of this energy into that represented by a current flowing in the high-tension circuit. If we neglect for the moment all losses in the iron of the armature core, pole-shoes and magnet, and neglect also any current which may flow either into the condenser fitted in the primary circuit or into the capacity of the leads and terminals of the high-tension circuit, and if, further, the rupture of the primary current were absolutely instantaneous, then the flux reversal in the core, upon the breaking of the primary current, would also be instantaneous. This would mean that, for a given flux-change, an infinite voltage would be produced. Actually, however, the voltage is limited by that required to spark across the plug, and, in the conditions we have assumed, an indefinitely small flux-change would be sufficient to produce this voltage. Once the spark has passed and a current is established in the spark-gap and high-tension circuit, the flux-changes in the system will be subject to the same laws as those we have already considered in the case of the primary, with the exception that the resistance in circuit will not be that of the secondary winding only, but of the secondary winding plus the spark-gap. This latter will not in general be constant, but will depend upon the current passing, falling with an increase of current, and vice versa. In such a machine there will therefore be no sudden flux-change and no discontinuity in the magnetic conditions of the circuit, but only a sudden transference of current from the primary to the secondary winding.

In an actual machine the conditions are different. In the first place, the presence of eddy currents in the armature core and pole-shoes retards the movement of the flux, and therefore lessens the secondary voltage which a given flux-change can produce. In the second place, there is capacity associated with both primary and secondary windings; in the former in the shape of the condenser, which is necessary to suppress the sparking at the contact points; in the latter in the form of the self-capacity of the windings and that of the leads and terminals. Further, the primary and secondary windings, each with its own associated capacity, form a pair of coupled circuits which interact, so that the calculation of the flux-change necessary to produce a given voltage is very difficult indeed.

It is important to make the necessary flux-change as small as possible, for it is upon this that the performance of the machine at low speeds primarily depends. At speeds of the order of 100 r.p.m. the flux available is only a comparatively small fraction of the total amount given by the magnet, and the best use must be made of this fraction. The most obvious point calling for attention is the correct lamination of the magnetic circuit. If we examine the changes in the flux path at the moment of break, we see that there is a flux reversal in the armature core accompanied by a flux swing in the pole-shoes and that portion of the magnet adjacent to them. It has always been the practice to laminate the armature core carefully, but until recently the pole-shoes have been constructed of cast iron. Now, however, the majority of British firms and one or two American ones are fitting laminated pole-

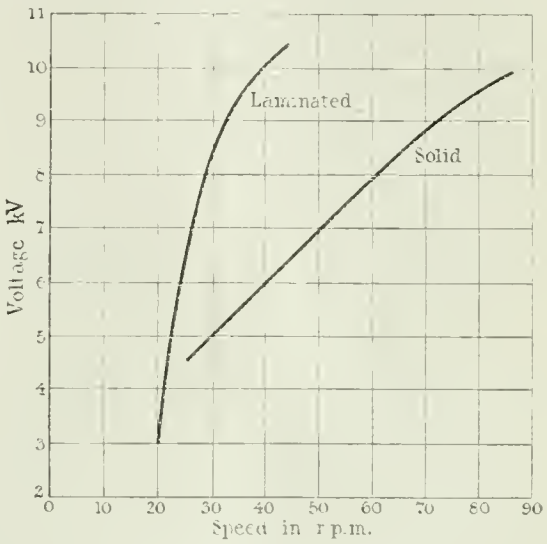

Fig. 11.—Comparative low-speed performance with solid and laminated pole-shoes.

shoes to their new models, and there is no doubt of the improvement which is thus effected. Fig. 11 gives comparative tests made on the same machine with solid and laminated pole-shoes, and shows clearly the advantage to be gained by their use. Fig. 12 shows the actual flux-change occurring in a solid pole-shoe, and Fig. 13 in a laminated one, and it will be noted that there is a much larger flux-change with this and, moreover, that it follows much more closely after the interruption of the primary circuit, thereby giving a higher secondary voltage with less lag in operation. On the same curve is shown also the flux-change taking place in the limbs of the magnet. This, as might be expected, is so small and sluggish as to be negligible, and it is clearly advisable to make the pole-shoes themselves of sufficient thickness to carry the working flux without the saturation density of the material being exceeded.

The chief reason why laminated pole-shoes have not been used to a greater extent in the past is probably the difficulty of making a satisfactory mechanical job of them with the methods of construction employed. In

all the Bosch machines the tunnel in which the armature rotates consisted of three separate pieces, a gunmetal base and two cast-iron pole-shoes held together by screws and dowels. The end-covers closing the two ends of this and carrying the bearings were separate castings spigoted on to this tunnel and secured by screws tapped into the pole-shoes and base. The pole-shoes were, in addition, tapped to receive the screws fixing the magnet, so that they really formed quite an important structural part.

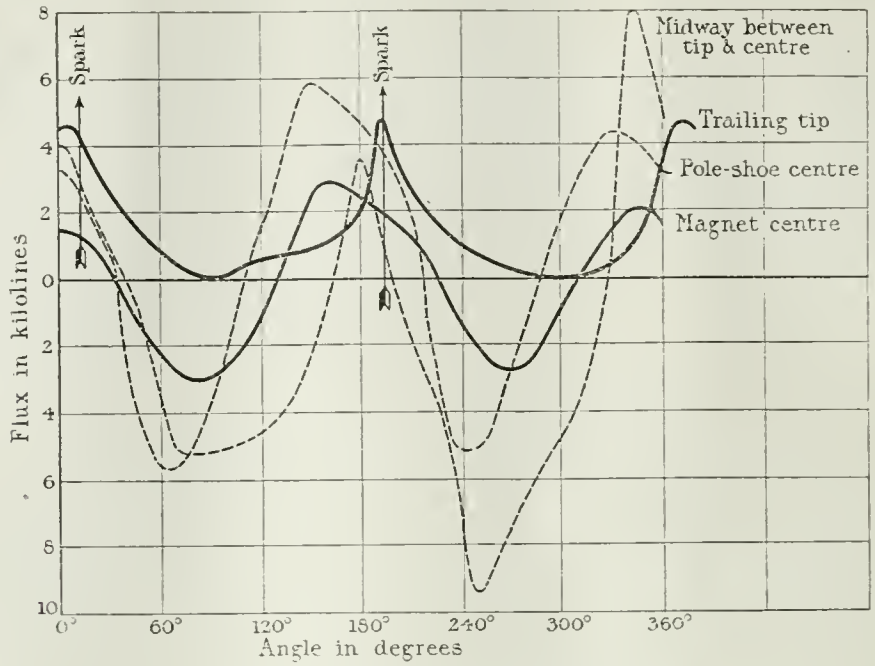

FIG. 12.—Flux-changes in solid pole-shoes.

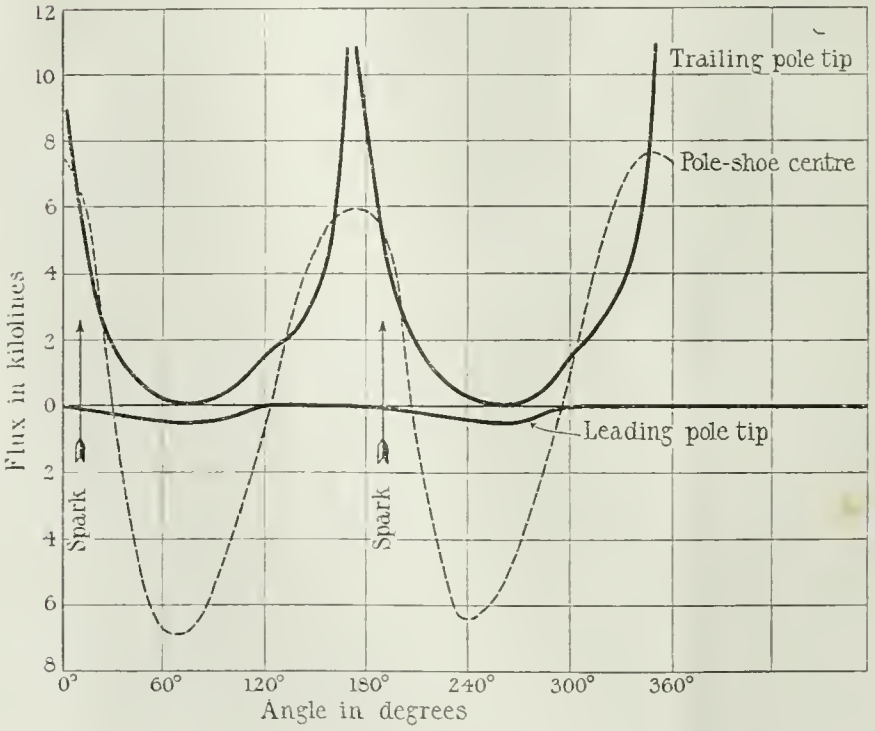

FIG. 13.—Flux-changes in pole-shoes. Laminated field.

Modern British and American practice is to make the tunnel in one piece and integral with one of the end covers, the pole-shoes being no longer a structural part, but inserted into the main frame casting, either by casting in or by inserting in a slot milled to receive them. The main frame generally consists of aluminium or, in some of the cheaper types, of a zinc base alloy similar to type metal. Both these metals permit of casting in chills or dies, and pole-shoes either solid or laminated can be inserted in the dies and incorporated in the finished casting. By adopting the laminated construction a saving in weight of from 25 to 30 per cent can generally be effected without any deterioration in the performance of the machine.

Effect of Condenser.

We may regard the condenser in two ways, first, purely as a device for suppressing sparking at the contact-breaker points, and second, as forming one element of an oscillatory circuit inductively coupled with the secondary. The earlier studies of the action of the condenser were based entirely on the first viewpoint, but latterly Professor Taylor Jones in a series of very complete researches accompanied by a masterly mathematical investigation has shown that a complete theory must take into account also the existence of oscillations in the circuit comprising the condenser and primary winding, and their interaction with the secondary system. We will, however, first of all consider the function of the condenser solely as a suppressor of sparking at the contact points, and will later see to what extent the conclusions thus arrived at are likely to be modified as the result of Professor T. Jones's work.

If we imagine the rupture of the primary circuit to be instantaneous, then, if no capacity is associated with either the primary or secondary windings, the maximum voltage developed will be attained without the lapse of any measurable period of time, i.e. the rate of voltage-rise will be infinite. Any capacity in either the primary or secondary circuit will reduce this and make the rate of rise finite, energy being transferred from the electromagnetic field to an electrostatic one in the form of an oscillatory current. The rate of voltage-rise will therefore be determined by the constants of this oscillatory circuit, and will be steeper the less the value of the capacity and the higher the frequency of the oscillations.

Now, in practice the rupture of the primary circuit can never be instantaneous, as the contact points can only separate at a finite rate, depending upon the speed of the machine and the design of the contact breaker and its operating cams, and there must therefore be a definite relation between the sparking voltage between the two points and the time that has elapsed since they commenced to separate. We should expect that, if this were at any moment less than the voltage due to the oscillations which accompany the break, sparking would occur.

It is, however, difficult to make any estimate of the condenser capacity required for the suppression of sparking, for the reason that the relation of spark voltage to distance of separation can never be laid down for the conditions prevailing. These are not the same as for a pair of points placed at a small distance apart, but are more of the nature of those governing the formation of an arc. Were they the conditions of true sparking, we should expect that the capacity required to render a machine sparkless would depend upon the rate of separation of the points, i.e. upon the contour of the cams. Such, however, is not the case, a machine being in general just as sparkless with a gently graded cam as with a very abrupt one, giving a very sudden opening. Moreover, the rate of pressure-rise and maximum voltage developed appear to be independent of the cam shape, so that the mechanical conditions of break do not appear to be of great importance. Again, data available on the pressure required to produce short sparks indicate a voltage of about 350 volts for a gap of 0·1 mm., rising somewhat as the gap is reduced to 0·001 mm., and then falling again. On this basis, provided the voltage had not attained a higher figure than 350 volts during the first 0·001 mm. travel of the points, sparking would not occur, whereas it is found that the capacity must be much more than corresponds to this rate of rise in order to suppress sparking.

Sparking should be independent of the material of which the contact points are made, whereas it is well known that only certain metals, such as platinum and iridio-platinum, which do not form oxides when heated, can be employed, and that even with these metals the sparking is largely affected by the condition of the points themselves, and can be greatly increased by the presence of small traces of dirt and impurities.

The problem, therefore, is not so much the suppression of true sparking as that of the suppression of arcing, and it is probable that the rate of movement of the points once they commence to separate is of secondary importance, the function of the condenser being to prevent local heating of the points which would tend to vaporize the metal and make conditions favourable for the production of an arc. Such local heating would be accompanied by a large value of the IR drop, and this can be prevented by a condenser of suitable capacity. It could also be reduced by keeping I as small as possible, i.e. by using a large number of turns in the primary winding, and, as a matter of experience, this is actually the case, the sparking at the points being diminished with an increase in the number of primary turns beyond the value at present customary. If the number of turns be made sufficiently large, true sparking sets in of an appearance quite different from that of the arcing usually known by this name. At present, primary windings in which the number of turns is sufficient to give these conditions are not employed, as the voltages to which the insulation of the condenser and contact breaker would be subjected are considerable.

Generally speaking, it may be said that the conditions existing at the moment of break are but little understood and that a carefully conducted research into them would be well worth while, especially if it should lead to the successful employment of some other material in the place of platinum, the price of which is continually advancing with apparently small hope of increased supplies.

If we consider the condenser merely as a spark suppressor, it is clear that the smaller the capacity which

can be employed the higher will be the secondary voltage for a given primary winding and a given flux-change, the voltage increasing with a diminution of condenser capacity down to that value at which sparking commences. At this point the interruption of the primary current is no longer perfect, energy is dissipated at the break and irregular functioning of the machine occurs.

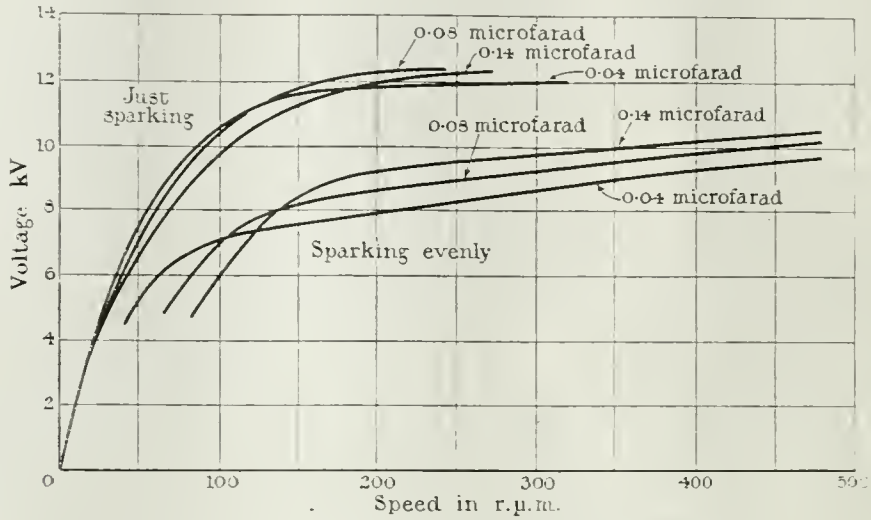

FIG. 14.—Effect of condenser capacity on low-speed voltage curves.

Fig. 14 shows the way in which the secondary voltage of a given machine changes with variation in condenser capacity. As might be expected, the minimum capacity which can be employed is smaller the less the energy which has to be handled.

Fig. 15 shows the intensity of sparking at the points in terms of the condenser capacity, the sparking being

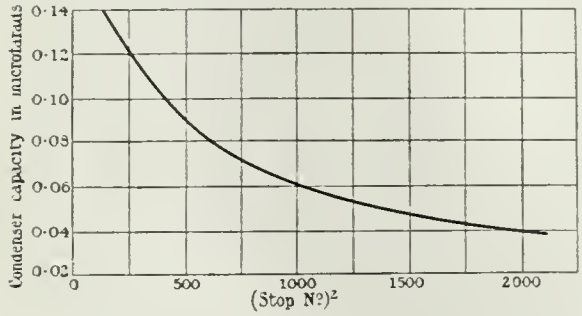

FIG. 15.—Effect of varied condenser capacity on sparking at contact points.

determined by observing the image of the contact breaker on the ground glass of a camera, and finding the size of iris diaphragm which would just render it invisible.

Here it will be seen that the sparking falls steadily as the condenser capacity is increased. Comparing the figures with those given on Fig. 14 we note that the capacity required to render a machine quite sparkless is considerably greater than that which gives the highest voltage for a given low speed, and hence we should expect the capacity fitted to be a compromise between that necessary to give absolute suppression of sparking and that which gives the best low-speed performance. The capacity fitted may depend upon the duty required from the machine; for example, a magneto which never required to run at very high speeds, but in which good low-speed performance was essential, might have a smaller condenser than one which had to operate at very high speeds.

Professor Taylor Jones's treatment of the magneto shows that the voltage at the secondary terminals when the machine is running on open circuit, i.e. without any spark occurring, will be given by a wave composed of two superimposed oscillations, a long-period one and a shorter-period one. The magnitude and frequency of these will depend upon the inductances and capacities associated with the primary and secondary circuits, and upon the coupling between them. As the damping in a magneto is always very large, only the first half-wave is of importance, and the best results will be obtained when the maximum of the low- and high-frequency oscillations coincide. Consequently, the condenser capacity should be chosen to conform as nearly as possible to this condition, provided of course that such a capacity is not likely to produce sparking at the points.

In nearly all magnetos the primary and secondary windings are very intimately linked together, the insulation between them being, from space considerations, made a minimum. Consequently, the coupling is normally very close, and the corresponding value of primary capacity which will give the maximum secondary voltage is in general much less than that required to suppress sparking. Moreover, the theory shows that with such close coupling the optimum values of primary capacity are not clearly marked, and that the choosing of such a capacity can only correspond to an improvement of a few per cent. In order to gain any real

advantage from the correct choice of primary capacity, it is necessary to make the coupling much looser, and theoretically in this case a considerable improvement in secondary voltage can be obtained by choosing the optimum primary capacity. In addition, this capacity becomes more of the value which is required in order to suppress sparking.

There are, however, two objections to such a course. In the first place, the difficulty of providing for the decreased coupling without increasing the size of the machine or decreasing its magnetic efficiency, and in the second, the fact that the secondary voltage will depend very largely upon the secondary capacity and its correct relation to the other constants of the machine. Consequently, a slight change in the length of high-tension leads or in the type or dimensions of the sparking plugs fitted would affect the performance of the magneto and would need a corresponding adjustment of the primary capacity to be made. Again, the damping plays such an important part in the performance of all magnetos, even with the best laminated magnetic circuits, that the predictions based on any theory ignoring damping cannot be fulfilled, and actual measurements made on existing machines scarcely seem to indicate that, at present at all events, it is possible to make any very practical application of Professor Jones's work. Possibly, future changes in design and methods of construction will permit of better use being made of his very complete mathematical analysis, and enable machines to be constructed in accordance with the principles therein laid down.

The reasoning we have just applied does not necessarily imply the passage of a spark in the secondary circuit, but has only dealt with the voltage waves set up in the windings. Depending upon the characteristics of the spark-gap and the machine, there will be a speed at which the flux-change at break will be sufficient to raise the capacity of the secondary circuit to the breakdown potential of the gap. Provided the flux-change is only just sufficient to enable this voltage to be attained, the whole of the electromagnetic energy will be converted to electrostatic energy prior to discharge, and the spark will be a so-called capacity spark only. Such a spark is, in general, oscillatory, of extremely high frequency and short duration, as the circuit in which it flows is of little or no inductance. Consequently, very large currents may exist in it, although only for a brief period of time. In appearance it is a bright white line, the intensity of which depends upon the capacity across the gap and the amount of energy stored in it. Owing to the very high frequency, no mechanical means will enable us to resolve it into its individual oscillations. It is undoubtedly this first white spark which produces ignition of the charge.

As the speed of the machine rises and the energy available increases, the conversion of electromagnetic to electrostatic energy prior to the passage of the spark is no longer complete. Consequently, after the passage of the initial spark, we are left with a certain proportion of the working flux still distorted and a further quantity of energy available. This energy is now communicated to the secondary circuit, but under different conditions, as the passage of the initial spark has left the spark-gap in a highly conducting condition. The discharge will, in fact, follow the law governing the decay of current in an inductive circuit, i.e.

$$-n_2 \frac{dN}{dt} = I_2(R_2 + R_s)$$

where n_2 = number of secondary turns,
N = total flux distorted,
R_2 = the secondary resistance,
R_s = the spark resistance,

$$4\pi n_2 I_2 = NK$$

and

$$K = f(\theta)$$

It is not possible to make further use of this relation, however, without a knowledge of R_s, the spark resistance, which is not a constant but varies with the value of I_2, the secondary current, and also probably with time. Generally, R_s will fall as the current increases and will rise rapidly when I_2 falls below a certain lower limit, so that the current curve tends to follow approximately a logarithmic law but with the latter portion cut off, i.e. the discharge ends sharply when the current has died down to a certain value, instead of gradually dying out.

Further, this law can hold only so long as the resistance of the spark-gap is sufficiently low to keep the potential across it small compared with its initial breakdown voltage. So long as this is the case, the electrostatic energy stored in the terminal capacity is negligible compared with the electromagnetic energy. If, however, owing to high gap resistance or other causes, the electrostatic energy becomes sufficiently large in comparison with the electromagnetic, the discharge may become discontinuous and consist of two, three, or more bright sparks separated by distinct intervals, the terminal capacity being repeatedly charged and discharged.

Such a condition may easily be produced artificially by using a well-cooled spark-gap and adding capacity in parallel with it, but it probably rarely occurs under actual working conditions.

The nature of the spark discharge can be studied very easily by the device of observing the image of the spark in a mirror rotated by the magneto. If this is done it will be seen that the spark generally consists of a bright line followed by a luminous band. This band is generally not quite uniform but shows a number of transverse markings, which appear to indicate that the discharge is not quite steady and does not follow a smooth curve, but that the current curve is slightly rippled. These ripples appear to be due to the action of the primary condenser, as the pitch of the markings is changed if this be altered. The discharge is, however, unidirectional, as examination of the electrodes will show, one electrode being surrounded by a glow which remains upon it during the whole of the discharge. That the luminosity is due to incandescence of the gas through which the spark is passing, may be shown by directing a blast of air upon it, when the flame may be stretched out a distance of several millimetres.

Another way of examining the structure of the spark consists in causing it to take place between two points rotating around a centre or between a revolving point and a concentric ring. Such an arrangement, known generally as a rotary spark-gap, has been made considerable use of in the study of the conditions governing the discharge.

Although the flame which follows the initial spark is valueless from the point of view of ignition, its study is of considerable importance. It is particularly important that at all speeds the flaming portion of the spark shall have died out and that the secondary current shall have ceased before the primary circuit has been closed again. Unless this is the case, the flux in the armature core will not have been able to re-form, and consequently the energy available in the succeeding spark will be correspondingly reduced.

The complete extinction of the spark is controlled by the following factors:—

(1) The time available, i.e. the speed of rotation and length of cam.
(2) The resistance of the spark circuit.
(3) The inductance of the secondary winding, i.e. the stiffness of the magnetic field and the number of secondary turns.

To make extinction as complete as possible we therefore require to use as long a cam as possible, keeping the contact points open for the greatest possible angle consistent with the provision of the short-circuited winding necessary for producing field distortion. The second factor is not under our control to any great extent, but it should be noted that the additional spark-gaps sometimes inserted in the secondary circuit and provided in all jump-spark distributors serve the purpose of increasing the total resistance of the spark-gap circuit and thereby reducing the spark duration.

There are undoubtedly certain conditions of operation in which a jump-spark distributor will give more satisfactory ignition than one in which the distributor rotor makes actual metallic connection. It is sometimes stated that this is due to the gap permitting a voltage to be built up unhindered by any leakage across the plug, and permitting energy to be stored and suddenly discharged through the ignition circuit.

This, however, is hardly the correct explanation, as the length of gap required to be effective is much greater than is ever used in actual service. The true function of the spark-gap, in so far as it effects an improvement, is in its power of securing extinction of the spark before the primary circuit is closed again. It is particularly effective when the insulation resistance of the plug is low, due to carbon deposit or other causes or when one of the gaps is very closely set. In such a case it may easily happen that the resistance of the spark-gap circuit is insufficient to produce extinction of the spark in the time available, when the succeeding spark will be weakened by the reduction in flux which can be distorted by the armature. Consequently, missfiring will occur, not necessarily upon the leaky plug itself, but on the one next to it in succession.

The third factor can be controlled to a considerable extent by modifying the shape of the pole-tips fitted to the machine, and also to a slight extent by changing the number of secondary turns. A lower limit to this is usually set by the low-speed performance which is required from the machines, and this number should not be exceeded. A greater number of turns than is required to enable the machine to give a good low-speed

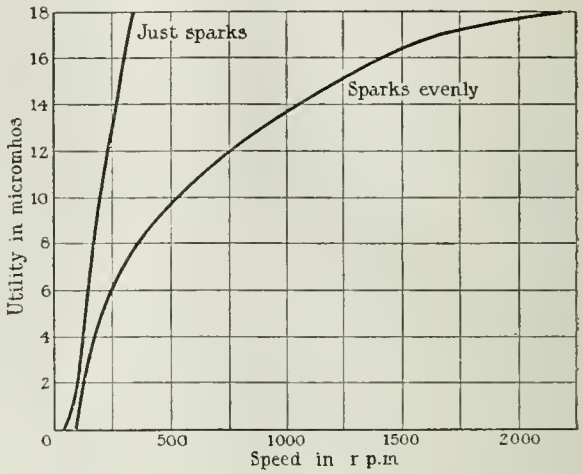

FIG. 16.—Utility test on long-cam magneto.

performance is detrimental from all points of view. The shape of the pole-tips should clearly be that which will give the maximum reluctance to the path traversed by the distorted field, hence extended pole-tips or other similar devices employed in order to give a wide range of control will tend to give a long drawn-out spark and

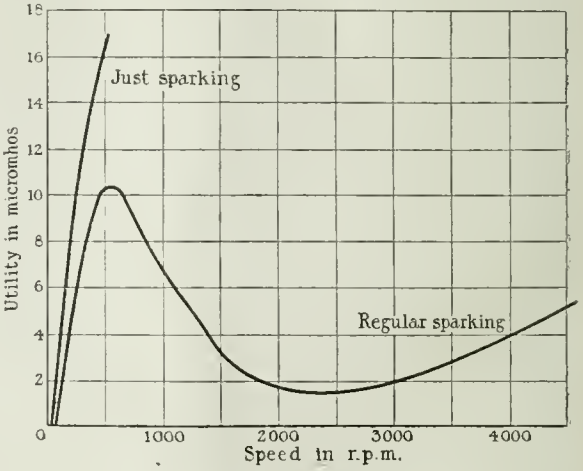

FIG. 17.—Utility test on short-cam magneto.

possible trouble at high speeds. On the other hand, the shape of the tips and reluctance of the flux path must for the best conditions be such as to make the most effective use of the magnet by working at the point of maximum *BH*.

It will be clear that to obtain a satisfactory compromise between all the factors involved is a matter of some difficulty, but in general it may be said that the

following principles will form a basis for the correct relationship between them :—

(*a*) The number of secondary turns to be the least which will satisfy low-speed conditions.

(*b*) The reluctance of the flux path to be such that under the worst conditions of high speed and maximum flux distortion the point of maximum *BH* is not exceeded.

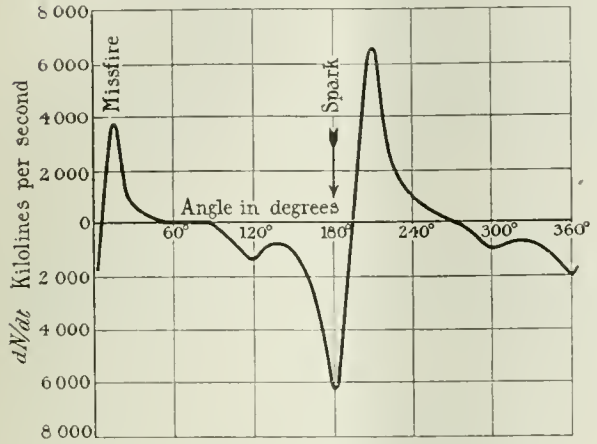

FIG. 18.—Flux-changes in pole-tips. Magneto missing alternate sparks due to incomplete extinction.

(*c*) The duration of open circuit of the primary winding to be as large as possible, i.e. the points should be held open from the sparking position until the axis of the H armature is across the pole-faces or slightly beyond this position. This will be found in most machines to correspond to a duration of "make" of 100° and of

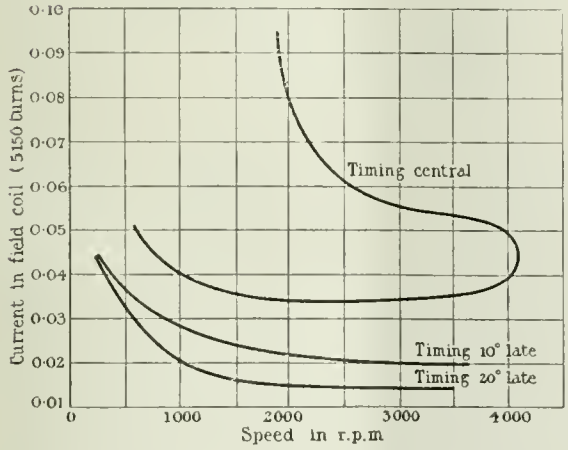

FIG. 19.—Effect of incomplete spark extinction.

"break" of 80°, the break occurring at approximately 10° past the mid-position of the armature. These angles have been standardized by all the British magneto manufacturers with the object of ensuring uniformity and interchangeability of contact breakers on different machines.

Curves 1 and 2 in Fig. 7 show clearly the effect of incomplete spark extinction in reducing the output on a magneto. The two curves are for the same machine, fitted in one case with a long cam opening for 80°, in the other for a short cam opening for only 12°. The two curves it will be seen are coincident at low speeds, and separate as the speed rises, the curve for short-cam machines falling below the other one. At very high speeds the short-cam curve tends upwards again, owing to the inertia of the contact-breaker arm causing it to leave the cam and give the effect of an increased cam length.

Figs. 16 and 17 show the "utility" figures for a similar pair of machines. Here it will be noticed that the just-sparking figures for the two machines agree,

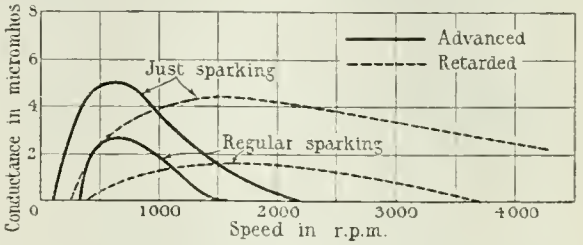

FIG. 20.—Four-cylinder carbon-brush distribution. Short cam ; 3 terminals earthed.

but that the regular sparking curve for the short-cam machine falls below the other one. Naturally, with a short cam there will be occasional cycles when the full working flux is available, as a missed spark will be accompanied by a period when both primary and

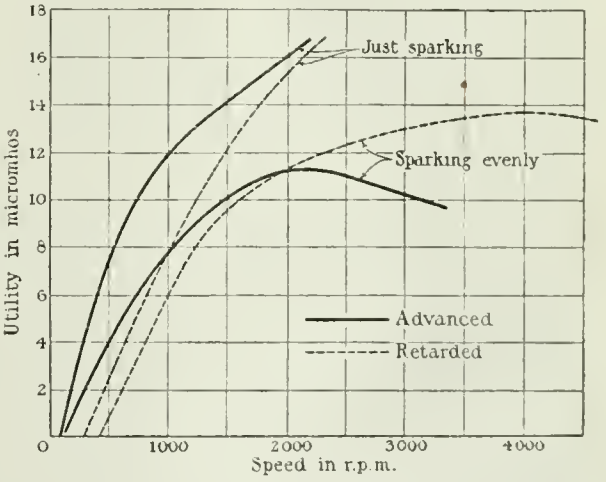

FIG. 21.—Four-cylinder jump-spark magneto, with 3 terminals earthed.

secondary circuits are open and when the flux can properly re-form. A short-cam machine will in general tend to miss alternate sparks, and it can frequently be made to give either positive or negative sparks at will.

Fig. 18 shows the flux-changes taking place in the pole-tips of a machine running under such conditions,

missing alternate sparks. The difference in the flux-change for the two sparking positions is very noticeable.

Fig. 19 shows the results of some tests made on a machine which was liable to missfire on alternate gaps, and in which an electromagnet was fitted in place of the ordinary permanent magnet. It will be seen that, for certain positions of the timing, regular firing was possible only over a certain small range of field strength. An increase of field beyond this caused missfiring due to the lingering of the spark with the lower resistance brought about by the increased current flowing. It will be concluded from this that putting a better magnet in a magneto may not always be a cure for missfiring but may even make it worse.

Figs. 20 and 21 show comparative tests made on a magneto with short cams and carbon-brush distributor, and one with long cams and a jump-spark distributor. In this case three of the distributor terminals were earthed to the frame, and the sparking was observed on the unearthed one. It will be seen to what an extent the failure to extinguish one spark is able to influence the spark which follows it.

Summary of Relation Between Electrical and Magnetic Characteristics.

Having outlined the successive stages in the cycle of operations we may summarize the results of our reasoning as follows :—

(1) *Low-speed voltage.*—Dependent upon available magnetic energy, primary resistance, speed of rotation, number of secondary turns, energy losses and condenser capacity. Increases with increasing speed up to a limiting value, which increases with increase in available magnetic energy.

(2) *Utility.*—At high speeds under normal working conditions when the potential required to spark the plug is small compared with that which the magneto is capable of giving on open circuit, the utility is proportional to magnetic potential of distorted field and inversely proportional to number of secondary turns. At low speeds the utility is dependent upon speed and primary resistance.

(3) *Energy available for initial spark.*—By this is meant the energy which could be discharged in the form of a capacity spark provided the circuit conditions will allow it. It is the energy stored in the condenser placed across the terminals of the machine when the capacity is raised to the maximum which still permits sparking, or, conversely, it is the energy stored in the capacity of the leads, etc., when the spark potential is increased to the limit which the magneto is capable of giving. It is dependent upon the stored magnetic energy which is available at the moment of break, upon the losses in the iron of the armature, etc., and to a certain extent upon the condenser capacity. If losses were absent the energy would be independent of the voltage, but, owing to their presence, the energy decreases as the voltage is raised. As the energy is dependent upon the magnetic energy stored it will be dependent upon the armature resistance and speed of rotation, rising to a maximum as the speed of rotation is increased.

(4) *Total energy in spark and flame.*—This follows the same law as the energy in the initial spark, but is greater in value. Theoretically the total energy may be twice that in the initial spark. This energy we have seen is approximately that stored in a magnetic field of value $2B$ at a potential of H and is therefore $2BHl/(8\pi)$ or $BHl/(4\pi)$.

If now, instead of allowing this field to collapse suddenly, we do work on it, maintaining the potential H until $B = 0$, additional work $BHl/(4\pi)$ will be done upon the field, and the total available energy will be $BHl/(2\pi)$. An analogue to this is given by a charged condenser. The energy in a condenser of capacity C at a potential difference of V is equal to $\frac{1}{2}V^2C$. If, however, we join the coatings by a resistance, and separate the plates, reducing C at such a rate that V is maintained until $C = 0$ and the condenser is emptied, we have :—

$$\text{Energy} = V \times VC = V^2C$$

or twice that stored in the condenser originally.

(5) *R.M.S. value of current in secondary circuit.*—Depends upon characteristics of magneto itself and also to a very large extent upon the spark-gap resistance. A magneto may be made to give almost any reading desired upon a hot-wire ammeter, by altering the constants of the circuit. In particular, adding capacity to the terminals will increase the reading several fold.

Trend of Development in Magneto Design.

The following points always form the basis of development of any piece of mechanism :—

(1) Improvements in material available, including the use of cheaper but equally effective substitutes.
(2) Better utilization of existing material.
(3) Improved mechanical design, tending to longer life and greater reliability in service.
(4) Simplification of parts, reducing cost of construction and elimination of difficult manufacturing problems.

In some cases these points are to a certain extent incompatible, notably (2) and (4), and a compromise has to be effected ; it is in arriving at a satisfactory compromise that much of the skill of the designer lies.

In dealing with these in turn we have already noted under (1) the use of the new cobalt steels in place of tungsten steels. Another direction under this heading in which much might be done is in the production of a satisfactory substitute for platinum for the contact points. Tungsten has been tried and is used by one or two firms, but is certainly not equal to platinum, as not only is its life shorter, but its contact resistance is much higher, and the performance of the machine at low speeds suffers accordingly.

Under heading (2) we have already dealt with the use of laminated instead of solid pole-shoes. Much might be done in the direction of improved insulating materials and methods of insulation, so as to enable a better space factor to be obtained. There is room also for improvement in the magnetic circuit. Examination of the armature reaction will at once show that certain portions of the ordinary horseshoe magnet are

extremely ineffective. For instance, in the portion which is adjacent to the pole-shoe there is a tendency for the flux to be reversed in direction every cycle—a most undesirable condition to be fulfilled by magnet steel. Fig. 22, which gives the distribution of potential along a typical horseshoe magnet after running, shows to what uneven demagnetizing forces the metal is subjected, and how badly it is employed. Owing to this and other reasons, the energy given as a spark is only a small proportion of that available in the magnet. For instance, a magneto fitted with a magnet having a value for $(BH)_{max.} \times$ volume of 40×10^6, corresponding to a theoretical spark energy of $40 \times 10^6/(4\pi) = 0{\cdot}32$ joule, would not under the best conditions give more than about 0·08 joule in the initial spark, corresponding to an efficiency of about 25 per cent.

It is possible that some re-arrangement of the magnetic circuit will enable an improvement in this figure to be obtained.

Under heading (3) very great progress has been made, particularly as the result of aeroplane experience, when many existing designs otherwise good were found unsatisfactory under condition of severe stress and vibration. Particularly is this true of the contact breaker and its actuating cams. The duty required of the contact breaker on a high-speed engine is very severe, as it has to make upwards of 100 interruptions per second, and in its working life may quite well have to break the circuit 500 to 1 000 million times. Unless the workmanship of this part is very good and the parts properly proportioned, failure is almost certain to ensue. Moreover, a slight amount of wear may cause irregularity in the point of operation, and faulty running of the engine. Practically all contact breakers are operated by an internal cam formed on a steel ring. Formerly this ring was carried in a brass housing or "cam cage," but it is now usual to form the cam and cam cage in one piece of hardened and ground steel fitted in a carefully bored housing and so proportioned that there is ample bearing surface to take the blow of the contact breaker or the cam. The shape of the cam also is made such that it conforms to the natural motion of the contact breaker arm under its controlling spring, thereby lessening shock and wear. The manufacture of such a cam to the fine limits necessary is a somewhat difficult proposition.

Under heading (4) very much has been accomplished of late by the use of die castings. These are produced both in aluminium alloys and in alloys with a zinc base. The former are usually poured, but can be cast under pressure. The latter are generally pressure cast. A pressure-cast part is usually very true to size, well finished, and needs but little machining. Holes, bushes and laminations can be cast in place, and very much machining, drilling, tapping, and assembling can be saved. Their use will probably extend very largely in the near future.

Conclusion.

The author cannot but feel that the treatment of the subject has been scanty and incomplete, consisting merely of an outline of the principles underlying the operation of one type of machine only. The same reasoning will, however, apply to all the other forms of magneto which are in use, and can readily be developed to include them by anyone interested. Were an attempt made to deal in detail with all the problems involved in the many different types of magneto at present in use, it would require not a paper, nor even a book, but a series of volumes. Those interested he would refer for further information to the published papers by Dr. G. E. Bairsto, Dr. N. Campbell, Dr. N. W. Maclachlan, Mr. J. D. Morgan, Mr. C. C. Paterson, Professor Taylor Jones, Mr. A. P. Young, and many others, and also to the very exhaustive reports published by the Advisory Committee for Aeronautics, which embody a large amount of work by many of these authors.

Before concluding, the author would like to express his acknowledgments to his co-workers in the magneto

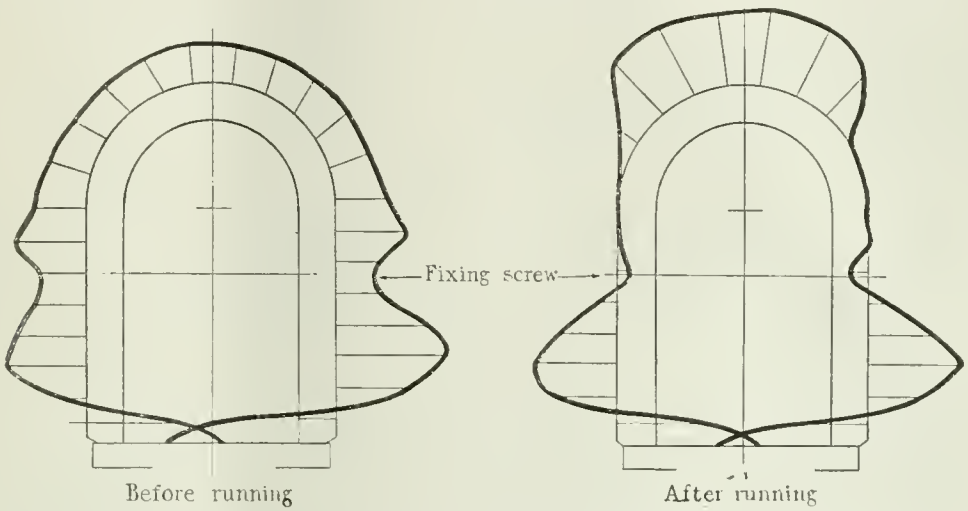

Fig. 22.—Distribution of magnetic potential in horseshoe magnet of magneto.

Note.—Ordinates perpendicular to the surface of the magnet represent to scale the M.M.F. per unit length of magnetic path.

industry, notably to Mr. Ireland of Thomson-Bennett, Ltd., Mr. J. D. Morgan of the Marks and Clerk Laboratory, and Mr. A. P. Young of the B.T.H. Company. Throughout the development of the industry there has been the closest co-operation between the technical staffs of the various magneto manufacturers. Frequent meetings have been held between them, and many of the ideas which have been worked out and put forward in this paper are the outcome of these discussions.

The author would also express his acknowledgments to his firm the M-L Magneto Syndicate, Ltd., in whose laboratory all the data here presented have been obtained, and to his colleague Mr. G. A. Lister for help and suggestions on many occasions.

APPENDIX.

Calculation of Flux in Armature.

In these calculations the following assumptions are made :—

(1) The reluctance of the armature is zero.

(2) The reluctance of the air-gap between the trailing pole-shoe and the armature is zero until the armature reaches its mid-position, and is thereafter a function of the angle turned through from this position, while the reluctance of the gap between the armature and the approaching pole-shoe becomes zero when the armature reaches its mid-position, and remains at this figure during the whole of the cycle considered. The nearest approach to this would be obtained by making the armature just bridge the gap between the pole-shoes and reducing the air-gap to the smallest possible figure. Under these conditions the storage of magnetic energy will be in the two spaces between the armature and the trailing pole-tips, and the magnetic potential of the armature faces will be the same as the pole-shoes under which they lie.

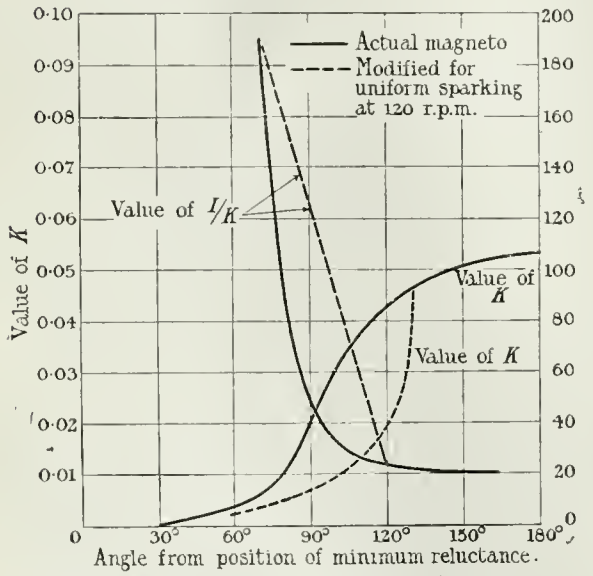

Fig. 23.—Values of k and $1/k$, at 120 r.p.m.

In the following let

N = flux traversing armature after time t,
n = number of turns in primary,
R = primary resistance,
k = reluctance of path traversed by flux,
I = current in primary winding,
l = length of mean turn of primary winding,
S = total cross-section of copper in winding.

$$4\pi nI = Nk$$

Also,
$$I = \frac{E}{R} = -\frac{n}{R}\cdot\frac{dN}{dt}$$

$$\therefore -\frac{4\pi n^2}{R}\cdot\frac{dN}{dt} = Nk$$

Writing $f(\theta)$ for k we have

$$-\frac{4\pi n^2}{R}\cdot\frac{dN}{dt} = Nf(\theta)$$

whence
$$\frac{dN}{N} = -\frac{R}{4\pi n^2}f(\theta)d\theta$$

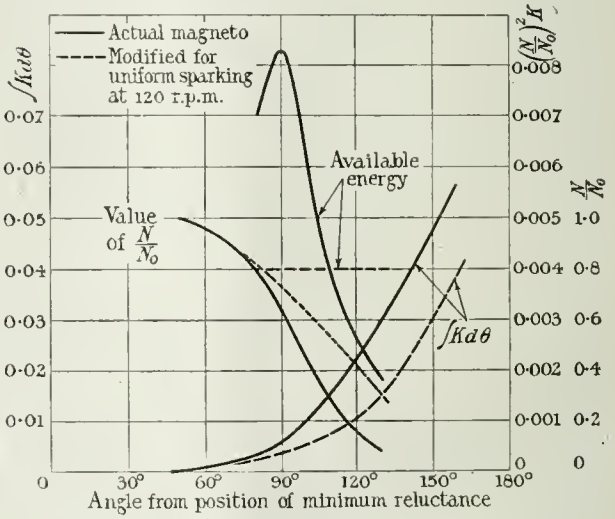

Fig. 24.—Values of $\int kd\theta$, N/N_0, and energy available, at 120 r.p.m.

If v is the speed of the machine in revs. per sec., and θ the angle turned through by the armature, we have

$$\theta = 2\pi vt$$

whence
$$\frac{d\theta}{dt} = 2\pi v$$

and
$$dt = \frac{d\theta}{2\pi v}$$

Then $$\frac{dN}{N} = -\frac{R}{8\pi^2 n^2 v} f(\theta) d\theta$$

and $$\log N = -\frac{R}{8\pi^2 n^2 v} \int f(\theta) d\theta + A$$

When $\theta = 0$, $N = N_0$, $f(\theta) = 0$, then $\log N_0 = A$

and $$\log \frac{N}{N_0} = -\frac{R}{8\pi^2 n^2 v} \int_0^\theta f(\theta) d\theta$$

Substituting for R in terms of l and S and the specific resistance of copper ($1{\cdot}7 \times 10^{-6}$), we obtain

$$\log \frac{N}{N_0} = -\frac{1{\cdot}7 \times 10^3 \times l}{8\pi^2 v S} \int_0^\theta f(\theta) d\theta$$

The condition we have to aim at is to make N, i.e. the final flux, as large as possible, that is, N/N_0 must be

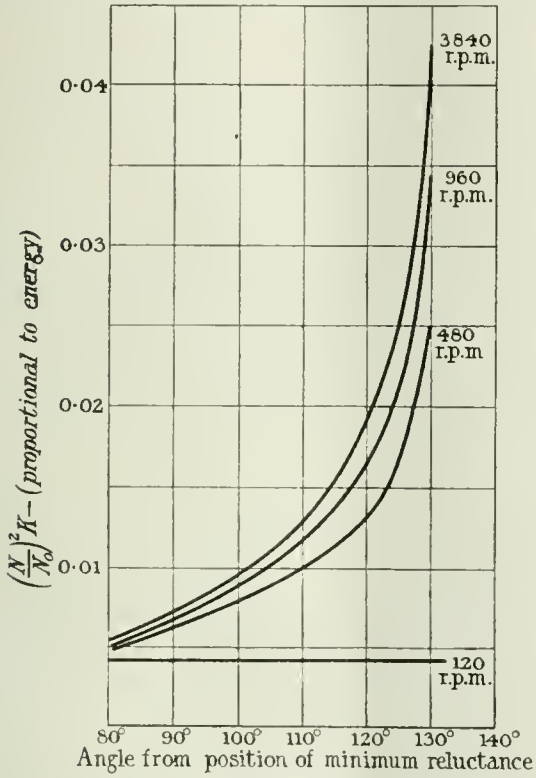

Fig. 25.—Energy curves for normal pole-tips.

as nearly as possible unity, which will make $\log (N/N_0)$ zero.

Hence to get the best results for a given v, S should be as large as possible, and l as small as possible, i.e. the mean turn must be a minimum and the cross-section a maximum.

Also, for a given value of $f(\theta)$, the value of $\int f(\theta)$ up to that point must be as small as possible, or $f(\theta)$, i.e. the reluctance, should keep small until the point under consideration is nearly attained, and then rise suddenly to its maximum value.

With a machine in which the position of the spark is never varied, i.e. one intended for fixed ignition, this condition can be approached by making the pole-tips as narrow as possible and arranging the point of break when the distance between armature and pole-tips is comparatively small, but where variable ignition is required this condition cannot be fulfilled, as our object then is to obtain uniform performance over a considerable range.

If we interpret this as meaning that the stored energy over a certain range is to be constant, we have

$$N^2 = \frac{8\pi E}{k}$$

from which $$\frac{dN}{dk} = \frac{\sqrt{(8\pi E)}}{2k^{3/2}}$$

and $$\frac{dN}{N} = -\frac{\sqrt{(8\pi E)}}{2k^{3/2}} \times \frac{k^{1/2}}{\sqrt{(8\pi E)}} dk$$

$$= -\frac{dk}{2k}$$

Substituting for $\frac{dN}{N}$

gives $$\frac{dk}{2k} = \frac{R}{8\pi^2 n^2 v} k d\theta$$

whence $$\frac{dk}{2k^2} = \frac{R}{8\pi^2 n^2 v} d\theta$$

Let $$\frac{R}{4\pi^2 n^2 v} = C$$

then $$\frac{dk}{k^2} = C d\theta$$

and $$-\frac{1}{k} = C\theta + A$$

If we write $\theta = 0$, $k = 0$, we obtain $A = -$ infinity, and the equation is of no value. The physical meaning of this is that it is not possible to obtain uniform operation over the complete range. Consequently there will of necessity be a point of discontinuity just after the armature tip leaves the pole-tip.

The other terminal condition which we may employ is that, in the extreme case when θ has its maximum value θ_0, the value of k must be such that when the magneto is operating at a high speed the maximum allowable demagnetizing force on the magnet is not exceeded. In the most extreme case the full flux N_0 will be stretched across the reluctance k_0, and the M.M.F. ($N_0 k_0$) must not give a demagnetizing force greater than H_3 (see Fig. 4). This enables us to assign a value to k_0, whence we obtain

$$-\frac{1}{k_0} = C\theta_0 + A$$

$$A = -\frac{1}{k_0} - C\theta_0$$

$$-\frac{1}{k} = C\theta - C\theta_0 - \frac{1}{k_0}$$

$$\frac{1}{k} = \frac{1}{k_0} + C(\theta_0 - \theta).$$

It will be noted that the term C is really a fraction containing the speed v in its denominator. Consequently the condition of uniform operation can only be satisfied for one particular speed. Further, the smaller v is, i.e. the lower the speed, the greater the value of C and the more rapidly must $1/k$ increase as we move backwards from the point $k = k_0$. Now in normal full-speed operation the value of N is nearly equal to N_0, and the energy available is therefore very nearly proportional to N_0k^2. If $1/k$ be increased too rapidly, the value of N_0k^2 in the advanced position, i.e. when θ has its least value, will become very small, and a condition may be reached such that the magneto,

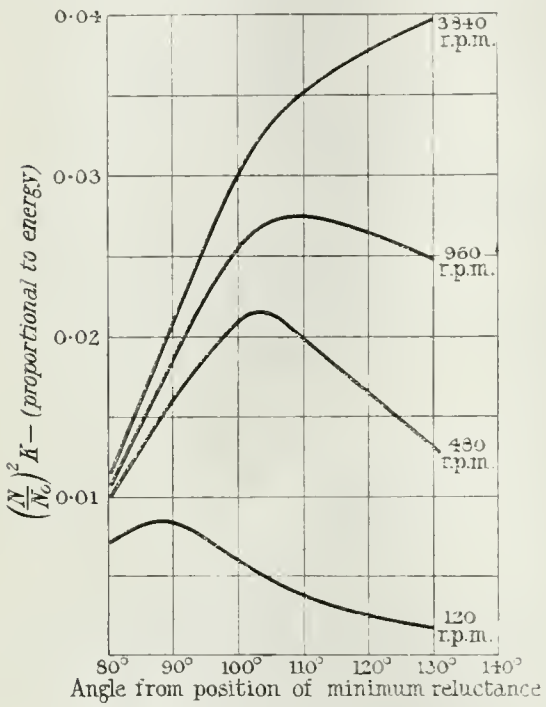

FIG. 26.—Energy curves for extended pole-tips.

although operating satisfactorily at low speeds and when retarded, is incapable of supplying sufficient energy to cope with the plug leakage when running normally.

Figs. 23 and 24 show a case which has been worked out for an actual magneto. This machine was a light aero machine with plain pole-tips, no extension of any sort being provided. The full-line curves are the data for the machine as constructed. The dotted lines show the figures which might have been expected if the machine were modified to give a uniform performance at a speed of 120 r.p.m. over a timing range extending from the mid-position of the armature to a point 40° past this position.

Fig. 23 gives the values of k and $1/k$ for the actual machine and also the modified one. Fig. 24 gives the corresponding values of $\int k d\theta$, N/N_0 and energy available, all worked out for a speed of 120 r.p.m.

Figs. 25 and 26 give a series of energy curves worked out for various speeds up to the maximum and it will be seen from a comparison of these that, although the modification has increased the available energy in the retarded position at low speeds, it has very seriously diminished it in the advanced position when running normally.

BIBLIOGRAPHY.

ARMAGNAT, H.: "Magneto Machines." *La Revue Electrique*, 1915, vol. 23, p. 321.

—— "The Magneto." *Ibid.*, 1915, vol. 23, p. 321; *Electrician*, 1915, vol. 76, p. 865.

—— "Induction Coils." [Translated by the Kenyon McGraw Publishing Company.]

BAIRSTO, G. E.: "The Effect of Shunted Resistance, or Plug Leakage, on the Sparking Performance of an Electrical System." *Journal I.E.E.*, 1920, vol. 58, p. 507.

—— "The Synchronism of Spark of a Magneto as Affected by the Method of Coupling." Advisory Committee for Aeronautics, Engine Sub-Committee, Report no. 21, 1917.

—— "The Synchronism of Spark of the Dixie Magneto." *Ibid.*, Report no. 22, 1917.

—— "The Rate of Rise of the Secondary Potential of an Ignition System." *Ibid.*, Report no. 31, 1918.

—— "The Use of the Series Gap for Intensifying the Spark of a Magneto under Leaky Plug Conditions." *Ibid.*, Report no. 17, 1917.

—— "The Relation between the Output of a Magneto, and its Size and Weight." *Ibid.*, Report no. 42, 1918.

—— "The Spark Energy of Electrical Ignition Systems." *Ibid.*, Report no. 51, 1919.

—— "The Relation between the Number of Secondary Turns on a Magneto Armature, and the Secondary Voltage with Shunted Resistances." *Ibid.*, Report no. 52, 1919.

—— "The Relation between the Number of Secondary Turns on a Magneto Armature, and the Secondary Voltage with Shunted Resistance. Measurements on a Rotating Armature Magneto." *Ibid.*, Report no. 313, 1920.

CAMPBELL, N.: "Experiments on the H.T. Magneto." *Philosophical Magazine*, 1919, vol. 37, p. 284.

—— "Notes on the Break of a Magneto or Induction Coil." *Ibid.*, 1919, vol. 37, p. 481.

—— and PATERSON, C. C.: "Some Characteristics of the Spark Discharge and its Effect on Igniting Explosive Mixtures." *Proceedings of the Physical Society*, 1919, vol. 31, p. 168.

CODD, M. A.: "Induction Coil Design." [E. and F. N. Spon.]

GEIST, C. F.: Various papers on Magnetos. *Automotive Industries*, 1918 and 1919.

HAYWARD, C. B.: "Automobile Ignition and Lighting."

HENDERSON, J. F.: "Magneto Ignition." *Proceedings of the Institution of Automobile Engineers*, 1915, vol. 9, p. 443.

JONES, E. T.: "Electrical Oscillations in Coupled Circuits." *Philosophical Magazine*, 1909, vol. 17, p. 28.
—— "Induction Coil Potentials." *Ibid.*, 1914, vol. 27, p. 565.
—— "On the Potential Gradient of High-Tension Magnetos." *Ibid.*, 1918, vol. 36, p. 145.
—— "Oscillations during the Discharge of an Induction Coil." *Electrician*, 1919, vol. 83, pp. 167 and 201.
—— "The Most Effective Adjustment of an Induction Coil." *Philosophical Magazine*, 1915, vol. 29, p. 1.
—— "The Secondary Potential of an Induction Coil at Make." *Ibid.*, 1917, vol. 33, p. 322.
—— "The Capacity-Potential Curves of an Induction Coil." *Electrician*, 1918, vol. 81, pp. 376, 396 and 416.
JONES, E. T.: "The Optimum Secondary Capacity of an Induction Coil." *Electrician*, 1919, vol. 82, p. 99.
—— "The Maximum Primary Potential in an Induction Coil." *Ibid.*, 1919, vol. 82, p. 121.
—— "The Action of Induction Coils." *Journal of the Röntgen Society*, 1920, vol. 16, p. 47.
—— and ROBERTS, D. E.: "The Induction Coil." *Philosophical Magazine*, 1911, vol. 22, p. 706.
LIBBY, A. D. T.: "Advantages of Magneto Ignition." *Journal of the Society of Automotive Engineers*, 1920, vol. 7, p. 277.
MACLACHLAN, N. W.: "An Experimental Method of Determining the Primary Current at Break in a Magneto." *Proceedings of the Physical Society*, 1919, vol. 32, p. 38.
—— "On the Energy in the Magnetic Circuit of a Magneto." *Electrician*, 1920, vol. 85, p. 128.
MORGAN, J. D.: "Choice of Electric Ignition Apparatus for Motor Engines." *Engineering*, 1919, vol. 107, p. 36.
MORGAN, J. D.: "Criteria for the Testing of Magnets." *Electrician*, 1920, vol. 84, p. 4.
—— "Electric Spark Ignition." [Crosby Lockwood and Sons, 1919.]
—— Miscellaneous papers. *Engineering*.
—— "Principles of Electric Spark Ignition in Internal Combustion Engines." [Crosby Lockwood and Sons, 1919.]
PAGÉ, V. W.: "Automobile Starting, Lighting and Ignition Systems." [N.W. Henley Publishing Company, New York.]
PATERSON, C. C. (*See* CAMPBELL, N., and PATERSON, C. C.)
ROBERTS, D. E. (*See* JONES, E. T., and ROBERTS, D. E.)
SILSBEE, F. B. "Ignition Work at the Bureau of Standards." *Automotive Industries*, 1919, vol. 40, p. 1294.
WARREN, H. (*See* YOUNG, A. P., and WARREN, H.)
WATSON, E. A.: "The Magneto, its Function, Design and Construction." Coventry Engineering Society, 1917. Published in *Automobile Engineer*, May, 1917.
YOUNG, A. P.: "Magnetos." [Iliffe and Sons, 1919.]
—— "The Magneto." Coventry Engineering Society, 1915. Published in *Automobile Engineer*, March 1915.
—— "The High Tension Magneto." Aeronautical Society of Great Britain. Reprint No. 4, 1917
—— and WARREN, H.: "The Process of Ignition." *Journal of the Institution of Automobile Engineers*, 1920, vol. 14, p. 305.
NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS (U.S.A.): "Characteristics of High Tension Magnetos." Report No. 58.

DISCUSSION BEFORE THE INSTITUTION, 14 APRIL, 1921.

Mr. C. C. Paterson : The characteristics and design of a magneto are determined not by what is necessary to produce ignition but by what is necessary in order to overcome the defects of sparking plugs and cables. The author has dealt very largely with the energy obtained from a magneto. The necessity for obtaining an appreciable amount of energy is due to the fact that the sparking plugs and cables will not remain free from leaks, and energy has to be supplied in order to overcome those leaks and to build up the necessary potential between the points of the sparking plug. Energy, according to the results which Dr. Campbell and I obtained, is a very unimportant matter in connection with the actual process of ignition. If the highest possible potential is produced at the critical moment between the points of the sparking plug, it matters very little what is the amount of energy used. The results of some tests that we made during the war on a 6-cylinder engine showed that there was absolutely no difference in the power obtained, whether the explosive mixture was fired by means of a very "fat" spark, or whether such a minute amount of energy was applied between the sparking plugs and the cylinder that the sparks were hardly visible at all to ordinary sight. The maximum current needed to run the engine was 30 micro-amperes. This is equivalent, at the pressure used, to about 0·001 joule, a very small amount of energy and only about 1/70th of the actual energy usually supplied. The spark consists of a first discharge which may last not more than one-millionth of a second, and after that the main portion of the energy follows, which is not really wanted at all.

Dr. N. W. McLachlan : As the author points out in the conclusion to his paper, it would be impossible to investigate all the details of the problems which arise in a magneto. There is one aspect of the subject which I think he has not emphasized sufficiently, namely, the efficiency of the magneto, i.e. the ratio $C_2V_2^2/L_1I_1^2$, where C_2 is the self-capacity of the secondary plus leads, V_2 is the secondary sparking voltage, L_1 is the primary inductance, and I_1 the primary current just before break. Experiments by Professor Taylor Jones showed that the ratio $C_2V_2^2/L_1I_1^2$ is about 20 per cent. Now, the ignition of an explosive mixture is dependent chiefly on the value of $\frac{1}{2}C_2V_2^2$, the initial electrostatic energy discharged across the spark-gap, so

that, in the absence of plug leakage, the major portion of the available electromagnetic energy in the primary is not utilized in effecting ignition. This was foreshadowed in the experiments of Paterson and Campbell, and has been substantiated recently by Morgan and Wheeler.* Most of the energy which the magneto supplies to the spark-gap is wasted in what the author terms the flame part of the spark, that is to say, the inductive component which is associated with the secondary current. This being the case, it was desirable to ascertain how the reduction in the initial electrostatic energy in the secondary circuit was caused. Some time ago I carried out a series of experiments † to obtain data from which the efficiency of the magneto could be determined. Measurements of the primary and secondary effective resistances showed that the values were very high. The bulk of the loss appeared to be due to hysteresis and eddy currents in the iron. The effective resistance of the windings was large enough to affect the periodic time of the circuits, i.e. the factor $R^2/4L^2$ in the formula $f = 1/(2\pi)[1/LC - R^2/4L^2]^{1/2}$, was not negligible compared with $1/LC$. The dielectric loss did not appear to be serious, but the leakage loss is probably larger than is generally anticipated. The stray losses in the metal fittings were not measured separately, but one might anticipate fairly large values for the H-type armature, owing to the masses of metal near the laminated core on which the primary is wound. In order to obtain information regarding such losses, the armature laminations should be built into a non-metal framework, but care must be taken against dielectric loss. An advantage of using "Stabilite" or "Bakelite" would be the reduction of leakage. There is, of course, a certain loss in the magnet, due to variation of flux, but this is not a large proportion of the total. On measuring the ratio of the first maxima and minima peaks of the secondary oscillation after break, the value was always in the neighbourhood of 2·5, which showed that the damping was excessive, and there would be little more than one oscillation with the secondary on open circuit. Measurements of the primary and secondary effective inductances showed that the inductance decreased rapidly with the frequency. This decrease in inductance, due to the oscillations which occur after break, means a reduction in the secondary peak voltage and therefore a loss in efficiency The efficiency of the magneto can be considered from two points of view, (*a*) as a generator of high secondary voltage, so that the ratio CV^2/LI^2 is as near unity as possible, and (*b*) as an ignition apparatus which will explode the charge in an engine cylinder under adverse conditions, such as a leaky plug. The latter, of course, corresponds to the author's "utility" measurements. In the first case we are solely concerned in obtaining the highest possible value of V_2, without regard to its rate of rise, i.e. the problem is to convert as much as possible of the primary electromagnetic energy into electrostatic energy of high peak potential in the secondary. It is assumed that the secondary voltage is measured by a rectifying valve and an electrostatic voltmeter, so that the maximum voltage is obtained in the absence of plug leakage. In the second case, the conditions are quite different. Here we are concerned in getting sufficient ionization in the gap between the points of the sparking plug, and a sufficient quantity of electricity ready to be discharged across the gap when the spark occurs. Provided an adequate voltage is applied, the ionization is a function of the voltage and its time of application. If the voltage rises rapidly, the value required to produce the necessary ionization, and therefore to cause sparking, is greater than if it rises slowly. Thus we have two factors to take into account: (1) the rate of rise of voltage, and (2) the voltage necessary to spark across the gap. The former depends, for any given magneto, on the primary and secondary inductances and on the plug leakage. The inductances decrease due to the oscillations which succeed the breaking of the primary, and this causes the rate of growth of the secondary voltage to increase, but there is a corresponding reduction in the maximum secondary voltage. The plug leakage causes the charge, which would accumulate in the winding in virtue of its self-capacity, to leak away, and therefore reduces the maximum secondary voltage, the electrostatic energy, and dV_2/dt. For any given value of plug leakage, it is seen that a compromise must be made between the maximum V_2 (i.e. the value as measured by a valve) and dV_2/dt, and it is essential to reduce the secondary inductance by removing some of the turns, so that dV_2/dt is increased and the secondary I^2R loss is decreased. The author refers to this in conclusion (2) on utility. The utility, however, is inversely proportional to the number of secondary turns in the absence of the secondary capacity, and this can hold only approximately for a limited range in the actual machine. It is clear, however, that in practice the maximum secondary voltage is never reached even with a plug of zero leakage, and therefore only a small fraction of $\frac{1}{2}L_1I_1^2$ is transformed into energy in the initial capacity component of the spark. If the secondary peak voltage V_2 is limited there is a certain value of dV_2/dt for which sparking will cease, since sufficient time does not elapse between zero voltage and maximum secondary voltage, and the ionization is inadequate to produce a spark. It is quite uncertain what will happen if we apply the author's super-voltage effect described on page 445. Since the natural frequency of the circuit is infinite, the voltage curve is a straight line of infinite length perpendicular to the time axis. But, although the voltage is infinite, its duration is zero, and therefore there can be neither ionization nor spark. If, on the other hand, we fix dV_2/dt and vary V_2, there is a certain value of the latter at which sparking will cease. The author states that, owing to the close coupling between primary and secondary, it is difficult to ascertain the optimum primary capacity. I think the major part of the trouble is due to the high value of the primary and secondary effective resistances after break, since these make the capacity/potential curves much more flat-topped than in the case of the induction coil. If the author were to define more concisely the conditions under which his conclusions apply, the results would be valuable. I am not quite clear about conclusion

* *Journal of the Chemical Society*, 1921, vol. 119, p. 239.
† *Philosophical Magazine*, 1921, vol. 41, p. 33.

(3) regarding the energy in the initial spark. I presume that the author means the capacity component of the spark. The energy in this component (apart from the increased sparking potential resulting from the increased rate of rise of voltage due to greater current at high speeds) * is constant at all speeds for a given sparking plug. I think the author is confusing the actual spark energy given by $\frac{1}{2}C_2V_2^2$, with that when V_2 is governed by the length of the spark-gap. I cannot see why the energy should be independent of the voltage in the absence of loss, since in this case $\frac{1}{2}C_2V_2^2$ max. = energy, and C_2 is fixed. I think the author must mean that the energy at high speeds is independent of the speed, because the primary current break is almost constant. Coming now to the energy stored in the magnetic circuit, in a recent publication (*see* Bibliography on page 467) I treated the energy problem in a slightly different way from the author, and showed that in the absence of armature loss, with a magnet of fixed dimensions, there is a certain value of the magnet flux for which the primary energy is a maximum. This particular flux can be obtained by varying either the length of the air-gap or the diameter of the armature. If we take the latter as being constant, there is a length of gap for which the energy is a maximum. This conclusion is approximately true at high speeds. In view of this fact, it would be interesting to know if magnetos are being worked with the optimum gap. It does not follow, of course, that this gap is the optimum for performance under working conditions, because there is the question of producing sparks at low speeds. The analysis of the energy also shows that the point on the hysteresis quadrant at which the primary energy is a maximum is in the neighbourhood of that where BH is a maximum. In view of the above results, I should like to ask the author what he means when he says on page 453 that the general effect of air-gap reluctance will be to reduce the stored energy, and also why the armature core and air-gap should be reduced to the least possible size, unless this is necessary for working at low speeds. I quite agree that something of this nature must be done if the smallest possible size of magnet is to be fitted. The size of the magnet cannot be reduced indefinitely, however, owing to practical limitations. Since BH is to be a maximum for maximum primary energy, it follows that when the armature and air-gap are fixed, there is a certain size of magnet which gives the desired condition. It would appear that the iron loss might be reduced by using laminations of a thickness of 1·5 mil, the surface not being ground sufficiently to cause short-circuiting of adjacent laminæ. The stray losses might be lessened by a slight modification in construction, but this is where mechanical difficulties arise. The use of magnet steel of coercive force 250 would enable a larger air-gap to be used, and would give more latitude in respect to the finish of the armature surface. This type of magnet steel would probably enable the air-gap to be adjusted more nearly to the optimum value, provided this value permits satisfactory performance at all speeds. As an offset to such procedure, there is the question of cost, which might prove prohibitive. There are several points on the question of magnet energy which I should like the author to explain. He says on page 450: "The total number of lines of force stretched out across this portion of the field will be equal to twice the working flux." I should like to know how he arrives at the condition in which the working flux at any moment is double that given by the magnet. On the same page he states that the reluctance of the magnetic field will vary, and then proceeds to integrate an expression in which the reluctance is assumed constant. In this equation, what angle of rotation from the zero position is taken to represent the upper limit?

* J. D. Morgan: "Impulsive Sparking Voltages in Small Gaps," *Philosophical Magazine*, 1921, vol. 41, p. 462.

Professor E. W. Marchant: The paper shows how important is the minute and detailed testing of materials, and how greatly the lack of this has arrested the construction and development of the magneto. The author refers to the fact that the new steels which are now being used in the construction of magnetos are likely to revolutionize their construction in the future. We are all familiar with the effect that "Stalloy" has had on the design of transformers, and possibly a similar discovery may be made in connection with magnetos. Mr. Paterson has pointed out that, from the point of view of ignition, voltage and not quantity of electricity is the essential factor. I have made experiments on the peak voltages obtained, and also of the currents which flowed under different working conditions, by means of an oscillograph, and there were great variations in them. It may be that these variations were important from the point of view of the operation of the magneto, that is to say, the larger currents may have heated up the coils and the contacts, although they have not affected the ignition at all. In the slide which the author showed, the curve for the laminated pole-shoes and that for the solid pole-shoes crossed each other. In the paper the curves are distinct from one another, and it is difficult to see why they should cross, that is to say, why the laminated pole-shoes should not always be better than the solid pole-shoes. I think the author's method of testing utility is particularly good. It seems to me to be very simple in operation, as it consists solely of shunting the spark-gap by a known resistance and finding the value of the shunt resistance which must be inserted across the spark-gap before the spark disappears.

(*Communicated*): In some experiments which I carried out I endeavoured to find what variations occur in the current given by a magneto under the different working conditions of a petrol engine. In order to determine what may be called the "characteristic curve" of the magneto, a number of records were made by means of an oscillograph of the current through the spark and of the voltage on the contact breaker of the armature of a standard M.L. magneto. The oscillograph was connected in the usual way, with a non-inductive resistance varying between 2 000 and 5 000 ohms in series with it across the contact maker, for taking the pressure records, and connected across a shunt of 1-ohm resistance in series with the spark, for recording the current through the spark-gap (see Fig. A). The magneto used in the experiments was supplied by the M.L. Magneto Syndicate, of

Coventry. The first series of experiments was made with a constant speed of magneto working on different spark-gaps in air. As the magneto was to be used in connection with an engine running normally at about 1 500 r.p.m. and connected by 2 : 1 gearing to the shaft of this engine, i.e. so as to run at 750 r.p.m., the bulk of the tests were made at this speed.

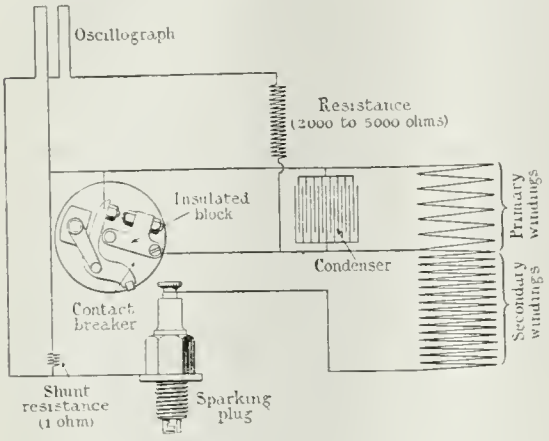

FIG. A.—Diagram of connections.

Effect of varying spark-gap at atmospheric pressure.—A number of records were taken of the current through a circuit with 1-inch sparking balls, the distance between which could be altered. The curve of voltage on the contact maker consisted of a sharp peak, the exact maximum value of which was difficult to estimate, followed usually by a more or less steady voltage which lasted as long as the spark current continued to flow.

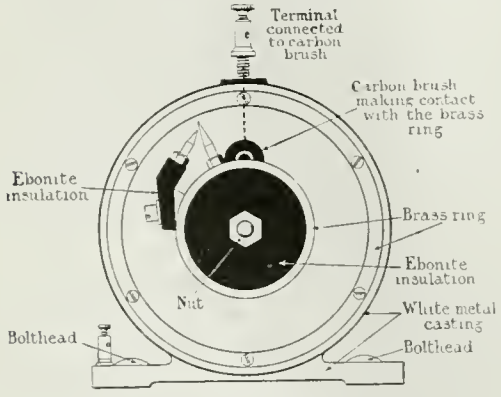

FIG. B.—Revolving spark-gap.

The time for which this pressure-rise lasted was so short that the oscillograph strips were unable to record the maximum value exactly. A voltage record is shown in Fig. C. The spark-gap was adjusted so that the machine was just not able to spark over on the outside gap, and this rush probably represented the maximum voltage which the magneto would produce. It corresponded to about 200 volts. The curves shown in Fig. D give the voltage and current when the magneto was working on a 1-mm. spark-gap between 1-inch spheres. The current through the gap occurs at the instant that the pressure-rise on the contact breaker takes place, and the pressure lasts as long as the spark continues. The spark current dies away very quickly, and it is clear that the

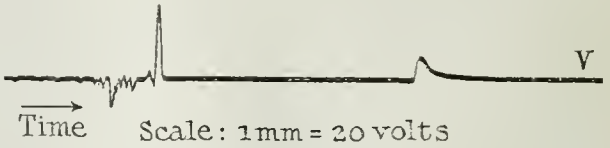

FIG. C.—Magneto speed 3 000 r.p.m. Resistance 4 000 ohms.

curve of dying away of the current is very far from being logarithmic, as would be the case if the resistance in the circuit were constant. In this, as in nearly all the curves of current obtained, the dying away of the current appears to follow a straight-line law, thus

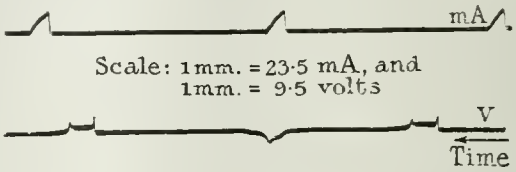

FIG. D.

showing that the resistance of the spark (which increases with diminishing current) forms an important part of the total resistance of the circuit and thus affects the decrement of the current. A similar curve is shown in

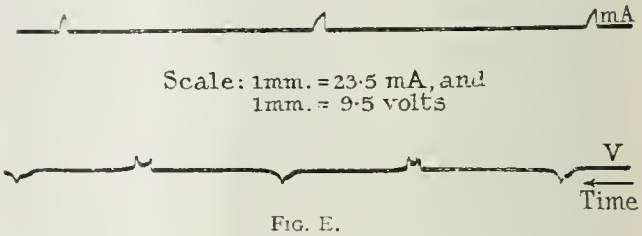

FIG. E.

Fig. E for a spark between points 5 mm. apart. It will be noticed that in this case the spark lasts a much shorter time than when it takes place between spheres, and that there is a definite kick on the voltage curve at the instant when the circuit is broken at the

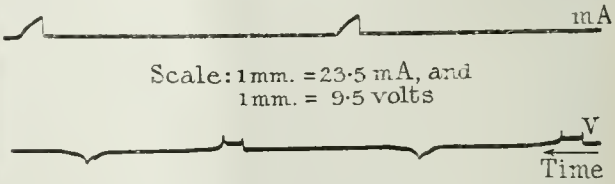

FIG. F.—Speed 750 r.p.m.

spark-gap. The resistance in the spark-gap circuit across which the oscillograph is shunted is in this case 1 ohm, which is small compared with the resistance of the magneto winding (2 000 ohms) and should make no difference to the working of the magneto. Fig. F shows the curve of current with a pair of larger

copper-disc electrodes 1 mm. apart, to see to what extent the capacity of the electrodes affected the rush of current; very little difference can be detected between the record with this gap and that with 1-inch spheres. The same figure also shows the curve of voltage on the contact maker. It is of the same general character as in the other case, but the interval between the make and break is, of course, much longer, as the spark in this instance lasts for a longer period. A series of tests was made with a number of spark-gaps each with

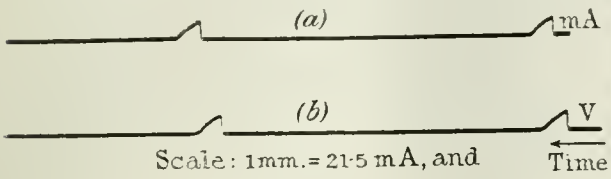

FIG. G.

a nickel wire in the centre of a circular hole in a sheet of copper. The ratio between the diameter of the hole and that of the wire was the same in each gap, the diameter of the wires varying from 0·9 mm. to 1·8 mm., and that of the hole from 4·5 mm. to 9·0 mm. This corresponds to a pressure on the secondary circuit of the magneto varying from about 4 700 to 9 500 volts. Although the pressure on the contact breaker varied within wide limits, the shape of the current curve was very similar in all cases. These curves are shown in Fig. G, (*a*) and (*b*), for the 0·70-mm. and 0·54-mm.

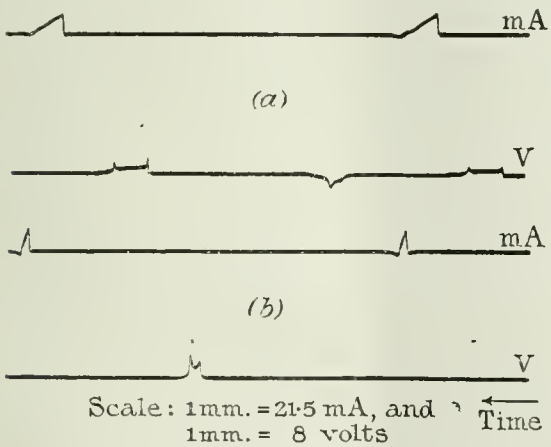

FIG. H.—(*a*) Lodge plug; (*b*) revolving spark.

wire air-gaps respectively. Several records of the pressure and current for a plug as used in motor cycles set with a gap of about 1 mm. were obtained when the plug was working in the open air, but as they correspond exactly to those obtained for the other gaps it is not necessary to record all of them. The current and voltage curves for a Lodge plug are, however, given in Fig. H (*a*), as this plug was used for most of the tests. Fig. H (*b*) shows the curves of current and pressure for a revolving spark-gap formed by two points, one the main electrode and the other an auxiliary, revolving together inside a brass ring, which forms the other electrode of the spark-gap (see Fig. B). The object of this gap was to enable the duration of the spark to be observed, by noticing the length of the stream of light formed when the spark passed. It will be noticed that with this spark-gap the duration of the spark is perceptibly less than it is when the spark occurs between the terminals of a plug, and also that there is a considerable voltage-rise when the spark ceases, due to the inductive effect of the magneto winding. This voltage-rise is as great as the original voltage which gave rise to the spark at the beginning of the rush of current.

Effect of air pressure on the spark-gap.—A number of tests were next made on the current passing when the plug was put in an air chamber in which the pressure of air could be gradually raised by pumping. The pressure was varied from atmospheric pressure up to 100 lb. per square inch, and a series of records was obtained. Although the voltage necessary to produce the spark increased as the air pressure was raised, there

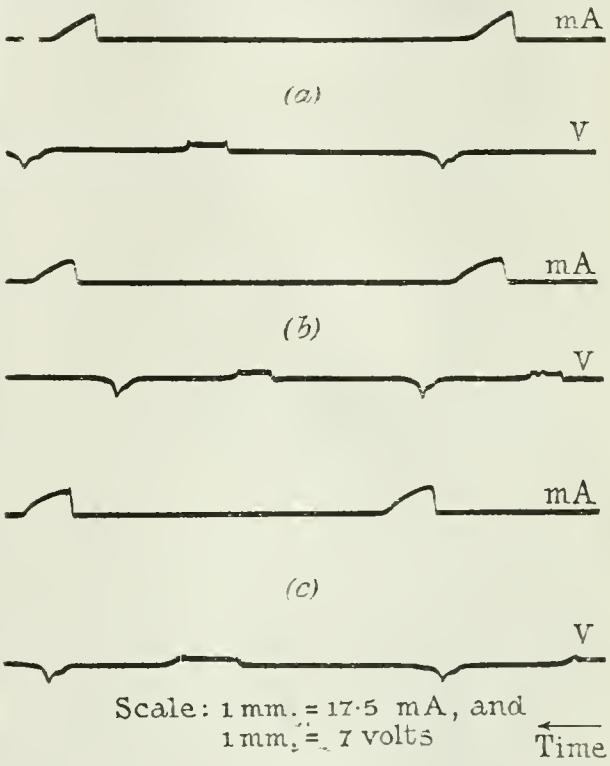

FIG. J.—Spark in compressed air (100 lb. per sq. in.).

was not much difference between the shapes of the current curves. These are shown in Fig. J, which gives the current curves for the Lodge plug working, (*a*) under atmospheric pressures, (*b*) at 20 lb. per square inch above atmospheric pressure, and (*c*), at 100 lb. per square inch above atmospheric pressure. The duration of the current is practically the same in all cases, but it will be noticed that at atmospheric pressure the curve of dying away follows nearly a straight-line law. At 20 lb. per square inch the curve is concave to the horizontal axis, and slightly more so at 100 lb. per square inch, showing that the spark dies away at a slower rate when the pressure of air on the air-gap

is increased. There is, of course, more energy stored in the electrodes with the higher pressure, on account of the increased voltage necessary to produce the spark, and this energy dies away less rapidly than it does at atmospheric pressure. These results are confirmed by experiments made on an actual engine working with different compressions, the duration of the current from the magneto being very nearly the same for an engine working on a high compression as for one working with a low compression, i.e. on a restricted air inlet. The greater suddenness with which the current is broken causes a slight increase in electrical pressure on the contact maker, but this is not very marked.

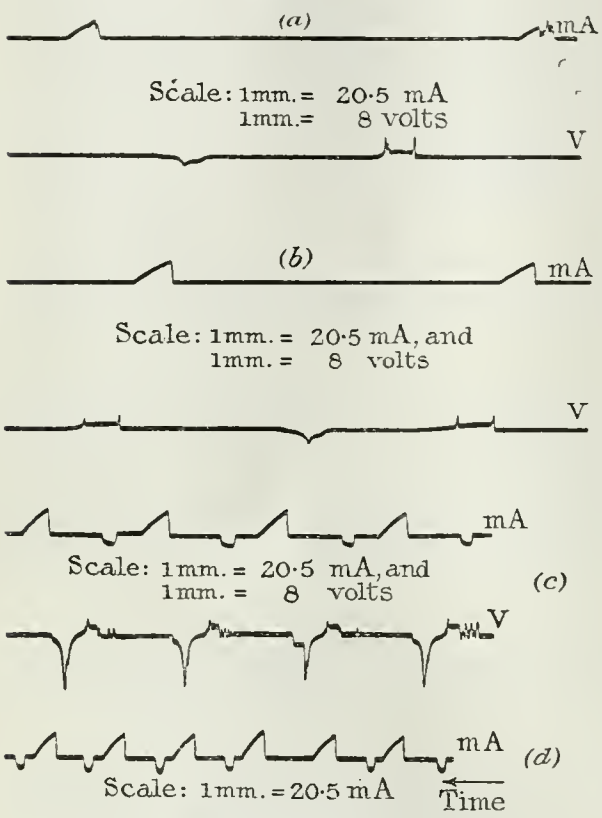

FIG. K.—(*a*) Speed 600 r.p.m.; (*b*) speed 750 r.p.m.; (*c*) speed 2 000 r.p.m.; (*d*) 3 000 speed r.p.m.

Influence of speed of magneto on current and voltage.—Since the pressure required to produce the spark given by a magneto is produced by a cam-operated contact breaker connected with the primary winding, it is evident that the magnitude of the voltage so produced must vary with the speed at which the magneto is working. A series of records was therefore taken with the magneto running at speeds varying from 500 to 3 000 r.p.m. The records for four speeds are shown in Fig. K, (*a*) at 600 r.p.m., (*b*) at 750 r.p.m., (*c*) at 2 000 r.p.m., and (*d*) at 3 000 r.p.m. The magneto was connected to a Lodge plug for all the tests, as it was found that this plug gave consistent results. As was anticipated, the maximum value of the "rotational" voltage increases with speed, but as the spark-gap was constant the voltage on the circuit breaker does not increase very much. The current through the spark-gap increases from about 55 mA at 500 r.p.m. to nearly 80 mA at 2 000 r.p.m. The duration of the spark is sensibly constant, varying between 0·007 second and 0·01 second under varying conditions at the spark-gap. The following table gives approximate values of the current and duration of spark for different magneto speeds.

Speed of magneto	Maximum current through spark-gap	Duration of spark
r.p.m.	mA	sec.
600	55	$7{\cdot}5 \times 10^{-3}$
750	70	$8{\cdot}5 \times 10^{-3}$
1 000	80	$9{\cdot}0 \times 10^{-3}$
1 500	80	10×10^{-3}
2 000	75	$7{\cdot}5 \times 10^{-3}$
3 100	70	—

The figures must not be interpreted too literally; they are only approximate since, as explained above, the variations between individual sparks and sets of sparks are considerable, and it may be that the conditions existing when the measurements at one speed are made do not correspond to those existing when the measurements are made at another speed. It will be noticed in Fig. K, (*c*) and (*d*), that there is a negative current passing through the spark-gap for a short time between the regular contact-breaker spark. This is produced by the rotational E.M.F. of the magneto, i.e. the E.M.F. due to the rotation of the magneto armature alone, without the added effect due to the contact maker. At the higher speeds this E.M.F. is large enough to produce a spark at the plug, but not to produce a very powerful discharge. It will be noticed that the pressure-rise at the end of the discharge is considerable, this rise being due to the inductive E.M.F. produced by the sudden rupture of the current through the spark-gap.

Tests of magneto working on an engine.—Tests were next made when the magneto was used on a $3\frac{1}{2}$-h.p. petrol engine. Three series of tests were made:—

(1) The mixture used was kept as constant as possible and the load on the engine varied by altering both the amount of petrol and the air supply, the magneto being adjusted so that the spark was timed to give maximum output for a given supply of petrol. The load on the engine was varied by altering the load on a separately-excited generator coupled by a belt to the engine. There was no perceptible difference in the current taken by the plug, although the pressures required to produce the spark with the higher compressions were, of course, greater than those required at the lower compressions and loads. The duration of the spark was not constant, the curves for three successive explosions being shown in Fig. L. The same phenomenon was observed at all loads, and these variations were

greater than any which could, with certainty, be observed between the records obtained at different loads. It will be noticed that the current appears to end somewhat abruptly, the spark presumably being blown out by the high pressure of the exploding gases and, on the curve of pressure given by the magneto, there is evidence of a pressure-rise at the end of the spark due, apparently, to this effect. This pressure kick is often higher than that observed at the break of the contact maker when the spark passes (see Fig. L). It is interesting to observe

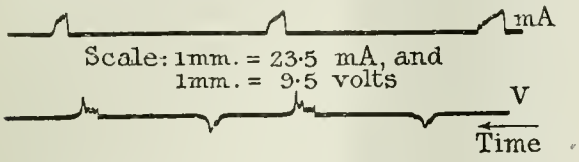

Fig. L.—Tests on engine.

that there is a very large factor of safety with the magneto. The pressure actually used in producing the spark, even with the highest compression, is less than one-quarter of the maximum pressure that the magneto can give when running on open circuit.

(2) A number of experiments were then made, using a constant-air inlet at constant speed, varying the strength of the explosive mixture and, therefore, the output of the engine, to see what effect this had on the spark. In this case very considerable differences were observable, the spark being of much longer duration when the mixture was weak than when it was strong. The maximum current passing through the spark was nearly the same in all cases, this being controlled almost entirely by the resistance of the secondary circuit of the magneto. The duration of the current in the three cases was, however, very different.

Approximate engine output	Time of duration of spark	Average pressure-rise at break of current
h.p.	sec.	volts
1·0	0·0077	25
1·7	0·0062	45
2·7	0·004	65

It will be noticed (see Fig. M) that the current appears to be cut off more quickly, i.e. the rate of decrease of

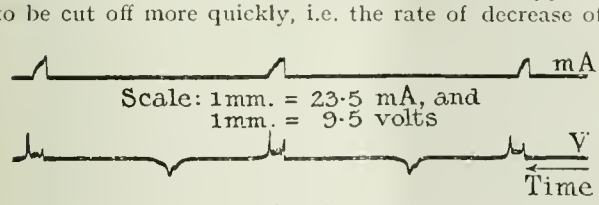

Fig. M.

current is more rapid, when the mixture is rich than when it is weak. This is shown by the electrical pressure-rise at the end of the explosion, as seen in the voltage curves. The corresponding figures are given in the table. It is interesting to note that this voltage-rise is considerably greater than that produced at the initial spark before the explosion, and it is evident that this is quite possible, since the pressure of gas in the cylinder immediately after the explosion is very much greater than it was before the explosion occurred. It follows from this that the spark-gap would require a much higher electrical pressure to produce a spark after the explosion had occurred in the cylinder than it did to fire the charge. This again points to the necessity for a considerable factor of safety in the armature insulation of the magneto, in order to avoid breakdown. This danger can, of course, be guarded against by employing an auxiliary safety-gap on the magneto itself outside the engine. It seems not unlikely that this gap may be in frequent use when the magneto is running. Fig. M illustrates the phenomenon.

(3) A third series of tests was made by keeping the mixture constant, or rather by keeping the position of the petrol and air valves the same, and varying the speed of the engine by altering the timing of the spark.

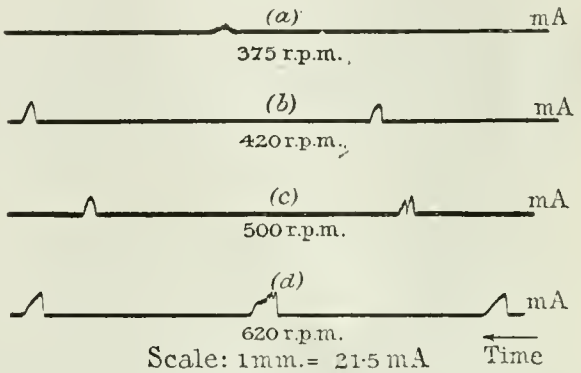

Fig. N.

The speed in this case was variable and much of the difference in the operation of the magneto was undoubtedly due to this cause. When the engine was running at the lower speeds both the magnitude and duration of the current passing were considerably reduced. When the curves of current and pressure were taken with a De Dion type plug fitted to an engine, similar results were obtained. At the lower speeds the current was reduced both in its maximum value and in the duration of the rush. These curves are shown in Fig. N, a constant mixture being used. The maximum value of the current passing through the spark-gap was less with the plug in air, but the difference was not very marked at 750 r.p.m., i.e. when the engine was running at 1 500 r.p.m. In order to find out whether any of the effects observed were due to the warming up of the engine, records were taken under given load conditions, (*a*) as soon after starting as possible, and (*b*) after the engine had been running for 10 minutes. The chief differences observed were in the shape of the current curves. After the engine had been running for some time the current rush was more nearly of a square form, and the rupture of the current due to the explosion in the cylinder was more sudden. The duration of the current in both cases was very nearly the same. The effect of this sudden

rupture of current can be seen in the pressure curves, as there is a much greater rise in pressure at the end of the explosion than when the charge was ignited (see Fig. O). In this figure the two curves of currents and pressures are taken on the same film, the curves of pressure being slightly separated for clearness. A number of experiments were made on the engine running with a mixture of petrol and paraffin in

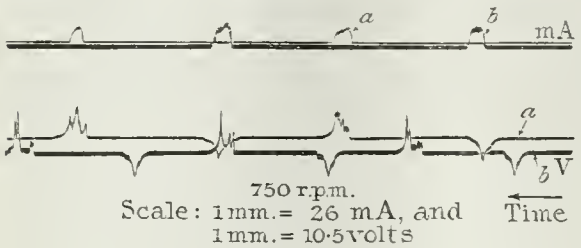

Fig. O.

about equal proportions. The curves are similar to those obtained with petrol, but the current in the spark-gap dies away more slowly, and then ceases abruptly, much in the same way as it does with the engine working on petrol only after running for some time. Two characteristic curves with the engine running on petrol only and on a mixture of

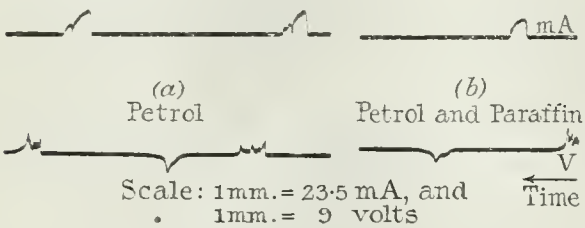

Fig. P.—(*a*) Petrol; (*b*) Petrol: paraffin = 1 : 1.

paraffin and petrol are shown in Fig. P. Fig. Q shows a similar curve for the magneto when working on an engine using paraffin only. The shape of the current curve is much the same as that when a mixture of petrol and paraffin was used. It is remarkable, however, that in this case the maximum value of the current and the duration of the spark are, if anything, less than when the engine works on petrol only.

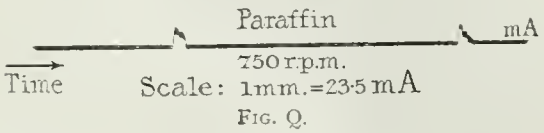

Fig. Q.

It is evident, therefore, that when once an engine is started running on paraffin, there should be little difficulty in igniting the mixture.

Mr. J. F. Kayser: During the past few months much has been said about the magnetic circuit, but there is one question which frequently arises and which has not been directly dealt with, viz. what are the dimensions of a cobalt-steel magnet necessary to replace a given tungsten magneto magnet? While the calculation of the dimensions of a magnet necessary for an instrument, from a consideration of the electrical constants and dimensions of the machine, is a matter of considerable difficulty, yet, if we have one magnet which will do the work, it is comparatively easy to determine the dimensions of another magnet which will give exactly the same service. The first step is the determination of the demagnetization curves of the two steels, followed by the determination of the working flux of the tungsten-steel magnet. Reference to the demagnetization curve of the tungsten-steel magnet gives a point corresponding to B_3 in Fig. 4, and the total energy given by the tungsten-steel magnet is then readily determined. By dividing that energy by the maximum available energy per unit volume of the cobalt steel, the necessary volume is determined. The next point is the area of cross-section necessary to give the flux, and this is very simply determined. The total flux necessary is the original area of the tungsten-steel magnet, multiplied by the flux density at which it is working, so that if we divide that product by the flux density at the point at which the cobalt steel gives its maximum available energy, we obtain the new area of cross-section necessary to give the same result. Previous experience in rule-of-thumb design has been gained only with tungsten or tungsten-chromium magnet steels, and is useless when applied to cobalt magnet steels with their greatly increased magnetomotive force. In fact, cobalt-steel magnets when designed according to general current practice will sometimes give results inferior to those obtained by the use of a tungsten-steel magnet of the same dimensions and weight. With proper design, however, a tungsten-steel magnet can always be replaced by a cobalt-steel magnet of a much simpler design and of never more than half the weight of the original tungsten-steel magnet.

Captain A. C. Burgoine: During the war the Air Services carried out a great deal of experimental and research work, partly in their own establishment at Farnborough and partly at the National Physical Laboratory. The work at Farnborough is still proceeding; a capable staff is engaged on research into all sorts of problems, not only what we may call the theoretical problems of ignition but very practical ones, with the object of assisting manufacturers to produce magnetos which will meet the very difficult requirements of the Air Ministry. The paper deals with magnetos for motor cars, but in the case of aircraft the conditions to be met are much more severe. In the first place, the engines run continuously at high speeds, but receive much greater attention than ordinary engines. At the altitude to which aircraft ascend, the density of the air falls to considerably less than one-half or one-third of what it is at ground-level, so that the problem of surface leakage within the magneto becomes very acute. Magnetos in aircraft have to operate at extremes of temperature, the temperature at a high altitude being sometimes 40 degrees below zero. On the other hand, they may sometimes be exposed to a good deal of heat, for example, from the exhaust. The Air Ministry has to deal with conditions which are never met with in motor cars. In order to

screen out interference with the wireless gear, a metal-braided high-tension cable has to be used, and this very considerably increases the capacity of the cable. Magnetos which will function perfectly well on plain cables may give a poor performance with braided cables. After a great deal of consideration and consultation with the Technical Committee of the Magneto Manufacturers' Association, a specification has been drawn up for aircraft magnetos. The Air Ministry has laid down, amongst other conditions, that the magneto must be capable of firing the plug with a leakage of a value of 200 000 ohms when running at 300 r.p.m. This is a much more severe test than that which the author calls for in his standard utility test, and it must be carried out with a braided cable. The magneto must be capable of functioning properly after 48 hours in a saturated atmosphere, and after 48 hours at a temperature of 55° C.; it must stand an endurance run of 500 hours; and it must give a satisfactory low-speed performance at 200 r.p.m. with both advanced and retarded timing. That is much more drastic than anything called for in connection with a motor car. The question is often asked, whether the British magneto is as good as the foreign magneto. I have tested a large number of foreign magnetos—German, French, Italian, Swiss, and American—and I have come to the conclusion that the up-to-date British magnetos are superior to any in the world from the point of view of design. The best English magnetos are, from the point of view of workmanship, at least equal to any in the world, and I think I may say that practically every type of British magneto is better than the best American magneto. The French magneto is nearly as good as the German magneto, which is a very excellent piece of workmanship. As regards mechanical design the German magneto is poor; the electrical performance is rather indifferent, but it functions very well indeed under very bad conditions of service with a small amount of attention and care, chiefly because of the fine workmanship and the great attention which is paid to detail. That is where the English manufacturer often fails; his workmanship is apt to be rather poor. Another question which is frequently asked, is whether the battery-and-coil ignition system will displace the magneto, or whether it is better than the magneto. That is a question which cannot be answered without qualifications. The question of price comes first. In the case of a motor car which has a dynamo and battery there should always be current available. In this case the coil will function well and is somewhat cheaper and easier to fit. If the dynamo or battery cannot be relied upon, then the magneto will always give a good performance. I think that in the course of a few years things may change, that is to say, we may have some totally different system of ignition. But, assuming we have both the battery and coil and the magneto, I think there will always be a field for both. The better cars, particularly those running with very high-speed engines, will have magneto ignition with, possibly, a battery set as a stand-by. The cheaper cars will have a coil system. The Americans make them in large quantities, not because they give better ignition than magnetos, but because they may be rather cheaper and are certainly easier to make. But they have not succeeded in making a thoroughly good magneto. As regards the design of magnetos generally, cobalt steel will render it possible to adopt radically new designs, which will be much neater than the older types with tungsten-steel horseshoe magnets. The use of cobalt magnets enables the design to be radically altered, so that the magnet may be fixed on the spindle. In some inductor magnetos the spindle carries solid steel inductors. There is no reason why the magnet itself should not rotate there, and this would revolutionize the whole of the design. Then, again, the use of the straight-bar cobalt magnets now being made should reduce the cost very considerably, and I expect that sooner or later the cost of cobalt steel will fall. The author has introduced more improvements into the British magneto than any other person in this country. He introduced the laminated pole-shoe; and I believe he was the first to use a cobalt-steel magnet, or at any rate to put it on the market. He introduced the steel cam and cam cage made in one piece of hardened and ground steel, in place of the separate steel cams which were fixed into the cam cage by screws, and he has made quite a number of detailed improvements in the machine. There is no one whose assistance the Air Ministry would seek on a difficult problem rather than the author.

Mr. A. P. Young : It has always seemed to me that the high-tension magneto is one of the most interesting and complicated pieces of electrical apparatus to be found in the whole range of electrical appliances. The machine can be studied from so many different angles, and any investigation of its operation characteristics necessarily impinges on so many diverse branches of electrical engineering. As illustrating what I have in mind, I shall deal briefly with a few of the main components of a magneto, indicating some of the problems with which one is naturally confronted in studying the function and performance of each part.

(1) The armature is very small, and usually only 2 inches in diameter. It comprises some 200 turns of primary winding and from 9 000 to 10 000 turns in the secondary. The voltage generated each time the primary winding is open-circuited may reach a maximum value of 11 000. Magnetos have been built to give over 250 distinct sparks per second, so it will be appreciated that from the standpoint of voltage alone the problem of satisfactorily insulating the secondary winding is quite complex, and analogous in many respects to the general problem of insulating high-voltage electrical machinery. It is obviously made more difficult, as a result of the very small space occupied by the windings. Regarding Professor Taylor Jones's mathematical treatment of the magneto, referred to by the author, we find that high-frequency oscillations which come within the range of wireless frequencies occur in the secondary circuit. In fact, one of the problems which has to be faced in connection with the installation of magnetos on aeroplanes fitted with wireless apparatus, is the prevention of interference that may arise from high-frequency oscillations in the high-tension circuit of the magneto. It therefore follows that the magneto

armature and connections, if studied in all their aspects, may lead one into the realms of wireless telegraphy.

(2) A detailed consideration of the function of a magneto, and of the relationship between the magnetic characteristics of the steel and the electrical performance of the magneto, becomes very complex, as the author clearly shows. Such a study naturally covers the whole range of permanent magnetism, which abounds with interest, whilst an inquiry into the influence of such factors as the composition of the steel, hardening temperature, etc., on the magnetic characteristics of the magnet, leads one into the realms of metallurgy.

(3) The contact breaker is, perhaps, the most wonderful part of a magneto. A study of its operation introduces one to many interesting mechanical problems, whilst the phenomenon of contact arcing, if examined in great detail, leads one to consider many difficult problems in the realms of metallurgy and physics.

(4) The condenser is one of the vital parts of a magneto. It has to be built into such a small compass that the problem of securing a suitable dielectric is very difficult. A great deal has yet to be learnt con-

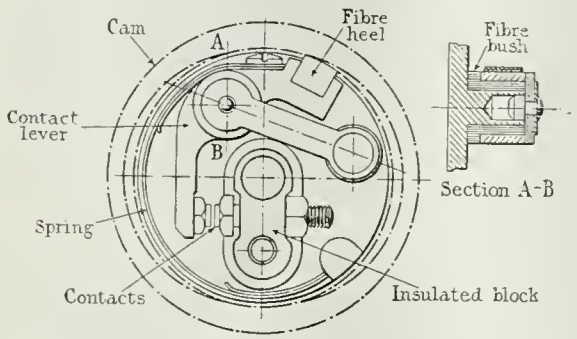

FIG. R.—Detailed construction of rotating-armature type of contact breaker used on B.T.H. type-G magneto.

cerning the exact nature and magnitude of electrical and mechanical strains to which a condenser is subjected in service. It is, of course, connected directly across the contacts, so that under normal operating conditions the condenser has applied to it a very rapid series of voltage impulses.

(5) The insulating parts, such as the distributor, slip-ring, etc., are made of a moulded insulating material having a rubber base. The manufacture of these parts in moulds, starting with a plastic mass, introduces one to a branch of electrical engineering which has not in the past received adequate consideration in this country. It is full of interest, and the potentialities of this particular line of manufacture are very great.

(6) Finally, we come to the interesting and complex problems involved in manufacturing magnetos. The limits of accuracy are necessarily very fine—much smaller than on any other class of electrical apparatus. For example, the air-gap between the armature and pole-pieces is usually about 0·005 inch, a tolerance of only ± 0·001 inch being allowed on this dimension. Limit gauges are, of course, used in this manufacture, and for a certain type of multi-cylinder aero magneto manufactured during the war no less than 720 gauges were necessary. This particular magneto had in its composition 400 parts of different design, the total number of parts being 860. The finest degree of accuracy worked to was a tolerance of 0·0003 inch.

The paper covers a very wide field, and in general I am in agreement with the views expressed therein. I propose to confine my remarks to a brief consideration

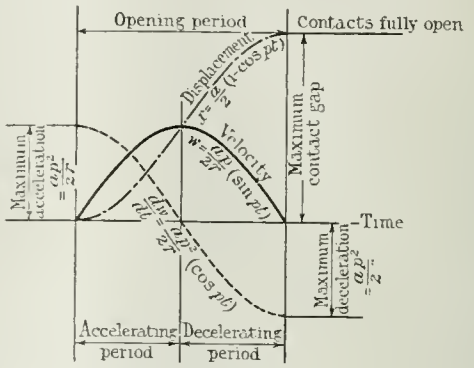

FIG. S.—Analysis of motion of contact-breaker lever arm, assuming harmonic motion.

a = Maximum contact gap in cm.
r = Distance in cm. between centre of contact face and axis of oscillation of lever arm.
x = Displacement in cm. of movable contact with respect to fixed contact.
n = Speed of magneto shaft in revs. per second.
w = Angular velocity of lever arm about its axis of oscillation.
β = Angle in radians measured on the rotor, corresponding to the movement of the contact from its zero to its maximum opening.
$p = \frac{2\pi^2 n}{\beta}$.

of contact breakers, laminated poles, and the characteristics of the high-tension spark.

Contact breaker.—Fig. R illustrates a type of contact breaker used on the orthodox rotating-armature type of magneto. It comprises a little steel spring-controlled contact arm, which is free to oscillate on an

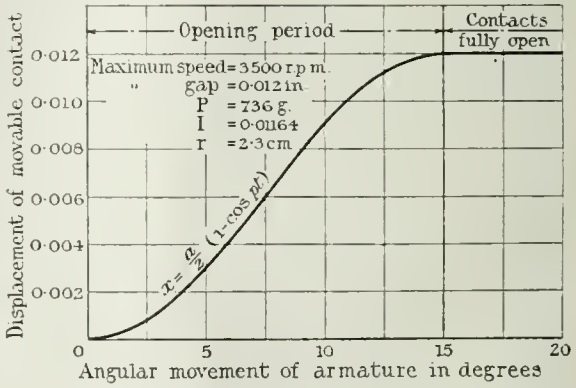

FIG. T.—Rotating-armature magneto. Displacement curve for movable contact, assuming harmonic motion.

P = Pressure between contacts when closed, due to control spring.
I = Moment of inertia of lever arm about its axis of oscillation.
r = Distance in cm. between centre of contact face and axis of oscillation of lever arm.

insulating bush usually made of fibre, such oscillation being imparted to the lever by virtue of the fibre heel attached to one end of it coming in contact with an annular cam which surrounds the revolving unit. The maximum gap between the contacts is only 0·012 inch, but although the movement is so small, the forces that

are brought into play at high speeds become relatively great. In fact, the purely mechanical problems involved in a study of the operation of a contact breaker are most interesting, and Figs. S and T show some curves which illustrate the relatively short period of time during which the contacts are opening, on the assumption that the heel and cam profiles are such as to impart harmonic motion. The point of special interest is that at the moment of impact the acceleration is a maximum (see Fig. S) and, in consequence, the pressure between the heel and the cam at this moment, which is equal to the spring pressure plus the force required to impart this acceleration to the lever, also reaches a maximum. When operating at high speeds the acceleration component of the force greatly exceeds that due to the spring pressure, and on a certain design of 4-cylinder magneto this acceleration force may reach 7 lb. at a speed of 3 000 r.p.m. On a multi-cylinder aeroplane magneto that I designed during the war the acceleration force was as much as 14 lb. The author has dealt very fully with the phenomenon of contact arcing. This particular aspect of the general performance of a high-tension magneto opens up wide fields for further research and investigation, because it is doubtful whether this phenomenon is clearly understood even at the present time. He rather suggests that the theory of sparking voltages which has been put forward in an attempt to explain what happens at the moment of separation of the contacts is hardly compatible with the theory of arcing. It is perfectly true, as he points out, that the local heating at the final point of contact between the two contact faces is a factor of primary importance in determining whether arcing is likely to occur, and I believe that other important factors are the variation in the contact resistance just prior to actual separation; the current passing at that instant; and the temperature of the contact faces as well as the degree of volatilization that obtains. All these factors will naturally tend to reduce the sparking voltage, looking at the two contacts as two electrodes separated by a short distance, and, if the local heating is excessive just before separation, the resistance of the gap will be so small as to make the sparking voltage practically zero, so that from this standpoint the two theories will be brought somewhat into line. It is certainly true that bad contact-arcing can be obviated with almost any contact material by making the condenser sufficiently large, but in a magneto certain considerations limit the size of condenser that can be used, and it is on this account that the problem of securing a satisfactory contact material becomes so difficult. At the present time, the most satisfactory contact metal is undoubtedly a platinum-iridium alloy containing from 15 to 25 per cent of iridium. Under certain conditions tungsten gives reasonably good results, but there is no question that it is inferior to the platinum-iridium alloy in respect of its range of adaptability, using this expression to denote such factors as current, flux, and speed. Even so, it is the next best contact metal to the platinum-iridium alloy and, on the battery-coil system of ignition used so extensively in the States, tungsten is found to yield quite satisfactory results and is generally used. I am inclined to think, however, that in the case of tungsten there is a certain critical value of the current which must not be exceeded no matter what the size of the contacts may be, and as the contact current in all battery-coil systems of ignition is much less than that in a magneto (see Fig. U), this is most probably the reason why tungsten is used successfully in America. It would appear, also, that a humid atmosphere such as frequently obtains in this country has a more detrimental effect on the operation of tungsten contacts than happens in the case of a platinum-iridium alloy. In a recent investigation that I made on this subject, I found that there was a certain relationship between the performance of different materials when used as magneto contacts, and the physical characteristics of these materials. Thus, by arranging different metals in their order of merit, as determined from their suitability for use as contact metals, it was found that, starting with the best metal and going down the scale, the density diminished, the specific resistance diminished, whilst the heat conductivity increased. I fully agree with the author that there is still a large amount of research to be done on this question, and a careful investigation of this nature should yield results that would be of great benefit to the magneto industry, as the problem of securing a cheaper metal for use on magneto contacts is of paramount importance now that there is a world-wide demand for a reduction in the price of commodities.

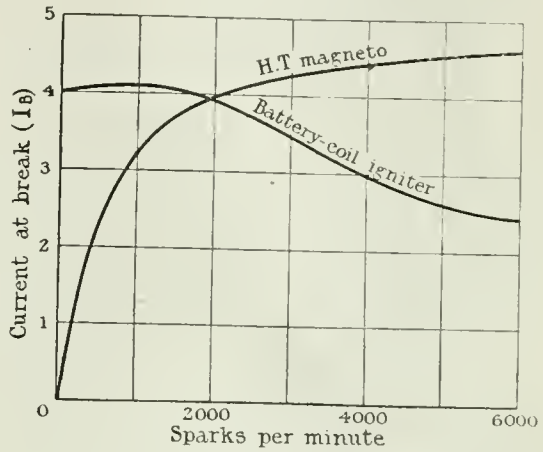

FIG. U.—"Break" current curves for magneto and battery-coil ignition.

Laminated poles.—The author refers to the use of laminated poles, and the introduction of this change has been of very great benefit to the British magneto industry. I believe that he was the first in this country to build a rotating-armature magneto with laminated poles, and this innovation is now adopted by many magneto manufacturers. The use of laminated poles not only enables the minimum sparking speed, and therefore the starting characteristics of the magneto, to be considerably improved for a given size of magnet and a given flux, but the energy in the high-tension spark is greatly increased by reason of the fact that the eddy-current loss in a solid pole-piece is very considerable. The curves in Fig. V are of interest in

this connection as showing how in a solid-pole machine the active flux in the armature core diminishes with increase in speed, solely as a result of the eddy-current damping in the solid masses of iron through which the flux has to pass. These curves were plotted from results of tests carried out on a 6-cylinder Bosch magneto built during the war. It will be noted that the flux drops from about 32 000 at low speeds to 16 000 at a speed of 5 000 r.p.m. Corresponding to this reduction in flux, the energy in the spark also falls off rapidly as the speed is increased. In using laminated poles it is an advantage to make the lamination as thin as possible. I investigated this point some time ago by obtaining a comparison between the operation characteristics of a certain magneto when it was fitted with pole-pieces made up of laminations of different thicknesses. The thickness of lamination was increased from 0·016 inch in steps up to 0·064 inch, a final series of tests being made with a solid cast-iron pole-piece as generally adopted by the Bosch Company. The results obtained were as follows:—

(a) *Minimum Sparking Speed.*

Thickness of lamination	Minimum sparking speed
in.	r.p.m.
0·016	70
0·064	100
Solid pole-piece	130

(b) *Peak Voltage Measured across a Standard 5·5-mm. 3-point Spark-gap.*

Thickness of lamination	Peak voltage
in.	volts
0·016	7 800
0·064	7 700
Solid pole-piece	7 200

(NOTE: The above voltages were measured at a speed of 2 000 r.p.m.)

(c) *Energy in High-tension Spark.*

Thickness of lamination	Energy in spark
in.	joule
0·016	0·060
0·064	0·059
Solid pole-piece	0·053

One of the difficulties that has to be contended with in the orthodox form of rotating-armature magneto is the variation in the minimum sparking speed that results from any adjustment of the timing lever. The timing is adjusted by rocking the cam cage with respect to the revolving contact breaker, so that as the sparking is retarded the air-gap between the trailing edge of

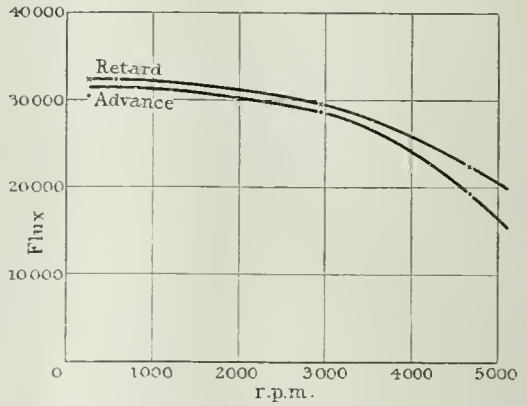

FIG. V.—Bosch type ZH6 magneto provided with adjustable polar segments. Curves showing variation of armature-core flux with speed.

the armature core and the leading edge of the pole-piece automatically increases. The curve given in Fig. W is of special interest in this connection, as it shows the relationship between the minimum sparking speed and this particular gap in the case of a small laminated-pole 4-cylinder magneto. The curve has been plotted from the results of tests, and it shows the

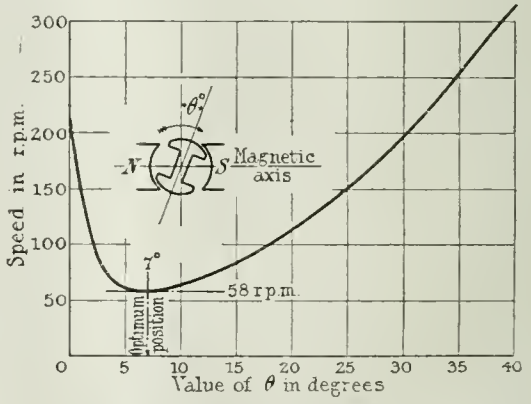

FIG. W.—Curve showing relation between minimum sparking speed and armature position.

(*Note.*—Tests made on small 4-cylinder rotating-armature magneto with laminated poles and spark-gap distributor, using standard 3-point test gaps set to discharge at 8 500 volts.)

actual relationship existing between the minimum sparking speed and the angle θ, this being the inclination of the axis of the armature core to a line drawn at right angles to the magnetic axis, when "break" occurs. The interesting point is the existence of a certain critical gap which gives the lowest minimum sparking speed, and in this particular design of machine this gap corresponds to an angle θ of about 7°, which means an

actual air-gap of about 2 mm. between the trailing edge of the armature core and the leading edge of the pole-tip at the moment of "break." Most manufacturers "time" their magnetos to give a gap of this order, so that when the timing lever is fully advanced the minimum sparking speed is at its optimum value. When the timing lever is retarded, however, the minimum sparking speed increases rapidly, and various methods have been introduced, such as the use of extended pole-pieces, to make this inequality as small as possible. Actually, it is possible so to modify the ordinary rotating-armature magneto to give uniform sparking over the whole timing range, by introducing polar segments which are mechanically linked to the contact-breaker cam cage. These segments are adapted for rotation between the revolving armature and the fixed pole-pieces with which the polar segments are in magnetic contact. In this construction the air-gap between the trailing edge of the armature core and the leading edge of the pole-tip at the moment of "break" is constant for all positions of the timing lever, and as a result the intensity of the spark is uniform over this range. Such a magneto, particularly when it is used

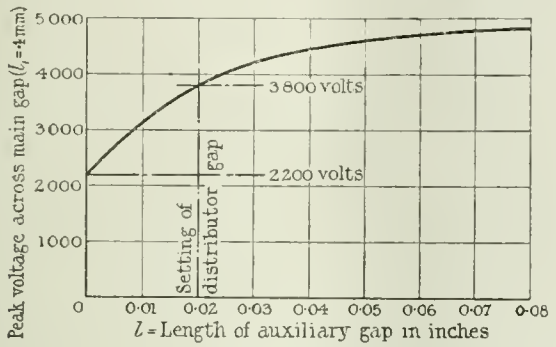

Fig. X.—Test to determine efficacy of auxiliary spark-gap. Resistance in parallel with main gap = 112 000 ohms.

on high-power engines, has a wonderful effect on the flexibility and performance of the engine, as it enables the spark to be very considerably retarded, for example, when going up a hill, without any appreciable reduction in power, such as is likely to occur with the orthodox form of magneto.

The high-tension spark.—The author refers to the use of the jump-spark form of distribution which is now very generally adopted on British magnetos. I fully agree that the introduction of this system is beneficial from the point of view that it quenches the high-tension spark and thus prevents the current from remaining in the high-tension circuit. I do not quite agree, however, that from the point of view of enabling the magneto to cope more readily with excessive leakage in the high-tension circuit, the spark-gap is of no material value. In support of this contention the curve given in Fig. X is of interest. It represents the results of certain tests that were made to determine the effect on the sparking voltage of a given gap connected to a magneto, of introducing in series with the gap a small auxiliary spark-gap such as is used in the jump-spark system. This curve shows that with a small gap of only 0·02 inch the sparking voltage across the main gap was increased from 2 200 to 3 800 volts, when a resistance of 112 000 ohms was connected in parallel with the main gap. The conditions obtaining in this test approximated somewhat closely to the worst conditions that may result in practice when the insulation resistance of the plug is reduced to a very

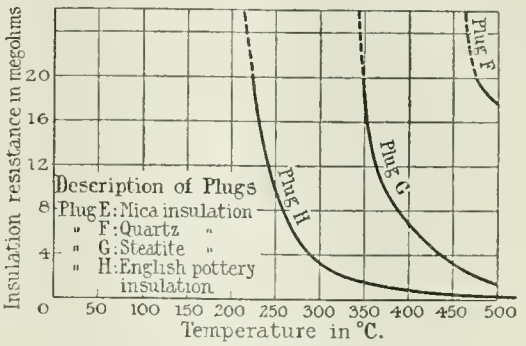

Fig. Y.—Insulation-resistance tests on plugs.

(*Note.*—Plug E had a resistance considerably greater than 20 megohms at all temperatures.)

low figure. As bearing on this latter point, the curves in Fig. Y may be of interest as showing how the insulation resistance falls away very rapidly with increase in temperature in the case of three plugs fitted with different forms of insulator. Plug insulation-resistances as low as 100 000 to 200 000 ohms are not at all impossible, and when the magneto has to function under such adverse operating conditions I believe that the auxiliary spark-gap is beneficial in enabling the voltage to rise more quickly to its maximum value, than it does

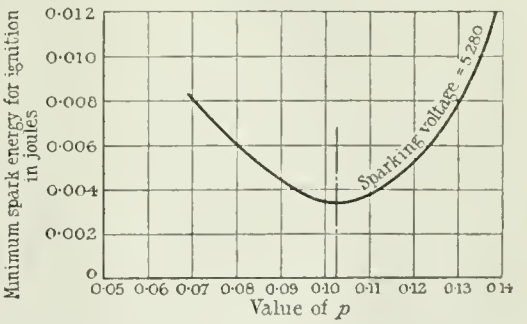

Fig. Z.—Curve showing minimum spark energy required for ignition of petrol mixture. (Test made by Paterson and Campbell.)

when there is no such spark in the high-tension circuit, and the end of the secondary winding is connected directly to the plug. The author says on page 460: "Although the flame which follows the initial spark is valueless from the point of view of ignition, its study is of considerable importance." I can hardly agree that the heat energy contained in the flame portion of the spark is of no value, and although the author's contention is probably quite correct when applied to normal running conditions, the adverse operating conditions really determine what the design of an efficient ignition unit should be. One of the most

important considerations is the question of starting, and when everything is cold there is no doubt that the petrol enters the cylinder in the form of a mist, and the mixture to be exploded is not a homogeneous gas. Under these conditions I believe that the heat energy contained in the spark is of value in volatilizing some of the minute petrol globules held in suspension in the air, thus forming in the vicinity of the spark-plug electrodes an explosive mixture which approximates to the ideal. The curve in Fig. Z is of interest as showing the amount of energy actually required to ignite an explosive mixture of petrol and air, this curve being taken from one of the papers by Mr. Paterson and Dr. Campbell, giving the results of a large amount of research work which these investigators carried out at the National Physical Laboratory. The curve shows that a spark containing as little energy as 0·0035 joule is sufficient to ignite an explosive mixture, and this amount of energy is less than one-tenth of that given by almost any form of magneto. In Fig. AA curves

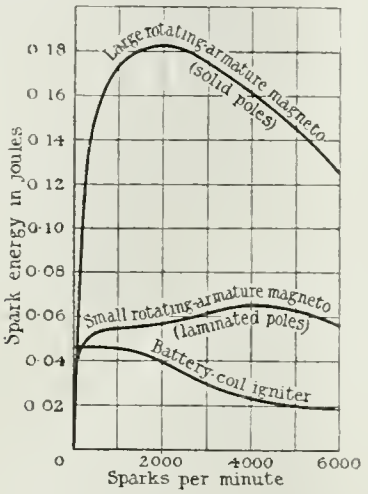

FIG. AA.—Spark-energy curves.

are shown which indicate the heat energy contained in the spark generated by different ignition units. It is of especial interest to note that, in the case of the small magneto fitted with laminated poles, the energy in the spark is maintained over the whole range of speed, whilst in the case of the larger solid-pole machine, for reasons already indicated, the energy quickly reaches a maximum and then falls rapidly away. The battery-coil system of ignition, on the other hand, gives a spark containing much less energy than that liberated by any form of high-tension magneto, the spark energy being a maximum at zero speed, and falling away gradually as the speed is increased.

Dr. G. E. Bairsto (*communicated*): Electrical engineers who have made a special study of magneto and electrical ignition problems will entirely agree with the author's introductory sentences. The successful design and manufacture of a magneto is not one problem, but a whole series of fascinating problems of a very varied nature in design, difficulties of production, properties of raw materials, high-frequency phenomena, electrical physics of sparks and arcs, high-frequency measurements, etc., and, moreover, the equipment necessary for dealing with all these problems is so specialized that it cannot be found in even the best-equipped electrical laboratory not laid out for this special work. Another point which should be emphasized is that the Germans kept the whole of the technical and scientific information gathered on magnetos and electrical ignition as a closely guarded secret, the result being that there was a very small fund indeed of information available in the scientific Press from which we could advance our knowledge. Hence, when the war broke out, all the essential facts relating to magneto design, etc., gathered by the Germans during the past 25 years, had to be obtained in a very short time, literally a few months, and those manufacturers who started the manufacture of magnetos only then realized the very great complexity of the problems involved. It speaks very well for the initiative and ability they put into the matter that we were able to get magnetos in rapidly growing quantities a few months after the commencement of hostilities. Without them there would have been only one result—we should certainly have lost the war. No greater tribute can be paid than that made by our President (Mr. Ll. B. Atkinson) in the Tenth Kelvin Lecture*: "Next to defeating the German armies the production in our own country of a magneto for petrol engine ignition has been the most remarkable achievement of the war." The author gives considerable attention in the early part of the paper to the effect of plug leakage. In a paper put before the Advisory Committee for Aeronautics in November, 1918, and later published in the *Journal* of this Institution for June, 1920, I dealt very fully with the subject, both experimentally and theoretically, and gave the results of a large number of measurements made in the electrical research laboratory at the Royal Aircraft Establishment. It was shown that the effect for large leaks is governed by the factor

$$\frac{M}{L_2} I_0 \left[\epsilon^{-[(R+R_2)/L_2]\tau/q} + \epsilon^{-[(R+R_2)/L_1]\tau/q}\right]$$

where M, L_1, L_2, R_2, and C_1 are the electrical constants of the armature, I_0 is the maximum current at break, R is the resistance of the leak, and $q = 1/\sqrt{(L_1 C_1)}$. This expression may be shown to be proportional to $1/N_2$. On the other hand, the spark energy which is roughly proportional to $\frac{1}{2}L_1 I_0^2$ is proportional to $1/N_1^2$. There cannot, therefore, be any direct connection between the plug-leakage effect and spark energy as implied on page 447. It was also shown in the above-mentioned paper that, for small leaks, the quicker the rise of potential the less the magneto system reacts to the plug leakage, and also that, provided the plug resistance is small compared with the reactance of the secondary winding, the smaller is the number of secondary turns and the less will be the amount of plug leakage. In later papers it is shown that there exists an optimum value for the secondary turns which secures the least reaction to leakage. Since then I have shown theoretically that such an optimum value exists. On page 460,

* *Journal I.E.E.*, 1919, vol. 57, p. 21.

the author states that the low-speed performance is limited by the number of secondary turns, and that a lower limit exists. I disagree with him here, and my reasons have been clearly set out in the papers quoted above. As regards a standard of "utility" the Air Ministry, as a result of tests carried out at the Royal Aircraft Establishment, has fixed 200 000 ohms at a voltage of 9 000 volts as a standard for future designs of magnetos. This is a more stringent test than that given by the author's figure. The ideal we are aiming for is 100 000 ohms at the same voltage, but this is beyond the capacity of present-day magnetos. The author gives a very elaborate analysis of the flux-changes in the magnetic system and shows how these build up the secondary voltage. While such an analysis is very illuminating as regards the complex effects which occur in the pole-shoes, pole-tips and core, I feel that it is too complicated a means of getting the final results. I do not think that one can get a quantitative idea of the phenomena, and I prefer to apply the ordinary induction-coil theory and assume two stages in the building up of the E.M.F., first, the generation of the primary current by the magneto as an alternator, and secondly, the generation of the secondary E.M.F. from this current by the magneto as an induction coil having certain electrical constants. The author's method cannot be used to calculate even roughly what the effect of a certain amount of plug leakage will be on a magneto, whereas in my paper quoted above the analysis would give this effect to about 20 or 30 per cent.

[The author's reply to this discussion will be found on page 486.]

North-Western Centre, at Manchester, 22 February, 1921.

Professor E. H. Crapper: I am particularly interested in the question of magnet steels, and am pleased to have the opportunity of thanking the author for his treatment of the problems associated with the magnetic circuit of the magneto, and especially that portion dealing with the part played by the permanent magnet as a source of energy. It is true that until recently we were dependent upon the simple carbon steel, chrome steel and tungsten steel for the production of permanent magnets, and the recent introduction of cobalt steel lends added interest to the subject of magneto magnets, and one can scarcely realize to what extent it will affect the design of the magneto. Whilst I agree with the author that the three working constants, remanence, coercive force and the product $\beta\phi$, associated with the quality of a permanent magnet, suffice for the purpose of the design of a magneto, I wish to emphasize the fact that a knowledge of the nature of the material and its suitability for use as a magneto magnet can only be satisfactory when we know the relationship existing between the constants named. For instance, it may not be generally known that the magnitudes of the constants remanence and coercive force of a magnet are definite functions of the saturation value of the intensity of magnetization of the material, in the condition in which it is to be used. Furthermore, it is essentially necessary to apply a sufficiently high value of magnetizing force to ensure saturation of the magnet. Fortunately, a linear relationship exists between the reluctivity of magnetic materials and the magnetizing force H; thus, if we express the relationship existing between the reciprocal of the magnetic susceptibility κ of the material and the value of the magnetizing force H, both of which are readily obtained from the ordinary magnetization curve, we obtain Fröhlich's equation, which is as follows:—

$$1/\kappa = a + bH$$

In this equation the value of b is the reciprocal of the value of the saturation intensity of magnetization of the material, provided that the magnetizing force $H_{max.}$ applied is sufficiently high. It is interesting to note in connection with the new cobalt steel that the saturation intensity of the 35 per cent cobalt alloy is higher than that of pure iron, whilst the high values of the coercive force and the reluctance of the material are remarkable. The former varies from 200 to 240 units of demagnetizing force, whilst the values of the latter correspond to those of good tungsten steel. The inherent higher reluctance of this steel necessitates the application of very high values of magnetizing force—from 1 000 to 2 000 units of H—if the best results are to be obtained. Furthermore, as the author has pointed out, the value of the product $\beta\phi$ for tungsten steel is of the order 300 000, whilst that for cobalt steel is 700 000 units. These values represent the technical values of the two materials. I was particularly interested in the following statement on page 450: "Between these points and for any value of H up to the point B_3, the magnet may be said to be truly elastic." I am in perfect agreement with this view, and a few years ago I stated in *Engineering*,* in referring to the connection between the magnetic properties and the mechanical properties of steel, that "whatever changes take place in a magnet in consequence of external influences, occur because the elastic limit of the material is such as to permit the movement of the molecules of the magnet to take place slowly or quickly, as the case may be."

Mr. J. W. Naylor: My chief interest is in the development of the cobalt steel. About a year ago, at the instigation of the author, I initiated experiments on that branch of the subject, and a month later discovered the material which has been worked on ever since and was the first steel made in this country to give such outstanding results. The results shown on the lantern slide by Prof. Crapper should have been increased by 25 to 50 per cent, because the full saturation was not given to the steel. When fully saturated, that particular sample will give a remanence of 10 400, a coercive force of 246, and a product of 960 000.

Mr. J. F. Kayser: Up to the present, the dimensions

* *Engineering*, 1918 vol. 106, p. 361.

of the majority of magneto magnets have been worked out on rule-of-thumb lines, and the reason why such methods have given satisfaction is no doubt due to the fact that it is quite easy for a bad or at least uneconomical magnet to perform a given service in a satisfactory manner, but in such cases it will be found that a correct proportioning of the dimensions of the magnet will enable considerable economies in volume to be made. The general conception has been that a certain remanence is required in order to give the working flux, and that a coercive force of about 60 to 70 is necessary to ensure permanence. A study of the paper will show that such a view is quite untenable. Coercive force does not give permanence, and it is quite possible to obtain, and I have frequently had under observation two steels giving the same remanence and the same coercive force which yet behave very differently when allowed to age. Users of magnets have been placing a great deal of faith in the remanence figure. Of all the magnetic constants remanence is one of the least useful to a designer. It is simply a property of a closed magnetic circuit, and a magnet which works with a flux density equal to the remanence will have no available external energy. I think it is necessary to bear in mind very carefully what are the magnetic constants which are fundamental in the design of a permanent magnet system, and I think it will be agreed after the perusal of the present paper and of Mr. Evershed's recent paper * that this is now a question settled beyond all doubt, that the true criterion of magnetic value is the maximum product of B and H, and that the fundamentals of design are the values of B and H giving that product. Whilst no doubt of great academic interest, such constants as the rate of change of susceptibility or the maximum saturation value are absolutely useless to any designer. I can confirm the figure of 900 000 for the maximum product given by a steel containing 35 per cent of cobalt, and I have myself obtained a product higher than one million, but on account of the high price of cobalt such a steel is not a commercial possibility at the present moment. Steels containing large quantities of chromium and less cobalt, whilst only giving a maximum product of about 700 000 to 750 000 are much less costly and are already incorporated in magnetic systems. Such steels as the latter have also a very great advantage over other magnet steels in that the final hardening treatment consists of an air cooling which eliminates any tendency to produce hardening cracks.

Dr. C. C. Garrard : With measuring instruments I have always found that, in order to secure really permanent magnets, one has to demagnetize them to a certain extent below the saturation point. I should like the author to give some information as to how this fits in with the rest of his theory.

Mr. G. A. Cheetham : As a magneto is a piece of apparatus designed to fulfil a particular purpose, it is quite evident that finality in magneto design cannot be accomplished until the problems relating to the function which it has to perform are definitely solved. The electrical ignition of explosive mixtures must therefore be carried towards a conclusion. There is usually a tendency for factors of safety to be high when knowledge of the problem to be solved is incomplete. When the war broke out engineers in this country were faced with a practically new problem in the rapid production of magnetos for internal combustion engines principally for use on aircraft. In view of the urgency of the demand and the scanty knowledge available on the subject, new British designs in the early stages of the war were based upon existing types. At the same time, existing work on spark ignition was collected and new work undertaken under the direction of the Air Board, which resulted in standard performance requirements for aircraft magnetos. The main problem, therefore, at that time was to fulfil this performance specification by all the machines completed, with as little difficulty as possible and without radical departure from fairly well-defined designs. This is quite a different problem from that which the author and his colleagues now have before them, but the investigations undertaken in the solution of the first problem must have been invaluable when design itself began to evolve along scientific lines. A recapitulation of some of the early difficulties may be of interest to those not very familiar with magneto problems. Some difficulty was experienced in obtaining agreement between various manufacturers for the remanent induction and coercive force of the magnets. It was found that this difficulty was mainly due to varying methods of magnetic measurement, which usually included an air-gap in the magnetic circuit, so that the factors measured were only approximations to those desired. The figures actually obtained depended upon the shape of the BH curve between the two limits of remanent induction and coercive force and, as the steel manufactured by various suppliers differs in this respect, the results varied with the method employed. It is interesting to note that a committee of the British Engineering Standards Association is at present engaged in the standardization of the commercial measurements of permanent magnets for all purposes, and this should finally overcome the difficulty. It is now conceded that a further requirement, in addition to the remanent induction and coercive force, is essential for magneto work. The additional figure usually specified is the maximum product of B and H obtained on the design, which, as the author shows, approximates to the maximum energy available. A very interesting case in connection with magnetos came to my notice at this period. It was noticed that the Air Board specification could be more easily met when using magnets supplied by one manufacturer, although the mechanical grinding of the faces of these magnets was much inferior to that obtained on magnets supplied by his competitors. The figures obtained for the remanent induction and coercive force on all magnets were very similar. Some of this difficulty was undoubtedly due to the shape of the BH curve previously referred to, but it was decided to make some experiments regarding the effect of increasing the air-gap of a magneto. These results showed that in some cases it was possible to obtain higher open-circuit voltages across the primary winding of the magneto at a given speed with a larger air-gap, provided the magneto was running under

* *Journal I.E.E.*, 1920, vol. 58, p. 780.

short-circuit conditions for a short period before the tests were carried out. The voltage curves obtained before running under short-circuit conditions showed reduced values with increasing air-gap in every case, as expected. These experiments were repeated on a number of machines and, whilst on many a reduction of open-circuit voltage was obtained with increasing air-gap even after short-circuit running, the slope of this curve was comparatively slight and it was felt that the very fine tolerance allowed in the air-gap could be widened considerably without seriously reducing the ability of the machine to meet its requirements. I should like the author to say whether the results obtained in the first series of experiments, where an actual increase of voltage was obtained with increasing air-gap, were due to the fact that the particular machine was working above the maximum energy point on the *BH* curve. Another curious condition arose which, although it gave no actual trouble, caused considerable thought. This was the fact that, although condensers were tested on voltages on which it was considered a large factor of safety had been allowed, insulation breakdowns of condensers were obtained occasionally under running conditions which finally led to the test voltage being considerably increased. It was considered that the maximum value of the transient voltage at the instant of break must be comparatively high, and some attempts were made to discover if this was the case. No useful results were obtained, owing to lack of suitable apparatus, but I understand that the National Physical Laboratory have made some tests to investigate this point. I should like to know whether the author has seen any figures relating to these tests, and whether the cathode-ray oscillograph has ever been used for the purpose of measurement of this type of transient phenomenon. The bearing in the magneto contact breaker should be capable of operating satisfactorily without lubrication, and to obtain this object a fibre bush has been used for some time. Difficulty has been experienced due to the hygroscopic properties of fibre. Unless this material is well treated before it is put into position in the contact breaker, considerable trouble is experienced due to seizing and cracking. I should like to know whether any material of a non-hygroscopic nature has been used for this purpose. One of the greatest mechanical difficulties met with in the production of a satisfactory magneto was to ensure that the cam was quite concentric with the armature after the assembly of the magneto, although great care was taken in the production of the individual parts. To overcome the difficulties sometimes experienced in this connection, it was found necessary in some cases actually to machine the cam cages from their respective ball-races after assembly of the magneto. This was, of course, rather troublesome and was not desirable from the point of view of interchangeability of parts. I should like to know whether this it still a difficulty and how it is overcome in commercial magnetos. With reference to the cobalt-chrome steels which the author has mentioned, whilst these will no doubt influence design to a considerable extent, their influence would considerably increase if less expensive constituents could be used. No doubt investigations along these lines are already proceeding. The present price of cobalt-chrome steel ranges from four to five times that of tungsten magnet steel, which means that for a given expenditure on a magnet it becomes rather doubtful whether improved performance results. I should like the author to explain, in connection with his utility curve, why the utility in micromhos is stated in connection with a particular voltage.

Mr. J. D. Paton: In reference to that characteristic of steel which practically has to be destroyed before we get a true zero, as referred to by Professor Ripper, a study of Professor Bragg's recent papers on molecular structure will show that there is an internal relation between the individual atoms, which I have already tried to define in many papers as the coulomb electrostatic capacity of the constituents. I first called attention to this matter in a lecture given before the Jones and Laughlin Steel Co.'s Technical Society in Pittsburg in 1904, when referring to the extruding action which takes place in the Laval steel-refining tube furnace, and I hold that, in steel which is a composite material, there are innumerable thermo-couples, and that there is an electrostatic relation between them which is changed, under the action of heat, into Chatelier currents in the molten metal. If the disappearing point of magnetism in iron is considered, the production of these internal Chatelier effects may neutralize the magnetic property; and in molten metal, particularly iron, there is a rotation of the particles, and I claim that this action is the interrelation of the former current and the magnetic property of the iron particles. This feature is also a basis for certain reactions which take place in electric furnaces, and gives rise to phenomena which are not yet appreciated by users of electric furnaces.

Mr. G. F. Sills: Five American magnetos which I used lasted about 60 miles. They were all of the same make, tested by their own men, and were not touched by me in any way. I was finally informed that the makers attributed the trouble to the high compression of the engine, and they substituted the ordinary type of magneto, which was satisfactory. There is little doubt, however, that the design and manufacture were faulty. From what the author has told us I gather that the user of a British magneto need not be worried on account of high engine compression. I was rather surprised that the author is of opinion that "as long as there is a spark satisfactory ignition is obtained." It is generally accepted that the fattest spark is required (that is, with the contact breaker fully advanced) at the normal point for running. On the 10 magnetos I have used, the spark was much weaker when the contact breaker was fully retarded; from the author's remarks, however, it would follow that satisfactory ignition can be obtained even if the contact breaker is fully retarded, provided that a spark takes place at the correct firing point. I had a rather curious experience with the British magneto. The trouble was traced to the cams. It was found that they had been incorrectly ground, with the result that the ignition was twice as far advanced in one cylinder as it was in the other. There is little doubt

that the gradually rising cam, as described by the author, is much easier on the contact breaker generally, and preferable to the sudden-rise type. I wish all magneto makers would supply magnetos with a "flick" starting device, as now available, so as to enable an engine to be started with the absolute minimum of effort.

[The author's reply to this discussion will be found on page 486.]

South Midland Centre, at Birmingham, 27 April, 1921.

Mr. E. O. Turner: With regard to the utility curves shown in Fig. 5, it would be of interest to know whether the author measured the value of conductance which entirely extinguished the spark or that lower value which caused occasional missing to take place; the latter, in my opinion, gives rather more definite values. The author mentions 4 micromhos as a standard figure of merit to which all machines must conform when tested at high speeds, but no mention is made of low-speed tests. Under normal conditions an engine is fired with the spark taking place at the end of the compression stroke at low speeds, but considerably earlier in the stroke at high speeds; this means that at low speeds the spark takes place at a much higher compression than at high speeds. The following table gives gauge pressures at different angles of the crankshaft in a typical automobile engine of present-day design:—

Angle	0° (Top of Compression Stroke)	35° (advance)	45°	50°
Gauge pressure (lb. per sq. in.)	100	58	45	37

Owing to the increased throttling at very high speeds, the lower pressures in the table may be still further reduced. It would thus seem that the best form of ignition apparatus would be one which had a utility curve not similar to any of those shown in Fig. 2, but with a definite value at the lowest speeds and approximately half this value at maximum speeds. Such a curve I find is given by a suitably designed battery ignition coil, in which the primary circuit has a resistance with a low temperature coefficient. Curve 7 in Fig. 2 is not representative of a typical battery ignition system. The spark entirely fails above 1 000 r.p.m. although, as is well known, the engines of many American cars fitted with battery ignition run successfully at two and three times this speed. I cannot quite agree with the author that very great care is necessary in the design of a battery ignition coil, as I consider that it presents a much simpler problem than the magneto. The racing speed of an engine and the shape of the contact-breaker cam being assumed, the time during which the primary circuit of the coil is closed has a definite value at maximum speed. Definite values which are capable of easy measurement can also be assigned to the self-induction and resistance of the primary circuits; hence the time-constant of the circuit is known, and, knowing also the least time of contact, the ratio of current interrupted at high speed to the maximum current can be ascertained by means of the equation for the rise of current in an inductive circuit. If values are so chosen that at maximum speed the current interrupted is about three-quarters of the maximum current, the energy content of each spark, and thus the utility, will be constant at low speeds, falling off gradually to half this value at the highest speed at which the engine can run. It is quite likely that the increasing use which has been made of battery ignition during the past few years is due in a large measure to the shape of the utility curve, which in the case of the battery ignition resembles the ideal shape referred to above much more closely than in the case of the magneto. I had some difficulty in understanding the construction referred to in Fig. 5, and I believe it would be of assistance if the author were to add diagrams of flux-change on closed and open circuits drawn in a similar manner to the voltage diagram shown in Fig. 3, but referring particularly to the conditions obtaining in Fig. 5. In the latter figure the author apparently omits any consideration of the effects of magnetic leakage. When the contact breaker is on the point of opening, and especially at low speeds, I should expect there would be a considerable amount of flux passing from North to South pole-shoes across the pole-tips of the armature. Such flux would not link the secondary winding, and it should presumably be excluded in the determination of stored energy. It would be of interest to know if the author has measured the proportion of flux thus wasted. Reference is made on page 460 to the use of a jump-spark distributor. The author's claim, that increased utility at high speeds with this form of distributor is to be attributed to more effective spark extinction, appears to be much more sound than the explanation so frequently put forward in the past, to which he alludes. According to his theory, however, although an improved utility is to be expected at high speeds, the low-speed utility should be reduced, as the spark is so feeble as to require no assistance in extinguishing it, and the extra air-gap adds to the total resistance. This I have found in practice to be the case, and on this account I favour a rubbing contact. In the Appendix, as his second assumption, the author assigns to the air-gap a reluctance which is a function of the angle turned through beyond the mid-position. Does he imply a linear function, or is the expression worked out from tests on an actual magneto?

Professor W. Cramp: The paper lifts the subject of magnetos from the region of empirical guesswork to that of scientific design. Fig. 2 and Table 1 are very valuable as providing a starting point for manufacturers. The author has evidently a physical picture in his mind of the action of the machine, which he has endeavoured to describe but which I find very hard to follow. How

is it possible for one part of the armature face to be at zero magnetic potential while another part is at a high potential? Again, if at any time the reluctance of the magnetic circuit is to be taken as zero, then at that time the magnetic flux must be infinite, which the Appendix does not contemplate. I have every sympathy with the conception of the storage of energy in the gap, but the author's picture seems to me to be impossible. On the other hand, his method of dealing with the optimum position on the hysteresis loop deserves the closest attention. The statement on page 453 that the amount of flux distortion is independent of the speed when the armature resistance is zero, is only true if iron losses are neglected, as they appear to be throughout the paper. I doubt if this is at all justifiable, as the iron losses are usually comparatively large. In my opinion it would be better if Fig. 8 were plotted to show the relation between energy and speed, as in this connection energy is the crux of the argument. Again, Fig. 6 contradicts flatly the results of the researches of Professor S. P. Thompson, which were to the effect that there is no change in the flux through the yoke of a magnet. This is a very important point which must not be left in doubt. The author habitually speaks of increase of reluctance as though it were a demagnetizing force. This is not correct and it should be altered throughout the paper. I think also that a much fuller explanation of Fig. 22 should be given, or else that it should be withdrawn. How were these curves taken, to what scale are they drawn, and how is it possible for the lines to cross? Is the zero of the scale along the face of the magnet, for, if so, how is it that the potential is all positive? The distribution of potential is a subject upon which we have taken a number of tests at Birmingham University, and the author's figures do not agree with either the expectations of theory or the results obtained in practice. The author's statements with regard to the effects of short-circuiting the primary with the engine running at full speed are very important and should result in the omission of the usual short-circuiting switch in car equipments. I should like to ask, however, whether, even after a severe short-circuit of this kind, it is not possible for the magnet slowly to recover to some extent? Has the author ever tried to measure mechanically the energy due to a given displacement of the armature? It seems to me quite possible to accumulate the energy of the return stroke by means of a moving mass, and this would render unnecessary much which had to be arrived at in the paper by intelligent guessing and clever mental pictures.

Mr. W. Wilson: One of the most interesting diagrams in the paper is that showing the effects of a good and a bad plug respectively, upon the performance of the magneto, and I should be glad if the author would indicate what are the characteristics of the "bad" plug that were responsible for so great a difference between the two curves. In 1907 it was the custom to build up the magnet in from two to six portions. The object of this was once stated in my hearing to be the increase of the steel surface, rendered necessary by the superficial nature of the magnetization. This explanation does not sound very convincing, as the magnets are not formed by the "stroking" process. It would be interesting to know why these multiple magnets were once used and have now been abandoned. With regard to the serious effect of short-circuiting a magneto, a machine I once possessed was fitted with a species of compound winding, apparently with a view to the protection of the permanent magnet during such a contingency. Upon each pole was fitted quite a large series coil, which, of course carried the full current delivered by the machine. What is the author's opinion of this device?

Captain N. F. Cave-Browne-Cave: The reference in the paper to the advantages of an "outside" spark-gap is especially interesting to users of these devices. I should be glad to have some information as to the use of vacuum tubes as outside spark-gaps. In the case of a magneto with a brush high-tension distributor, separate open 1/16-inch outside gaps over each plug have given good results. In cold weather starting difficulties are often serious, and any indication of the trouble is welcome. Even when no one is available to watch these gaps for sparks, I find I can hear the outside spark if the mixture does not fire when the engine is cranked. Inability to hear the outside spark indicates that the failure to start is due to a sticking rocker arm. If we accept the statement made in the paper that any mixture which can be ignited will be exploded by any sort of a spark at the plug points, we can eliminate at once one possible cause of failure. I have heard of occasional troubles with enclosed American jump-spark high-tension distributors, namely, short-circuits and earths caused by particles of metal shot off by the sparks. I have also heard of cases where a magneto, also American, was a total failure at starting, until the capacity of the condenser was halved. The paper demonstrates that the magneto is a highly complex machine. There is even a further very common type, which is not mentioned in the paper, i.e. the rotating-magnet type as used on Ford and other cars. Modern practice on large cars necessitates a direct-current generator and accumulators. Can we expect the high-tension magneto to survive much longer on such cars? Why cannot the expensive and weighty permanent magnets and armature be satisfactorily replaced by an induction coil energized from the accumulators? The case is, of course, different when a combination such as a Mag-dyno or Mag-lita is possible; but these do not appear at present to have been developed for large four-cylinder cars. When accumulators are available for difficult starting there is further the possibility of using an electrostatic or influence machine of, say, the Wimshurst type if, as the curves indicate, battery ignition is difficult at high speeds. It can be made equally small, is much lighter, has no permanent magnets, brush gear, armature windings, or even a make-and-break; it can be belt- or friction-driven if the jump-spark distributor be direct driven, and it is very simple and cheap to make. The distributor would be adjustable to advance or retard the spark. I saw the author experimenting with such a machine many years ago, and a statement as to its "utility" and possibilities would be interesting.

The paper brings out the fact that the magneto actually causes the mixture to explode by means of an electrostatic discharge, after several transformations of energy. Should we not, therefore, attempt to cut out these transformations by direct electrostatic generation?

Dr. D. K. Morris : The theory of machines which have to work with heavy flux distortion has been developed comparatively recently. An example is the eddy-current brake, the theory of which interested Mr. Lister and myself in 1904. But in the magneto, excessive flux distortion is promptly followed by no distortion at all. The author has acquired such insight into these highly complex conditions that he is able to give a simple general theory. The great importance attaching to that magnetic condition of a permanent magnet in which the quantity BH has its greatest (negative) value is well brought out in the paper. It is that state of partial demagnetization which must be reached but not exceeded in a magneto in order to utilize the permanent magnet to the utmost, and the value of this negative maximum is therefore the proper measure of the excellence of a magnet steel which has to stand up permanently against a fluctuating demagnetizing force. It is the greatest value of BH, and not coercive force or remanence or their product, which will specify such a magnet. An example showing how the maximum BH may differ in samples having the same coercive force and remanence might with advantage have been included in the paper. I would also suggest that the author's simple direct-reading indicator of this simultaneous product should have been described and illustrated.

Mr. E. A. Watson (*in reply, communicated*) : As several speakers in the discussions have raised similar points I think the best method of dealing with their remarks in replying to the discussion will be first of all to review briefly the paper and the comments upon it, before finally dealing with various speakers' remarks in detail.

Taking first the broad principles of ignition, I agree with Mr. Paterson's statement that the energy required to produce ignition is extremely small compared with that necessary to overcome defects of sparking plugs and cables ; but I agree also with Mr. Young that this statement is only true when the fuel employed is a truly gaseous mixture, and that it does not hold under starting conditions when the fuel is partly in the form of a mist of small globules of unvaporized petrol. It is certainly true, however, that the spark energy has no effect upon the horse-power which the engine develops, and the only way in which a feeble spark can cause loss of power is through actual missfiring. In this connection I would mention a rather interesting experience obtained when experimenting on a petrol engine with the influence machine referred to by Captain Cave-Brown-Cave. With this machine there was no perceptible difference in running under normal conditions between the influence machine and a magneto or battery and coil, but in cold frosty weather it was absolutely impossible to get the engine to start. The spark from such a machine consists of a capacity component only, and, as has been pointed out by many workers, it is the capacity component which produces ignition under normal conditions when the engine is running with a truly gaseous mixture. The fact, however, that the engine would not start when cold, with a spark of this nature, shows, I think, that with improperly vaporized fuels the capacity component only is not sufficient. The same ignition functioned absolutely perfectly on a gas engine, both at starting and when running. Exactly how much heat energy is required to give good starting with various fuels is a matter upon which considerable experimenting might be carried out, but it is a matter of great difficulty because of the tremendous variations which will occur due to alterations in temperature and quality of fuel.

Dealing next with the principles of operation and theory of the machine, Dr. McLachlan emphasized the importance of the ratio CV^2/LI^2 for a magneto. I quite agree that this ratio is of very great importance indeed, particularly when the magneto is working at low speeds, which generally give most trouble to magneto designers and manufacturers. The higher this ratio, the better will be the low-speed performance of a magneto with a given field system. The ratio, as Dr. McLachlan observes, is very greatly affected by hysteresis and eddy currents, and its value can be increased by careful lamination of the magnetic circuit. In modern magnetos I think a compromise has been arrived at between the performance of a machine on the one hand, and the cost of manufacture on the other, and that the machines which are now being made by the principal magneto manufacturers are really giving the optimum performance for a given cost of material. Experiments have been carried out with much thinner armature laminations, as low as 0·002 in., but I understand that very little improvement was obtained in the performance of the machine, probably due to the poorer space-factor which would be obtained with laminations of this thickness.

Owing to the necessity of providing for good low-speed performance, the $\frac{1}{2}CV^2$ component of the spark is relatively small at high speeds, and the majority of the energy at high speeds is, as Dr. McLachlan points out, in the flame which follows the initial spark. This would appear to be unavoidable, and the only way to overcome it would be to obtain a primary winding of practically no resistance, or a material with no reluctance for the armature core, neither of which essentials is, of course, likely to be obtainable.

The relationship of utility to the number of secondary turns and the other constants of the machine presents an exceedingly difficult problem, but I think Dr. Bairsto has put this very clearly in his remarks, and also in the paper to which he refers. Utility is dependent not only upon the maximum voltage which the machine can give on open circuit, but also upon the rate of rise of potential due to the fact that the machine has not only to charge up the terminal capacity to the sparking voltage, but also to provide the leakage which is occurring while it is being charged up. We must not lose sight of the fact that with most spark-gaps the sparking potential will be somewhat dependent upon the rate of rise due to the impulse ratio of the gap. With a

magneto working at a pressure nearly equal to its open-circuit voltage, an increase in utility may sometimes be obtained by an increase in the number of the secondary turns, but when the machine is working at a pressure low compared with its open-circuit voltage the reverse is generally the case and, in the extreme case when a magneto is practically short-circuited, the utility would be, as stated in the paper, inversely proportional to the number of secondary turns. A magneto running at high speeds under working conditions very nearly approximates to the short-circuit condition, as the voltage of the plug is only approximately one-third of that which would be given on open circuit, and the $\frac{1}{2}CV^2$ component only about one-ninth of that which the machine is capable of giving.

I regret to say that I completely fail to understand Dr. McLachlan's treatment of magnetic systems as a source of energy, neither can I understand the paper to which he refers. There is certainly a storage of energy in the small clearance between the armature and the pole-faces, and also in the armature core itself. This energy is not, however, available, as it is associated with the system when the armature is in the position of minimum reluctance, that is, directly across the poles of the magnet. As the armature rotates, the reluctance of the path which the flux traverses through the air between the armature and pole-tips is increased, and consequently the energy stored in this space is also increased. The flux is constrained into the position giving this energy storage by the current flowing in the primary winding, and on the removal of this constraint takes up a position in which the reluctance is very little different from that associated with the position when the armature is directly bridging the poles. The difference between the energy stored with the flux in the constrained position, and that stored in the position after removal of constraint is available for the spark, and I cannot see how this is connected with the size of the air-gap. The air-gap reluctance will certainly diminish the amount of energy which can be obtained from a given magnetic system, as the storage of energy in the constrained position is determined by the magnet, and the available energy is this quantity less that in the position of minimum reluctance. The higher this latter quantity, therefore, the less will be the net energy available. Neither can I see how the use of a steel of a higher coercive force will enable a larger air-gap to be used. It will certainly enable smaller magnets to be employed, but it would be very uneconomical to lengthen the air-gap and increase the magnet length to correspond, as the cost of providing a small air-gap is only a trifle of the cost of the extra magnet steel which would be required.

It is rather difficult at first to see why the total number of lines of force stretched out across the fields should be equal to twice the working flux. If Dr. McLachlan will draw diagrams of the magneto in successive positions, or, better still, make a little cardboard model, he will see that the total flux linking the armature is drawn out at each pole-tip, and the two pole-tips give the doubling of the flux to which he refers. Another way to look at it is to say that, of the flux distorted across the tips, approximately half is provided by the magnet and the remainder by the short-circuit current in the primary winding.

I think that Professor Cramp's criticism arises to a great extent from the fact that he has been considering the magneto as if it were a dynamo. The two faces of the armature which are at different magnetic potentials at any moment during operations are at two opposite extremities of the core, and the difference of potential is due to the current in the primary winding. With a permanent magnet the flux will not be infinite, even if the reluctance of the armature and air-gap is zero, as the flux can never be greater than that which the magnet is capable of giving on short-circuit. In all well-designed magnetos the iron losses in the armature have very little effect upon the cycle of operations up to the moment at which the contact-points separate. After that I admit they are of importance, but their bearing upon the amount of flux distortion is negligible.

I am of opinion that the reason the late Professor Thompson obtained no change in flux in the particular case mentioned was that the magnet was a very long one.

The tests, the results of which are shown in Fig. 22, were made by means of a Chattock potentiometer, and the ordinates on the diagram represent the magnetic potential between two points 1 cm. apart. The area of the diagram enclosed between the curve and the outside of the magnet which has been used as the base line will therefore represent the magnetic potential between the two ends of the magnet. I must say that I have never observed any slow recovery of a magnet after short-circuiting the magneto in the way which Professor Cramp suggests, and I am very doubtful whether such recovery takes place.

The linear equation which Professor Crapper gives is of very great value in dealing with magnet steel, as it enables the whole demagnetization curve to be constructed from three constants, which may be readily memorized if necessary. The constants which I prefer, personally, are saturation density, remanence, and coercive force, as if these are given it is possible to use a graphical construction by means of which the whole of the demagnetization loop can be very quickly drawn.

I am very interested in the remarks of Professor Crapper and Messrs. Naylor and Kayser in regard to the new cobalt steels. These steels offer very great possibilities, both for magnetos and also for other pieces of electrical apparatus. To make the best use of them it is necessary that the magnets should be carefully and scientifically designed, and not, as has been the case in the past, proportioned by rule of thumb or guesswork. One of the great advantages of the new steels is that they offer such a wide range of variation of properties. For instance, one can obtain a steel with a very high coercive force of 250 or even more, with a comparatively low remanence, or a high remanence with a more moderate coercive force, and these render it much easier to obtain a magnet to suit a particular design of magneto than is the case when the properties of the steel can only be varied within very narrow limits. At the present time the cost of these steels is undoubtedly against their use, as for the same amount of stored energy they cost slightly more than tungsten steel. On the other hand, they

simplify design considerably and enable many incidental savings to be made. Our own experience also is that they are easier to work than tungsten steels, or at least in the air-hardening variety, as the amount of scrap due to cracks and hardening defects is very much less. Some of these steels give really remarkable results, and at the present time we are producing quite a large number of magnets with a $(BH)_{max.}$ of between 900 000 and 1 000 000 and a coercive force of over 250.

Professor Marchant's contribution to the discussion gives an account of an investigation which should be of considerable help to any one engaged on ignition problems, as it gives valuable data of the duration of the spark under different conditions, and also of the nature of the current-wave associated with it. It shows very clearly that at high speeds and under running conditions the spark is very far from being an instantaneous discharge. Professor Marchant records spark durations of the order of 0·007 to 0·008 sec. When it is remembered that some types of modern engines develop their maximum power in the neighbourhood of 3 000 r.p.m., it will be seen that the spark in such a case lasts very nearly the whole of the working stroke, and through an angle of something like 150°. It is clear that under these conditions there will be a very great tendency for excessive armature reaction in the magneto, unless proper steps are taken to guard against it. It is of interest also to note that Professor Marchant confirms that the working voltage is comparatively low. The particular type of engine on which he experimented would probably not give such high figures for the working voltage as a number of the modern high-efficiency engines now being used, so that his figure of one-quarter of the maximum may occasionally be exceeded. Very much depends upon the position of the sparking plug in the cylinder, as well as upon the compression ratio, and there are certain types of engines at present on the road in which the plug voltage will rise as high as 10 000 or 11 000 when the engine is pulling fairly slowly up a hill, with a large throttle-opening. It is on engines of this type that certain sparking plugs with unsatisfactory characteristics occasionally give trouble through failure of the ignition, the magneto sparking at the safety-gap instead of at the plug. Professor Marchant also mentions the rotary spark-gap, which is of considerable use for investigating the length of spark duration and also as a works' apparatus for investigating evenness of firing at high speeds.

Mr. Young's review of the general problems of magneto manufacture is, I think, very opportune, as it draws attention to the many difficult problems which are connected with it, and which are hardly appreciated on the first inspection. I quite agree with him as to the difficulty of the problem associated with the contact breaker, but I do not quite agree with his estimates of the acceleration forces, which I think he has very much under-estimated. The reason is that it is not possible to work a magneto with a contact breaker in which the movement of the arm is a simple harmonic motion. This would, of course, give the least stress in the moving parts, but it would mean that the point of break was very indefinite, and on this account the ignition would be unsatisfactory. In all contact breakers at present employed, the fibre heel in the rocking arm strikes a surface making a definite small angle with the direction of motion of the heel, and the latter suddenly acquires a finite velocity at right angles to this surface. This gives a perfectly definite point of break, but it means that the stresses in the heel and rocker are indeterminate at the moment of impact, and would be infinite if the moving parts were perfectly rigid. The velocity of the heel normal to the cam is in most designs a maximum immediately after impact, and the duty of the spring fitted to the arm is to bring the moving part to rest within the space allowed by the lift of the cam, that is, the spring must cause the fibre heel to follow the cam contour, and not allow it to be flung off. This is quite a difficult problem at high speeds and means that the weight of the moving parts must be kept as small as possible. I am quite in agreement with Mr. Young's remarks regarding tungsten. It is not such an all-round satisfactory material as the platinum-iridium alloy, but I think it can be used if the current which the contacts have to break is kept within certain well-defined limits. It has the undoubted advantages of being very much cheaper and also considerably harder than platinum.

Captain Burgoine's remarks deserve careful attention, in view of the fact that he has been intimately associated with the development of aircraft magnetos. There is no doubt that the experience so gained has been of very great service indeed to British magneto manufacturers, and I have no doubt that it will continue to be so in the future. The design of a magneto to meet the latest requirements of the Air Ministry is certainly a difficult problem, but two or three firms are prepared to tackle it, and I have no doubt that before much longer a machine will be produced which will fulfil all their conditions. Captain Burgoine's remarks about the workmanship of German and British magnetos are quite justified. There is no doubt that the German manufacturer has the great advantage of possessing a large number of workmen thoroughly conversant with the manufacture of small and intricate mechanism and used to giving attention to minute details. The German workman seems to be of a temperament particularly suitable to this class of work, and it is a very difficult matter to instil the same spirit into the British workman.

The question of battery versus magneto ignition has been raised by two or three speakers, and in particular by Mr. Turner. I had hoped that the battery-ignition controversy would be kept out of the present discussion, which should merely deal with the technical aspects of magnetos themselves. I should have stated, in justice to battery ignition, that curve 7 in Fig. 2 was for a 6-cylinder ignition set, so that 1 000 r.p.m. on the ignition head corresponds to 3 000 r.p.m. of the engine, and I think Mr. Turner will agree with me that many battery-ignition sets, of the cheaper sorts at all events, fail at speeds of this order. I agree with him that the battery ignition set gives a characteristic which is very suitable for moderate-speed engines. At all events they give very good starting, particularly with low-grade fuels, and no trouble at ordinary engine-

speeds. I cannot agree with him that the design of a satisfactory battery set is quite so simple. The reason is that, with a fairly high-speed engine, if the inductance and resistance are so proportioned as to give a reasonable utility at high speeds, the current consumption of the set at low speeds will be excessive. I note that Mr. Turner suggests running with an interrupted current of three-quarters of the short-circuit current at maximum speed, but I should like to point out that with this ratio the set will be very inefficient and the amount of energy delivered at the plug points will be small compared with the input from the battery. The point at which Mr. Turner works corresponds very nearly to that which gives the maximum spark energy for a given primary closed-circuit current. If we suppose the primary resistance and duration of contact to be fixed, it is clear that there will be a value of the primary inductance which will give the maximum stored energy, for if the inductance is zero there will clearly be no stored energy, and if the inductance is infinite the current will never rise. The value of the inductance for which the stored energy is a maximum can be shown to be equal to $0{\cdot}8\ Rt$, where R is the primary resistance and t the duration of the closed circuit. From this we obtain

$$\text{Current at break} = 0{\cdot}714\ I_0$$
$$\text{Stored energy} = 0{\cdot}203\ Rt \times I_0^2$$

The corresponding efficiency, i.e. the ratio of stored energy to input, is almost exactly 25 per cent. I admit that with comparatively large dynamos, such as are needed when a self-starter is fitted, the low efficiency is not of great importance provided it does not cause excessive heating, but it would become of importance on small cars, where for the sake of price a self-starter is dispensed with, and only a small dynamo is employed. I would also say that modern magnetos can be made to give a characteristic very much better for starting and low-speed running than the old types of machines, and I think that in the great majority of cases the question of starting and slow running is more a problem of the carburettor than of the ignition system. To my mind the great weakness of all battery-ignition systems is the possibility of the battery becoming run down through inadvertence or neglect and the fact that once this has happened the car becomes immovable. I understand that in America a system of service depots has been developed to such an extent by the battery companies that it is only a matter of telephoning to the nearest one for a new battery, but I hardly think this will appeal to British motorists. It is sufficiently annoying to leave a car standing at night and come back to find that the lamps are out; but how much more annoying would it be to know that this meant that there was no chance of starting the engine again.

In conclusion I should like to take the opportunity of adding to that portion of the paper which deals with the storage of energy in the magnetic field. This seems to have aroused some criticism and to be not very generally understood, and I think the following remarks may help to make matters a little clearer.

Imagine two flat magnetic faces parallel to one another at a distance x apart. Assume their area to be 1 sq. cm., and the flux density between them B. Then, writing H for the magnetic potential between the faces, we have

$$H = Bx$$

A change in either B or x will cause a change in H, so that we have

$$dH = B\,dx + x\,dB$$

Now the force between the two faces is given by the expression

$$F = B^2/(8\pi)$$

and if the faces be separated by a small distance dx we obtain

$$\text{Work done} = F\,dx = \frac{B^2}{8\pi}dx = \frac{B}{8\pi}(dH - x\,dB)$$
$$= \frac{1}{8\pi}(B\,dH - H\,dB)$$

so that the total work done becomes

$$\frac{1}{8\pi}\int B\,dH - \frac{1}{8\pi}\int H\,dB \quad . \quad . \quad . \quad . \quad (1)$$

Now this is necessarily equal to the energy stored plus any electrical work done on the circuit.

Suppose the M.M.F. of H to be produced by a coil which links the field B and in which a current I flows in n turns.

$$\text{Then } H = 4\pi nI; \quad \text{or} \quad I = \frac{H}{4\pi n} = \frac{Bx}{4\pi n}.$$

The E.M.F. produced due to any flux-change is $v = n\dfrac{dB}{dt}$ so that the electrical work due to a flux-change dB is

$$vIdt = n\,dB \times \frac{Bx}{4\pi n} = \frac{H\,dB}{4\pi}$$

and the total work done is equal to

$$\frac{1}{4\pi}\int H\,dB \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Consider first the case where the field is distorted without change.

Since B does not change, $dB = 0$, and Equation (1) becomes :—

$$\text{Work done} = \frac{1}{8\pi}\int B\,dH$$

which, since B is constant, may be written

$$\frac{B}{8\pi}(H_1 - H_0) \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

H_1 and H_0 being the values of the M.M.F. before and after distortion.

Further, since dB is zero, expression (2) is equal to zero and there is no electrical work done, the whole of the mechanical work done appearing as stored energy.

Now let us assume the two faces to be held stationary while B is reduced to zero.

Since there is no movement of the faces no work can be done, so that we may write

$$0 = \frac{1}{8\pi}\int B\,dH - \frac{1}{8\pi}\int H\,dB$$

$$\int B\,dH = \int H\,dB$$

The electrical work given to the circuit is

$$\frac{1}{4\pi}\int H\,dB = \frac{1}{4\pi}\int Bx\,dB$$

$$= \frac{B^2x}{8\pi} \text{ (since } x \text{ is constant)}$$

$$= \frac{BH}{8\pi} \quad \ldots\ldots\ldots \quad (4)$$

This is the same as (3) above, when H_0 is zero, e.g. if in a magneto we take a field initially at zero potential and raise it without change to potential H we store energy $BH/(8\pi)$, and if we then let it collapse we obtain this same energy (neglecting losses) electrically in the form of a spark.

Now let us assume that during the collapse of the field the faces continue to move apart at such a rate that H remains constant. Clearly, work must still be done as there is a force between them.

From Equation (1) we see that this work is

$$\frac{1}{8\pi}\int B\,dH - \frac{1}{8\pi}\int H\,dB$$

Since H is constant dH is zero, while dB must be negative, as the flux must diminish in order to maintain a constant potential across an increasing distance. Hence the work done becomes

$$\frac{1}{8\pi}\int H\,dB$$

and if we continue the motion until B becomes zero we have

$$\text{Work done} = \frac{BH}{8\pi}$$

The total work done in the two stages, (*a*) in distorting the field up to potential H, and (*b*) during its collapse, is

$$\frac{BH}{8\pi} + \frac{BH}{8\pi} = \frac{BH}{4\pi}$$

The electrical energy given to the circuit during the collapse of the field is

$$\frac{1}{4\pi}\int H\,dB$$

Since H is constant this becomes $\frac{BH}{4\pi}$, or twice that given by Equation (4), and equal to the total work done in the two stages.

This last result represents the maximum energy which can be obtained in the spark and arc combined; Equations (3) and (4) give the energy which can be obtained in the spark itself.

As these changes and energy storages take place under both pole-tips, the total energy available (neglecting losses) will be twice these amounts, or $\frac{BH}{4\pi}$ and $\frac{BH}{2\pi}$ respectively.

HARMONIC ANALYSIS BY SELECTED ORDINATES.

AN INVESTIGATION INTO THE ACCURACY OF THE RESULTS OBTAINED BY USING SUCH METHODS FOR THE ANALYSIS OF WAVE-FORMS NORMALLY MET WITH IN ELECTRICAL PRACTICE.*

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Summary.

In this paper the accuracy of the Perry method of harmonic analysis by means of selected ordinates, and also of its Runge and Thompson simplifications, is investigated. It is shown that such methods are theoretically exact, provided that the number of ordinates chosen per half period is greater than the order of the highest harmonic present. Further, it is shown that the value deduced for the amplitude of the pth harmonic is theoretically exact in all cases when higher harmonics of the orders $2Mn \pm p$ are absent, M being any integer, and n the number of ordinates per half period; no error whatever is introduced by the presence of harmonics of any other orders. The value A'_p actually obtained for the amplitude of the pth sine term is shown to be:

$$A'_p = A_p - (A_{2n-p} - A_{2n+p}) - (A_{4n-p} - A_{4n+p}) \ldots$$

where A_p, A_{2n-p}, etc., are the actual amplitudes of the sine harmonics of the orders p, $2n - p$, etc.

Similar deductions are obtained for the case of the cosine terms, the value B'_p obtained being shown to be:

$$B'_p = B_p + (B_{2n-p} + B_{2n+p}) + (B_{4n-p} + B_{4n+p}) \ldots$$

The errors obtaining in the values of the amplitudes of the various harmonics as deduced by the use of selected ordinates are thus calculated for the case of a rectangular wave-form, and found to agree with the actual errors obtained.

It is further shown that under certain circumstances the methods of analysis under discussion can be utilized to determine the amplitudes of ripple harmonics of higher orders than the number of ordinates per half period, and that, without taking an inconveniently large number of ordinates, the method of selected ordinates can be used to give accurate results for all normal wave-shapes met with in electrical engineering. The conditions necessary to obtain reliable results are discussed.

In the second part of the paper a practical schedule for the analysis of wave-forms up to the 25th harmonic is given. The schedule is so arranged that the same tabular form can be utilized either for sine or cosine terms, thus obviating the need for separate schedules for sine and cosine terms.

The third part of the paper gives a brief review of three other methods of practical analysis of wave-forms, viz. Wedmore's graphical method, Fischer Hinnen's and Thompson's method of harmonic analysis by the addition of selected ordinates, and Russell's method of applying Weddle's rule to the determination of the values of the various Fourier integrals. The accuracy of the Russell method is investigated, and the effect of any particular harmonic upon the value computed by this method for the amplitude of any other harmonic determined.

* The Papers Committee invite written communications (with a view to publication in the *Journal* if approved by the Committee) on papers published in the *Journal* without being read at a meeting. Communications should reach the Secretary of the Institution not later than one month after publication of the paper to which they relate.

List of Symbols.

A_1, A_2, etc. = the amplitudes of the sine terms of the first, second, etc., orders.

A'_p = the computed value of the amplitude of the pth sine term.

A_p = the correct value of the amplitude of the pth sine term.

A'_{p_q} = the effect of the qth sine term upon the value computed for the amplitude of the pth sine term.

B_1, B_2, etc. = the amplitudes of the first, second, etc., cosine harmonics.

B'_p = the computed value of the amplitude of the pth cosine term.

B_p = the correct value of the amplitude of the pth cosine term.

B'_{p_q} = the effect of the qth cosine term upon the value computed for the pth cosine term.

M = any integer.

m = $n/6$ (see below).

n = the number of parts into which the half period of the wave-form is divided. (For the Runge method and its Thompson simplification, n is an even number, and for the Russell method n is a multiple of 6, and equal to $6m$.)

p = any odd number, and is used generally to represent the order of the harmonic under investigation.

q = any odd number representing the order of any harmonic.

S = the number of slots per pole on the armature.

S_c = the number of slot pitches per pole on the field system, for cylindrical-field machines.

y = the ordinate of the wave at a distance of θ radians from the origin.

y_1, y_2, etc. = the values of the ordinates at points distant π/n, $2\pi/n$, etc., radians from the origin.

y_{10°, etc. = the value of the ordinate at a point 10°, etc., distant from the origin.

ϕ_p = the phase displacement of the pth harmonic.

$\omega = 2\pi \times$ frequency.

$\gamma = \pi/(6p)$.

Introduction.

Of the methods in use for the mathematical analysis of wave-forms into their component harmonics, the one most favoured by electrical engineers is probably that first put forward in this country by the late Dr. Silvanus P. Thompson.*

This method is based upon that previously developed by Professor C. Runge,† Thompson effecting a considerable simplification by dealing solely with odd harmonics, whereas Runge dealt with the perfectly general case.

The principle upon which the Runge method is based had in turn been presented by the late Professor Perry; ‡ Runge, and, later, Thompson, reducing the labour involved by the original Perry method by grouping selected ordinates in a suitable manner.

It has been realized from the very outset that any method based upon the values of a limited number of ordinates cannot possibly give exact results in all cases, since it is possible to have an infinite number of wave-forms each of which will pass through the measured points on the wave. As a result, the Perry and Runge methods cannot possibly give exact results in all cases. In particular, appreciable errors may occur in cases where the very high harmonics are extremely pronounced, and where the subdivision of the period is limited. P. Kemp § has recently illustrated this point by tabulating the error obtained by applying the Thompson modification of Runge's method to the case of rectangular and triangular wave-forms.

The chief object of the present paper is to give an investigation into the reasons for the possible inaccuracy of the Runge method as applied to the analysis of wave-shapes occurring in electrical practice; to show how the error can be predetermined under given conditions; to show how the presence of any particular harmonic beyond the order to which the analysis is carried may influence the value determined for any particular lower harmonic; to show how, in many cases, the error obtaining in the value computed for any particular harmonic can be made to furnish useful information regarding the amplitude of an harmonic beyond the normal scope of the analysis; and to establish the conditions which should be fulfilled in order that the method may be employed to give reliable results in all normal cases.

In addition, the object of the present paper is also to give a brief review of the more important of the other methods that have been put forward for the practical analysis of wave-forms, and to give a critical comparison of the various methods, both from the point of view of accuracy, when applied to wave-shapes normally met with in electrical practice, and also from the point of view of the labour involved in their practical application.

In this connection, mention should be made of the valuable contribution * that Dr. A. Russell has made to the theory, etc., of the various "series" methods of analysing harmonic curves.

The manner of applying the Runge method for the analysis of alternating-current waves has been given by F. W. Grover,† but, so far as the author is aware, the exact determination of the accuracy of the various methods in use for approximating to the values of the Fourier integrals concerned in the harmonic analysis of wave-forms has not been presented previous to the present paper.

(1) The Accuracy of the Perry Method of Harmonic Analysis, and of its Runge and Thompson Simplifications.

In the present paper it is proposed to confine attention to the case normally met with in alternating-current engineering, viz. of wave-forms containing only odd harmonics. Under these conditions the Fourier series representing an alternating current or electromotive force will be of the form:

$$y=(A_1\sin\theta+A_3\sin 3\theta+A_5\sin 5\theta+\ldots+A_p\sin p\theta+\ldots)+(B_1\cos\theta+B_3\cos 3\theta+B_5\cos 5\theta+\ldots+B_p\cos p\theta+\ldots)$$

To determine the values of the various harmonics, Fourier multiplied the above equation throughout by the sine, or cosine, of the particular harmonic under consideration, and integrated the product over the complete period. To determine, for example, the value of A_p, the amplitude of the pth sine term, the series is multiplied throughout by $\sin p\theta\, d\theta$, and integrated between the limits 0 and 2π. Thus:

$$\begin{aligned}\int_0^{2\pi} y\sin p\theta\, d\theta &= \int_0^{2\pi}\Big[\big\{A_1\sin\theta\sin p\theta\, d\theta+A_3\sin 3\theta\sin p\theta\, d\theta+\ldots\\ &\quad +A_p\sin p\theta\sin p\theta\, d\theta+\ldots\big\}\\ &\quad +\big\{B_1\cos\theta\sin p\theta\, d\theta\\ &\quad +B_3\cos 3\theta\sin p\theta\, d\theta+\ldots\\ &\quad +B_p\cos p\theta\sin p\theta\, d\theta+\ldots\big\}\Big]\\ &= \pi A_p\end{aligned}$$

Hence
$$A_p=\frac{1}{\pi}\int_0^{2\pi} y\sin p\theta\, d\theta$$

and, similarly,
$$B_p=\frac{1}{\pi}\int_0^{2\pi} y\cos p\theta\, d\theta$$

Provided the value of the Fourier integrals can be obtained accurately, therefore, an alternating wave-form can be resolved accurately into its component

* S. P. Thompson: "A New Method of Harmonic Analysis by Selected Ordinates," *Proceedings of the Physical Society*, 1904, vol. 19, p. 443; see also *Electrician*, 1905, vol. 55, p. 78.

† C. Runge: "Über die Zerlegung empirisch gegebener periodischer Funktionen in Sinuswellen," *Zeitschrift fur Mathematik und Physik*, 1903, vol. 48, p. 443.

‡ *Electrician*, 1892, vol. 28, p. 362.

§ P. Kemp: "A Practical Method of Harmonic Analysis," *Journal I.E.E.*, 1919, Supplement to vol. 57, p. 85.

* A. Russell: "Practical Harmonic Analysis," *Proceedings of the Physical Society*, 1915, vol. 27, p. 149.

† F. W. Grover: "Analysis of Alternating Current Waves by the Method of Fourier, with Special Reference to the Methods of Facilitating the Computations," *Bulletin of the Bureau of Standards*, 1913, vol. 9, p. 567.

harmonics. To effect the required integration, Perry divided the whole period into a number of equal parts, the ordinates being y_1, y_2, y_3, etc., and A_p was taken as twice the mean value of $y \sin p\theta$ as determined from the measured ordinates.

When dealing with odd harmonics only it is not necessary to deal with the complete period, so that in the Thompson simplification of Runge's method the half period is divided into n * equal intervals as in Fig. 1, the ordinates being y_1, y_2, y_3, etc., and each interval corresponding to π/n radians.

The method devised by Runge is based upon exactly the same principles as that due to Perry, but by grouping the ordinates of the wave-form in a suitable manner, and by suitably choosing the number of ordinates, a considerable reduction in the number of necessary multiplications is effected, and when, as was put forward by Thompson, the Runge method is limited to the case

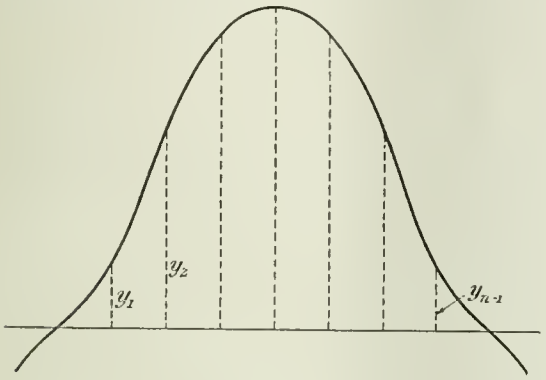


FIG. 1.

of odd harmonics only, a beautiful simplification is possible, and the labour involved is reduced to about one quarter.

With any of these methods, then, if A'_p represents the value computed for the amplitude of the pth sine term, as distinct from A_p, its real value:

$$A'_p = 2\frac{1}{n}\sum\left(y_1 \sin\frac{p\pi}{n} + y_2 \sin\frac{2p\pi}{n} + y_3 \sin\frac{3p\pi}{n} + \ldots \text{ to } n \text{ terms}\right) \ldots \quad (1)$$

Similarly, for B'_p, the value computed for the amplitude of the pth cosine term is:

$$B'_p = 2\frac{1}{n}\sum\left(y_1 \cos\frac{p\pi}{n} + y_2 \cos\frac{2p\pi}{n} + y_3 \cos\frac{3p\pi}{n} + \ldots \text{ to } n \text{ terms}\right) \ldots \quad (2)$$

Any errors resulting from the use of the methods under consideration can, therefore, only be due to errors thus introduced in the determination of the Fourier integrals. To determine the accuracy of the methods, therefore, it is necessary to examine in detail the two above series, (1) and (2).

Consider first the case of the sine terms. Let p be the order of the harmonic whose amplitude is required, and let q be the order of any other harmonic. Since only odd harmonics are met with in normal practice, attention will be confined to the case of odd harmonics. Hence both p and q are odd numbers.

The value obtained for the amplitude of the pth harmonic is:

$$\begin{aligned} A'_p &= 2\frac{1}{n}\sum\left(y_1 \sin p\frac{\pi}{n} + y_2 \sin\frac{2p\pi}{n} + \ldots \text{ to } n \text{ terms}\right) \\ &= 2\frac{1}{n}\left\{\sin p\frac{\pi}{n}\left(A_1 \sin\frac{\pi}{n} + A_3 \sin\frac{3\pi}{n} + .. + A_q \sin\frac{q\pi}{n} + ..\right)\right. \\ &\quad + \sin\frac{2p\pi}{n}\left(A_1 \sin\frac{2\pi}{n} + A_3 \sin\frac{6\pi}{n} + .. + A_q \sin\frac{2q\pi}{n} + ..\right) \\ &\quad \left. + .. \text{to } n \text{ terms}\right\} \end{aligned}$$

For terms containing the qth harmonic the total effect upon the value of A'_p becomes:

$$\begin{aligned} A'_{pq} &= 2\frac{1}{n}\sum\left(A_q \sin\frac{p\pi}{n}\sin\frac{q\pi}{n} + A_q \sin\frac{2p\pi}{n}\sin\frac{2q\pi}{n} + \ldots \text{ to } n \text{ terms}\right) \\ &= \frac{2}{n} A_q \sum\left\{\frac{\cos(q-p)\,\pi/n - \cos(q+p)\,\pi/n}{2} + \frac{\cos 2(q-p)\,\pi/n - \cos 2(q+p)\,\pi/n}{2} + \ldots \right. \\ &\quad \left. + \text{ to } n \text{ terms}\right\} \\ &= \frac{2}{n}\left\{\left(\frac{A_q \cos[(q-p)(n+1)\pi/2n]\sin[(q-p)\pi/2]}{2\sin[(q-p)\pi/2n]}\right)\right. \\ &\quad \left.-\left(\frac{A_q \cos[(q+p)(n+1)\pi/2n]\sin[(q+p)\pi/2]}{2\sin[(q+p)\pi/2n]}\right)\right\} \ldots \quad (3) \end{aligned}$$

Since $(q-p)$ is an even number, $\sin\frac{1}{2}(q-p)\pi$ is always zero. Hence the first term of (3) is always zero unless the denominator is also zero. For $\sin[(q-p)\pi/2n]$ to be zero, $(q-p)$ must be zero or some multiple of $2n$.

When $p = q$,

$$\frac{\cos[(q-p)(n+1)\pi/2n]\sin[(q-p)\pi/2]}{2\sin[(q-p)\pi/2n]} = \frac{n}{2}$$

and when $(q-p) = 2Mn$, where M is any integer,

$$\begin{aligned} \frac{\cos[(q-p)(n+1)\pi/2n]\sin[(q-p)\pi/2]}{2\sin[(q-p)\pi/2n]} &= \frac{\cos[M(n+1)\pi]\sin Mn\pi}{2\sin M\pi} \\ &= \frac{n}{2} \end{aligned}$$

Hence, when $q = (2Mn + p)$ the value of the first term * in (3) is $A_q \times \frac{1}{2}n$; and when $q = p$, the corresponding value is $A_p \times \frac{1}{2}n$.

Similarly for the second term of the expression for A'_{pq}, the value is zero for all cases except when $(q + p)$ is a multiple of $2n$.

* In the Runge and Thompson methods, n is an even number.

* Inside the bracket.

When $(q + p) = 2Mn$, where M is again any integer:

$$\frac{\cos\,[(q+p)\,(n+1)\pi/2n]\,\sin\,[(q+p)\,\pi/2]}{2\sin\,[(q+p)\,\pi/2n]} = \frac{n}{2}$$

Hence, when $q = (2Mn - p)$, the value of the second term in (3) is $-A_q \times \frac{1}{2}n$.

Therefore, in determining the value of the amplitude of the pth harmonic by the Runge, Thompson, or Perry methods, an error may be introduced, the value computed for the amplitude being

$$\begin{aligned} A'_p &= A_p + (A_{2n+p} + A_{4n+p} + A_{6n+p} + \ldots) \\ &\quad - (A_{2n-p} + A_{4n-p} + A_{6n-p} + \ldots) \\ &= A_p - (A_{2n-p} - A_{2n+p}) - (A_{4n-p} - A_{4n+p}) - \ldots \quad (4) \end{aligned}$$

To take a definite case, assume that the half period is divided into 18 equal parts so that $2n = 36$.

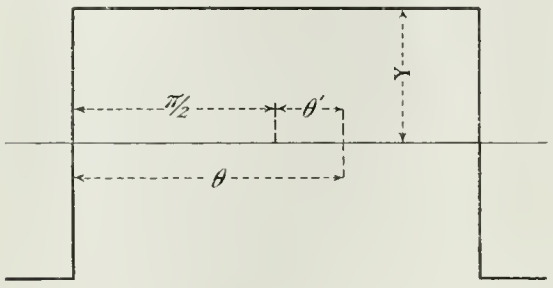


Fig. 2.—Rectangular wave.

Then:

$$\begin{aligned} A'_1 &= A_1 + A_{37} + A_{73} + A_{109} + A_{145} + \ldots \\ &\quad - A_{35} - A_{71} - A_{107} - A_{143} - \ldots \\ A'_3 &= A_3 + A_{39} + A_{75} + A_{111} + A_{147} + \ldots \\ &\quad - A_{33} - A_{69} - A_{105} - A_{141} - \ldots \\ &\quad \cdots\cdots\cdots\cdots\cdots\cdots\cdots \\ A'_{17} &= A_{17} + A_{53} + A_{89} + A_{125} + A_{161} + \ldots \\ &\quad - A_{19} - A_{55} - A_{91} - A_{127} - \ldots \end{aligned}$$

It is possible, therefore, with certain combinations of circumstances, for the total error, i.e. the difference between A'_p and A_p, to be considerable, when the higher harmonics are relatively important.

Let us take as an example the case, previously mentioned, of the rectangular wave (see Fig. 2). In this wave, the various harmonics are all "positive," and the amplitude of the pth harmonic is $(4/\pi) \times (1/p)$ times the height Y of the rectangle.

Thus:

$$\begin{aligned} A'_p &= A_p + A_{36+p} + A_{72+p} + A_{108+p} + \ldots \\ &\quad - A_{36-p} - A_{72-p} - A_{108-p} - \ldots \\ &= A_p - \frac{4Y}{\pi}\left(\frac{1}{36-p} - \frac{1}{36+p}\right) \\ &\quad - \frac{4Y}{\pi}\left(\frac{1}{72-p} - \frac{1}{72+p}\right) - \ldots \\ &= A_p - 2p\frac{4Y}{\pi}\left\{\frac{1}{36^2-p^2} + \frac{1}{72^2-p^2} + \frac{1}{108^2-p^2} + \ldots\right\} \end{aligned}$$

Although this involves the determination of the value of an infinite series, no great difficulty is experienced since the series inside the bracket soon merges—especially for low values of p—into the series:

$$\frac{1}{36^2}\left\{\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \frac{1}{4^2} + \ldots\right\}$$

the sum of which is readily calculable and is equal to $\frac{1}{36^2} \times \frac{\pi^2}{6}$.

Table 1 gives the calculations showing the error obtained in the values of the various harmonics for the particular case under consideration. For the joint effect of the harmonics of orders higher that 6×36, p^2 is neglected in terms of 216^2, and the above-mentioned series utilized.

It may be remarked that the values obtained for the error are in agreement with those calculated by P. Kemp* by using the ordinary Thompson method of analysis.

A consideration of the above expression indicates very clearly that to increase the number of ordinates will materially reduce the error; to a first approximation, although not exactly for the very high harmonics, the error is inversely proportional to the square of the number of ordinates chosen. For the lower harmonics, in addition, the actual error is, for all practical purposes, proportional to the order of the harmonic.

For reasons stated later, the present author has divided the half period into 26 parts when analysing E.M.F. and other wave-forms. This results in the error being reduced by about one half, as shown in Table 2, that of the fundamental and lower harmonics being reduced to about $(18/26)^2$ of the error obtaining with 18 ordinates, and the reduction in the error being relatively even more for the higher harmonics.

From the results tabulated it will be seen that, even for the rectangular wave chosen, a wave rich in higher harmonics, no serious error will result in the use of Thompson's method for the determination of harmonics up to at least the 9th, if the half period is divided into 26 parts.

The following conclusions may therefore be stated at this point, so far as sine terms only are concerned:

By using Thompson's method of harmonic analysis

(1) No error whatever is introduced if the highest order of the harmonics present in the wave-form is less than the number of ordinates chosen per half period.

(2) Of the harmonics higher than the nth, if any such be present (n being the number of ordinates), only $1/n$th of the total possible orders influence the value obtained for the pth harmonic, namely, those of the orders $2Mn \pm p$.

(3) The value actually determined for the amplitude of the pth harmonic is the real value plus the sum of the amplitudes of the harmonics of the orders $2Mn + p$, minus the sum of the amplitudes of the harmonics of the orders $2Mn - p$, where M is any integer.

We will now deal with the effect of the higher cosine

* *Loc. cit.*

TABLE 1.

Calculation of the Error Obtained in the Harmonic Analysis of a Rectangular Wave.
(18 *ordinates per half period.*)

	Value of p *								
	1	3	5	7	9	11	13	15	17
A_p . . . %	100·000	33·333	20·000	14·286	11·111	9·091	7·692	6·667	5·882
$-A_{36-p}$	− 2·857	− 3·030	− 3·225	− 3·448	− 3·704	− 4·000	− 4·348	− 4·762	− 5·263
A_{36+p}	2·703	2·564	2·439	2·326	2·222	2·128	2·041	1·961	1·887
$-A_{72-p}$	− 1·409	− 1·449	− 1·493	− 1·539	− 1·587	− 1·639	− 1·694	− 1·754	− 1·818
A_{72+p}	1·370	1·333	1·299	1·266	1·235	1·205	1·177	1·149	1·124
$-A_{108-p}$	− 0·935	− 0·953	− 0·971	− 0·990	− 1·010	− 1·031	− 1·053	− 1·075	− 1·099
A_{108+p}	0·917	0·901	0·885	0·870	0·855	0·840	0·827	0·813	0·800
$-A_{144-p}$	− 0·699	− 0·709	− 0·719	− 0·729	− 0·741	− 0·752	− 0·763	− 0·775	− 0·787
A_{144+p}	0·690	0·680	0·671	0·662	0·654	0·645	0·637	0·629	0·621
$-A_{180-p}$	− 0·559	− 0·565	− 0·571	− 0·578	− 0·585	− 0·592	− 0·599	− 0·606	− 0·614
A_{180+p}	0·552	0·546	0·541	0·538	0·529	0·524	0·518	0·513	0·508
$-A_{216-p}$	− 0·465	− 0·469	− 0·474	− 0·478	− 0·483	− 0·488	− 0·493	− 0·498	− 0·503
A_{216+p}	0·461	0·457	0·452	0·448	0·444	0·441	0·437	0·433	0·429
$-\Sigma A_{36M-p}$† $+\Sigma A_{36M+p}$	− 0·024	− 0·072	− 0·118	− 0·166	− 0·216	− 0·260	− 0·307	− 0·354	− 0·378
Total error	− 0·255	− 0·766	− 1·284	− 1·818	− 2·387	− 2·979	− 3·620	− 4·326	− 5·093
Deduced Value A'_p	99·745	32·567	18·716	12·368	8·724	6·112	4·072	2·341	0·789
Percentage error	− 0·255	− 2·300	− 6·420	− 12·7	− 21·5	− 32·8	− 47·0	− 64·8	− 86·5

* p = the order of the harmonic.

† The sum is from $M = 7$ to $M = \infty$.

TABLE 2.

Calculation of the Error Obtained in the Harmonic Analysis of a Rectangular Wave.
(26 *ordinates per half period.*)

	Value of p *								
	1	3	5	7	9	11	13	15	17
A_p . . . %	100·000	33·333	20·000	14·286	11·111	9·091	7·692	6·667	5·882
$-A_{52-p}$	− 1·961	− 2·041	− 2·128	− 2·222	− 2·326	− 2·439	− 2·564	− 2·703	− 2·857
A_{52+p}	1·887	1·818	1·754	1·695	1·639	1·587	1·539	1·493	1·449
$-A_{104-p}$	− 0·971	− 0·990	− 1·010	− 1·031	− 1·053	− 1·075	− 1·099	− 1·124	− 1·149
A_{104+p}	0·952	0·935	0·917	0·901	0·885	0·870	0·855	0·840	0·827
$-A_{156-p}$	− 0·645	− 0·654	− 0·662	− 0·671	− 0·680	− 0·690	− 0·699	− 0·709	− 0·710
A_{156+p}	0·637	0·629	0·621	0·614	0·606	0·599	0·592	0·585	0·578
$-A_{208-p}$	− 0·483	− 0·488	− 0·493	− 0·498	− 0·503	− 0·508	− 0·513	− 0·518	− 0·524
A_{208+p}	0·478	0·474	0·470	0·465	0·461	0·457	0·453	0·448	0·444
$-A_{260-p}$	− 0·386	− 0·389	− 0·392	− 0·395	− 0·398	− 0·402	− 0·405	− 0·408	− 0·412
A_{260+p}	0·383	0·380	0·377	0·375	0·372	0·369	0·366	0·364	0·361
$-A_{312-p}$	− 0·322	− 0·324	− 0·326	− 0·328	− 0·330	− 0·332	− 0·335	− 0·337	− 0·339
A_{312+p}	0·319	0·318	0·316	0·315	0·312	0·310	0·308	0·306	0·304
$-\Sigma A_{52M-p}$† $+\Sigma A_{52M+p}$	− 0·011	− 0·033	− 0·055	− 0·078	− 0·100	− 0·125	− 0·145	− 0·167	− 0·189
Total error	− 0·122	− 0·365	− 0·591	− 0·859	− 1·111	− 1·377	− 1·648	− 1·920	− 2·226
Deduced Value A'_p	99·878	32·968	19·409	13·430	10·000	7·714	6·044	4·747	3·656
Percentage error	− 0·122	− 1·10	− 2·96	− 6·00	− 10·0	− 15·1	− 21·4	− 28·2	− 38·3

* p = the order of the harmonic.

† The sum is from $M = 7$ to $M = \infty$.

terms upon the accuracy of the result obtained for the amplitudes of the various cosine terms. The total effect of the qth cosine harmonic upon the value deduced for the pth cosine term is, similarly to the case previously dealt with of the sine terms:

$$B'_{p_q} = 2 \times \frac{1}{n}\Big(B_q \cos\frac{p\pi}{n}\cos\frac{q\pi}{n} + B_q \cos\frac{2p\pi}{n}\cos\frac{2q\pi}{n} + \ldots \text{ to } n \text{ terms}\Big)$$

$$= \frac{2}{n} \times B_q\Big\{\frac{\cos(q-p)\pi/n + \cos(q+p)\pi/n}{2} + \frac{\cos 2(q-p)\pi/n + \cos 2(q+p)\pi/n}{2} + \ldots \text{ to } n \text{ terms}\Big\}$$

$$= \frac{2}{n}\Big\{\frac{B_q \cos[(q-p)(n+1)\pi/2n]\sin[(q-p)\pi/2]}{2\sin[(q-p)\pi/2n]} + \frac{B_q \cos[(q+p)(n+1)\pi/2n]\sin[(q+p)\pi/2]}{2\sin[(q+p)\pi/2n]}\Big\} \quad \ldots (5)$$

Again, corresponding to the case of sine terms, if $q = p$ the expression reduces to B_p.

The main difference between the expression for the value of B'_{p_q} and that for A'_{p_q} [cf. equation (3)] is that the sign of the second term is changed. As a result, the value obtained for the amplitude of the pth cosine term is:

$$B'_p = B_p + (B_{2n-p} + B_{2n+p}) + (B_{4n-p} + B_{4n+p}) + (B_{6n-p} + B_{6n+p}) + \ldots \quad (6)$$

Hence again a considerable error may arise in the case when the higher cosine harmonics are all in phase, i.e. all have the same sign at the chosen origin.

For the case when $2n = 36$, the values calculated for the amplitudes of the various harmonics are thus:

$$B_1 = B_1 + (B_{37} + B_{35}) + (B_{73} + B_{71}) + (B_{109} + B_{107}) + \ldots$$

$$B_3 = B_3 + (B_{39} + B_{33}) + (B_{75} + B_{69}) + (B_{111} + B_{105}) + \ldots$$

$$B_{17} = B_{17} + (B_{19} + B_{53}) + (B_{89} + B_{55}) + (B_{125} + B_{91}) + \ldots$$

It must not be concluded that a greater error is obtained by, for example, displacing the origin of a wave normally containing only sine terms by one quarter of a period and analysing for cosine terms, than would be obtained if the original origin were taken and the wave analysed for sine terms. The error is the same in the case of both methods.

Take the case of the rectangular wave, Fig. 2. With the origin at the point where the wave crosses the axis, the wave is represented by:

$$y = \frac{4Y}{\pi}\Big\{\sin\theta + \tfrac{1}{3}\sin 3\theta + \tfrac{1}{5}\sin 5\theta + \ldots\Big\}$$

If now θ' is measured from the new origin 90° displaced from the point where the wave crosses the axis, the wave is represented by:

$$y = \frac{4Y}{\pi}\Big\{\cos\theta' - \tfrac{1}{3}\cos 3\theta' + \tfrac{1}{5}\cos 5\theta' - \ldots + \ldots\Big\}$$

The value calculated for the amplitude of the pth harmonic is therefore:

$$B'_p = B_p + (B_{2n-p} + B_{2n+p}) + (B_{4n-p} + B_{4n+p}) + \ldots$$

$$= \frac{4Y}{\pi}\Big\{\frac{1}{p}\sin\frac{p\pi}{2} + \frac{1}{2n-p}\sin\frac{(2n-p)\pi}{2} + \frac{1}{2n+p}\sin\frac{(2n+p)\pi}{2} + \ldots\Big\}$$

$$= \frac{4Y}{\pi}\sin\frac{p\pi}{2}\Big\{\frac{1}{p} + \Big(\frac{1}{2n+p} - \frac{1}{2n-p}\Big) + \Big(\frac{1}{4n+p} - \frac{1}{4n-p}\Big) + \ldots\Big\}$$

since $2n$ is a multiple of 4.

The error is thus exactly the same as that calculated with the origin at the point where the wave crosses the axis.

So far as cosine terms are concerned, therefore, the first two of the conclusions stated above for sine terms also hold good. In addition, the value actually determined as the amplitude of the pth harmonic is the real value plus the sum of the amplitudes of all the harmonics of the orders $2Mn \pm p$, where M is any integer.

Practical Application.

In the majority of cases the wave-shape to be analysed is that of the pressure of an alternator on open circuit, a wave containing only sine terms. In such a wave, except in the case of the tooth ripples, harmonics of high orders are practically eliminated owing to the spread of the winding. As a result, provided that suitable steps are taken to avoid errors due to the tooth-ripple harmonics, the Thompson simplification of Runge's method can be used in perfect confidence.

Now the tooth-ripple harmonics are of the orders $2S \pm 1$, where S is the number of slots per pole. Depending on the value of the pole-pitch, S will for most low-speed alternators be from 6 to 12. For high-speed machines, double the latter figure may be reached, or even exceeded. The corresponding harmonics will therefore lie between the 11th and 25th for low-speed alternators. Normally, the tooth ripples will decrease in relative importance as the value of S increases.

In the case of low-speed alternators, it is desirable to carry the analysis of the wave-form far enough to include the tooth harmonics. For this reason, the author has for many years made it a practice to divide the half period into 26 equal parts and to make use of the form given in Part (2) of the present paper.

By the use of 26 ordinates, both the 23rd and 25th harmonics, representing the tooth ripple for a three-phase alternator having 4 slots per pole per phase, can be determined, and moreover, all the harmonics can be determined with as much accuracy as the data will warrant, up to the 25th, for any normal alternator having not more than 12 slots per pole. By using 26

ordinates, the method is theoretically exact, provided that all harmonics higher than the 26th are absent. Any error introduced is therefore in all probability to be found in the subdivision of the half period and the measurement of the corresponding ordinates of the pressure wave.

In this connection it should be mentioned that the choice of 18 ordinates, a number very often used, is not a happy one. Such a choice permits of a value being determined for harmonics up to the 17th. With the majority of three-phase alternators, the number of slots per pole will not be less than 9, so that the teeth may give rise to 17th and 19th harmonics in the pressure wave. If, as will normally be the case for a 9-slot-per-pole machine, the 19th harmonic is present, the value obtained for the 17th is worthless if only 18 ordinates are used, since the value obtained is really the difference between the amplitudes of the 17th and 19th harmonics if those of yet higher orders are negligible. To deal with the case of a 9-slot-per-pole alternator, 20 ordinates at least should be used; by using 26, any normal case can be dealt with.

Unless the original wave-form can be obtained, or accurately reproduced, on a large scale, it would not be desirable to carry the subdivision of the half period to beyond the number of parts adopted by the author. Further, the number of calculations increases as about the square of the number of ordinates employed, so that it is desirable, if possible, to be able to obtain an accurate estimate of the tooth harmonics in all cases without going beyond 26 ordinates, even when the harmonics are of much higher orders than the 25th. With turbo-alternators, not only may harmonics of very high orders be set up due to the stator teeth, but also due to the field winding being distributed in slots. The orders of the possible ripple harmonics with such a machine are therefore $2S \pm 1$, and $2S_e \pm 1$, where S_e is the number of slot-pitches per pole on the rotor. (Care is taken in practice that stator and rotor slottings are such that they do not tend to give rise to harmonics of the same order.) Normally, these ripple harmonics will be of orders higher than the 25th, and as a result the subdivision of the half period into 26 parts cannot give theoretically exact results for all the harmonics lower than the 26th. An inspection of the oscillogram is sufficient to show whether the ripple harmonics are of appreciable magnitude, and their orders are known to be $2S \pm 1$ and possibly, in addition, $2S_e \pm 1$ for the case of cylindrical-field machines. The only error introduced by these harmonics will occur in the amplitudes of the harmonics of orders, where 26 ordinates are used, $52 - (2S \pm 1)$, and possibly $52 - (2S_e \pm 1)$. The harmonics likely to be affected can at once be detected and due allowance made for the error.

In many cases, in fact in the majority of cases of pressure waves of alternators on open circuit, the error itself can be utilized to determine the value of the amplitudes of the ripple harmonics. For example, suppose the schedule given later were used to analyse the open-circuit pressure wave of a three-phase alternator having 18 slots per pole, and the corresponding tooth ripples were pronounced. The orders of the corresponding harmonics are the 35th and the 37th, and as a result the values obtained for the amplitudes of the $(52 - 35)$th and the $(52 - 37)$th harmonics would be respectively $A_{17} - A_{35}$ and $A_{15} - A_{37}$, assuming that, other than the ripple harmonics, harmonics of very high orders are absent. (This assumption is quite justifiable.)

An inspection of the oscillogram, and of the amplitudes deduced for the other harmonics, will in the majority of cases indicate that the 17th and 15th harmonics are negligible, so that it may be safely assumed that the value determined for the amplitude of the 17th harmonic is really $-A_{35}$, and that determined for the 15th really $-A_{37}$. This is illustrated by the actual figures given in the analysis schedule, which is made out for a wave-form represented by:

$$e = 1\,000 \sin\theta - 100 \sin 35\theta + 100 \sin 37\theta$$

From the schedule, the wave-form calculated is:

$$e' = 1\,000 \sin\theta + 100 \sin 17\theta - 100 \sin 15\theta$$

The 15th and 17th harmonics as calculated thus have amplitudes corresponding to those of the 35th and 37th, and are of reversed sign.

The actual wave-form is shown in Fig. 3, the ripples represented by the joint effect of the two high harmonics, corresponding to 36 times the main frequency, and their amplitudes varying over the period according to a sine law, being zero when $\theta = 0°$, and maximum when $\theta = 90°$.

If 18 ordinates were chosen to analyse this particular wave, as indicated in Fig. 3, the position of the ordinates would be such as to correspond always to the crest of the ripples. As a result, the amplitudes deduced for the fundamental would be 20 per cent too high; the results previously obtained are thus corroborated by a mere inspection of this particular wave. Similarly, if mid-ordinates were used, as also indicated in Fig. 3, the ordinates would correspond in every case to the troughs of the ripples, and would therefore be in every case 20 per cent less than the actual ordinate of the fundamental harmonic. With the use of mid-ordinates, therefore, the value deduced for the fundamental harmonic of this particular wave would be 20 per cent too low.*

Thus, under conditions generally occurring with cylindrical-field alternators, and low-speed alternators having more than 12 slots per pole, although the half period is divided into only 26 parts, and the ripple harmonics are of higher orders than the 26th, the amplitudes of these ripple harmonics can be deduced from the values calculated in the schedule for lower harmonics which obviously are not present in appreciable magnitude.

When used for the analysis of the flux-distribution wave, or field-form, or for the actual E.M.F. induced in an individual conductor, the Thompson adaptation of Runge's method can also be used to obtain reliable results. In such wave-forms, the lower harmonics are as a rule much more important than in the total pressure wave, but even in this case there is no difficulty in determining which harmonics, if any, are affected by the tooth ripples where these correspond to harmonics higher than the 26th. For such cases too, cylindrical-

* The general case corresponding to the use of mid-ordinates is discussed in Appendix 1.

field alternators have such a field-form that, excepting the spacing ripples, harmonics above the ninth are generally negligible,* so that with such machines the value of the harmonics due to the rotor slots can also be deduced from the results derived from the schedule.

With current wave-shapes both sine and cosine terms may be present, but, with the exception of circuits where the capacity effect is marked, the higher harmonics are reduced even more that in the case of the pressure wave, although those of lower orders may be more pronounced. For such waves, therefore, it may be taken that the use of the Thompson method with 26 ordinates does not result in any appreciable error at all. Where capacity effects are pronounced, higher harmonics in the current wave may be very prominent. But, again, their magnitude is accurately determined for all cases where the order of the harmonics does not exceed the number of ordinates chosen per half period. Should the harmonics be even of higher orders than the 25th, just as in the case of the 35th and 37th harmonics previously discussed, an error is introduced in the values obtained for certain of the lower harmonics included in the schedule. An intelligent use of the method of analysis enables the harmonics affected to be determined and, in addition, permits of a close approximation to the values of the amplitudes of the very high harmonics causing the error to be obtained. For the analysis of crank-effort diagrams, the original method due to C. Runge may well be used, although for this case the later Fischer Hinnen or Thompson method of summing certain selected ordinates is well suited to the analysis of such diagrams, as only the low harmonics need to be determined.

For the analysis of rectilinear wave-forms there is no justification for using any such method as those discussed in this paper. The exact series for any such wave can readily be calculated from a determination of the actual value of the integral concerned.*

To sum up, it can conclusively be stated that for all normal wave-shapes of pressure and current met with in practice, the Runge method as outlined by Thompson may be employed in perfect confidence. The method is theoretically exact provided that no harmonics of orders greater than the number of ordinates chosen per half period are present. Except for the case of tooth ripples with alternators of large pole pitch, such higher harmonics are in actual fact actually negligible if the half period is divided into 26 parts. With machines of large pole-pitch, the tooth ripples will probably correspond to harmonics higher than the 26th, but such higher harmonics can only each affect the value of one of the harmonics within the scope of the schedule, and normally the error thus obtained in the deduced amplitudes of two of the harmonics lower than the 26th can at once be detected and allowed for, and the actual amplitudes of the ripple harmonics themselves deduced.

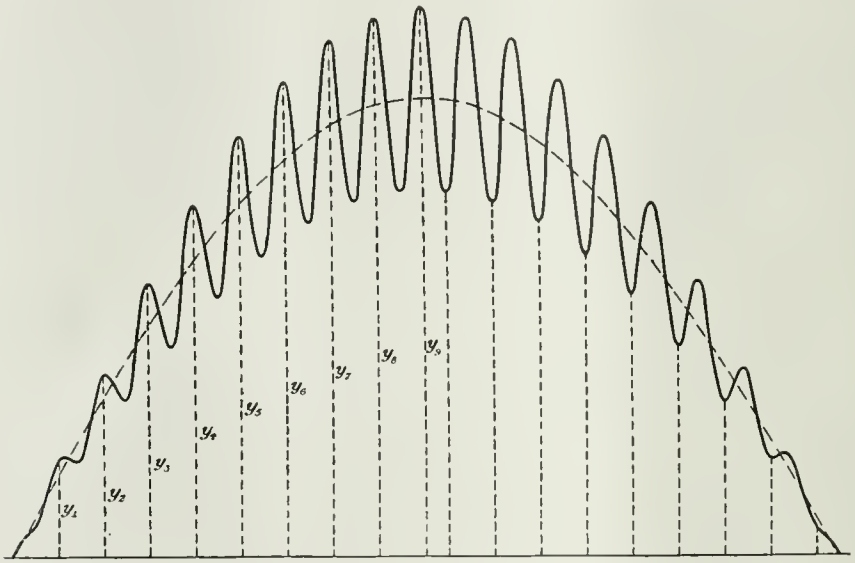


FIG. 3.—Wave-shape representing $e = 100 \sin \theta - 100 \sin 35\theta + 100 \sin 37\theta$.
Half period divided into 18 equal parts.
Actual ordinates y_1 to y_9 shown on the left, and the mid-ordinates on the right.

Generally, the accuracy of the results obtained by using the Thompson method is limited almost entirely by the accuracy of the construction and measurement of the various ordinates. Only too frequently two complete periods are crowded into the limited length of the film or plate. If an accurate analysis is required, the oscillogram should be to as large a scale as possible, and the wave-length as great as possible. It is therefore desirable that the complete rotation of the drum or the motion of the plate shall be such as to correspond to but little more than one half period of the wave. To obtain a good photograph this would normally involve running the machine under test at a reduced speed,

* A. E. CLAYTON: "Magnetomotive Force Calculations for Cylindrical-Field Alternators under Steady Short-circuit Conditions," *Journal I.E.E.*, 1919, Supplement to vol. 57, p. 70.

* A. E. CLAYTON: *Ibid.*, pp. 82 and 83.

to increase the time of the period considerably; this is not always possible, but presents no difficulty for the two important works' tests, i.e. the open-circuit and short-circuit tests.

(2) A Single Schedule for the Analysis of Wave-forms containing Odd Harmonics only; the Analysis being carried up to the 25th Harmonic.

The schedule here given is in a form which, as the result of much work on harmonic analysis in practice, the author has found to be the most convenient. Compared with the usual manner of applying the Thompson adaptation of Runge's method, the present method has the further advantage that only one tabular form is necessary, the same schedule being suited equally well for dealing with sine terms or with cosine terms. The convenience arising out of the possibility of dispensing with a separate schedule for the cosine terms is so obvious that there is no need to emphasize it further.

With the exception of the above further simplification, the method is based entirely upon that due to Runge, and the arrangement has been devised to eliminate the possibilities of errors in signs, etc., as far as possible.

The half period is divided into 26 equal parts, and the sine terms separated from the cosine terms in the usual manner by taking the sum and difference respectively, of ordinates which are at supplementary angles. The sums, the sine ordinates, are denoted respectively by the numbers 1, 2, etc., to 13. The differences, the cosine ordinates, are numbered in the reverse direction from the sine terms, and are denoted by the numbers 13′, 12′, etc., to 1′. It is this reversal of the numbering of the cosine ordinates which makes it possible to use the same form for either cosine or sine terms.

It is necessary to note, however, that in the case of certain cosine harmonics the value deduced from the ordinary application of the table has to be changed in sign; this is clearly indicated on the schedule.

The adoption of 26 total ordinates for the half period also does away with the need for grouping the ordinates for all cases except the 13th harmonic.

In the analysis table, the first column gives multiplying factors equal respectively to 1/13th of sin 90°/13, of sin 180°/13, of sin 270°/13, etc. These factors hold good for the whole of the line in which they occur.

The number of the sine or cosine ordinate which, when multiplied by the appropriate factor, has to be inserted in each section, is placed in one corner of the section. (When used for cosine terms, the ordinates 1′, 2′, etc., are utilized in the sections marked 1, 2, etc.) In order to avoid confusion of signs, when positive, the ordinate number is placed in the top left-hand corner of the section and, when negative, it is placed in the top right-hand corner; the dotted lines serve to separate the positive values from the negative.

It is usually convenient to start the half period at a point where the wave crosses the axis, for which case $y_0 = 0$. Should this not be the case, y_0 is the value of the function at the start of the half period, y_{13} representing the mid-ordinate of the half period.

SCHEDULE FOR ANALYSING WAVE-FORMS UP TO THE 25th HARMONIC (ODD HARMONICS ONLY).

Schedule A: For Separating out the Sine and Cosine Terms.

Ordinates of wave	y_0 0	y_1 112	y_2 204	y_3 421	y_4 476	y_5 460	y_6 738	y_7 834	y_8 663	y_9 908	y_{10} 1 101	y_{11} 828	y_{12} 922	y_{13} 1 200
Ordinates of wave	X	y_{25} 112	y_{24} 204	y_{23} 421	y_{22} 476	y_{21} 460	y_{20} 738	y_{19} 834	y_{18} 663	y_{17} 908	y_{16} 1 101	y_{15} 828	y_{14} 922	X
Sum—sine terms	X	1 224	2 408	3 842	4 952	5 920	6 1 476	7 1 668	8 1 326	9 1 816	10 2 202	11 1 656	12 1 844	13 1 200
Difference—cosine terms..	13′ /	12′ /	11′ /	10′ /	9′ /	8′ /	7′ /	6′ /	5′ /	4′ /	3′ /	2′ /	1′ /	X

Note.—The half period is divided into 26 equal parts; y_0 is the ordinate at the chosen origin, y_1, y_2, etc., are the ordinates at the points of division.

SCHEDULE FOR ANALYSING WAVE-FORMS UP TO

SCHEDULE B: FOR ANALYSING EITHER

Schedule used for:

Multiplying Factors	θ and 25θ				3θ and 23θ				5θ and 21θ			
	Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates	
	+	−	+	−	+	−	+	−	+	−	+	−
0·0093	1 *2·1*					−9 *16·9*			5 *8·6*			
0·0184			2 *7·5*				8 *24·4*					−10 *40·5*
0·0273	3 *22·0*				1 *6·1*				11 *45·2*			
0·0358			4 *34·1*					−10 *78·8*				−6 *52·8*
0·0437	5 *40·2*				7 *72·9*				1 *9·8*			
0·0510			6 *75·2*				2 *20·8*				4 *48·6*	
0·0576	7 *96·1*					−11 *95·5*				−9 *104·5*		
0·0633			8 *84·0*				6 *93·5*				12 *116·8*	
0·0681	9 *123·8*				3 *57·4*					−7 *113·5*		
0·0719			10 *158·2*					−12 *132·7*			2 *29·3*	
0·0745	11 *123·3*				5 *68·5*				3 *62·7*			
0·0764			12 *141·0*				4 *72·8*					−8 *101·2*
0·0769	13 *92·2*					−13 *92·2*			13 *92·2*			
Individual Sums, + and − ..	*499·7*		*500*		*204·9*	*204·6*	*211·5*	*211·5*	*218·5*	*218·0*	*194·7*	*194·5*
Sums of Odd and Even Ordinates respectively ..	*500*		*500*									
Σ Odd Ordinates + Σ Even Ordinates	$A_1 = 1\,000$ (or $B_1 =$)				$A_3 =$ (or $-B_3 =$)				$A_5 =$ (or $B_5 =$)			
Σ Odd Ordinates − Σ Even Ordinates	$A_{25} =$ (or $B_{25} =$)				$A_{23} =$ (or $-B_{23} =$)				$A_{21} =$ (or $B_{21} =$)			

Computed Analysis of *Sine* Terms:

THE 25TH HARMONIC (ODD HARMONICS ONLY)—*continued.*

THE SINE OR THE COSINE TERMS.

Sine Terms.

7θ and 19θ				9θ and 17θ				11θ and 15θ				13θ
Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates		
+	−	+	−	+	−	+	−	+	−	+	−	
11 *15·4*					**−3** *7·8*			**7** *15·5*				
			−4 *17·5*			**6** *27·2*					**−12** *34·0*	
	−7 *45·5*				**−9** *49·6*			**5** *25·1*				
		8 *47·4*				**12** *66·0*				**2** *14·6*		
3 *36·9*					**−11** *72·4*				**−9** *79·3*			
			−12 *94·0*			**8** *67·6*				**10** *112·4*		
1 *12·9*					**−5** *53·0*				**−3** *48·5*			
		10 *139·5*				**2** *25·8*					**−4** *60·3*	
	−5 *62·6*			**1** *15·3*				**11** *112·9*				
			−6 *106·0*				**−4** *68·4*				**−8** *95·4*	
9 *135·1*				**7** *124·3*				**1** *16·7*				
		2 *31·2*					**−10** *168·2*			**6** *112·8*		
	−13 *92·2*			**13** *92·2*					**−13** *92·2*			
200·3	*200·3*	*218·1*	*217·5*	*231·8*	*182·8*	*186·6*	*236·6*	*170·2*	*220·0*	*239·8*	*189·7*	$\left\{\begin{array}{l}\text{(Sum of ordinates } 1+5+9+13) - \text{(sum of ordinates } 3+7+11)\end{array}\right\} \times 0{\cdot}0769 = A_{13}$ (or $B_{13} =$)
				49·0		*− 50·0*		*− 49·8*		*50·1*		$(224 + 920 + 1\,816 + 1\,200) \quad (842 + 1\,668 + 1\,656)$
												4 160 *4 166*
$A_7 =$ (or $- B_7 =$)				$A_9 = -1$ (or $B_9 =$)				$A_{11} =$ (or $- B_{11} =$)				$A_{13} =$ or $B_{13} =$
$A_{19} =$ (or $- B_{19} =$)				$A_{17} = 99$ (or $B_{17} =$)				$A_{15} = -100$ (or $- B_{15} =$)				

$y = 1\,000 \sin\theta - 100 \sin 15\theta + 99 \sin 17\theta.$

Separate copies of the schedule are filled up for the sine and cosine terms, and the results of the two schedules combined in the usual manner, the pth harmonic being represented in the series as

$$\sqrt{(A_p^2 + B_p^2)} \sin p\,(wt + \phi_p), \text{ where } \phi_p = \frac{1}{p} \arctan\left(\frac{B_p}{A_p}\right)$$

In using the above schedule it is convenient to have available information relating to the effect of harmonics higher than the 26th upon the values of the various amplitudes obtained from the schedule. Neglecting harmonics higher than the 77th we have:

$$\begin{aligned}
A_1' &= A_1 - A_{51} + A_{53}; & B_1' &= B_1 + B_{51} + B_{53}\\
A_3' &= A_3 - A_{49} + A_{55}; & B_3' &= B_3 + B_{49} + B_{55}\\
A_5' &= A_5 - A_{47} + A_{57}; & B_5' &= B_5 + B_{47} + B_{57}\\
A_7' &= A_7 - A_{45} + A_{59}; & B_7' &= B_7 + B_{45} + B_{59}\\
A_9' &= A_9 - A_{43} + A_{61}; & B_9' &= B_9 + B_{43} + B_{61}\\
A_{11}' &= A_{11} - A_{41} + A_{63}; & B_{11}' &= B_{11} + B_{41} + B_{63}\\
A_{13}' &= A_{13} - A_{39} + A_{65}; & B_{13}' &= B_{13} + B_{39} + B_{65}\\
A_{15}' &= A_{15} - A_{37} + A_{67}; & B_{15}' &= B_{15} + B_{37} + B_{67}\\
A_{17}' &= A_{17} - A_{35} + A_{69}; & B_{17}' &= B_{17} + B_{35} + B_{69}\\
A_{19}' &= A_{19} - A_{33} + A_{71}; & B_{19}' &= B_{19} + B_{33} + B_{71}\\
A_{21}' &= A_{21} - A_{31} + A_{73}; & B_{21}' &= B_{21} + B_{31} + B_{73}\\
A_{23}' &= A_{23} - A_{29} + A_{75}; & B_{23}' &= B_{23} + B_{29} + B_{75}\\
A_{25}' &= A_{25} - A_{27} + A_{77}; & B_{25}' &= B_{25} + B_{27} + B_{77}
\end{aligned}$$

The schedule given is shown completed for the analysis of a wave represented actually by

$$e = 1\,000 \sin\theta - 100 \sin 35\theta + 100 \sin 37\theta$$

This wave is represented in Fig. 3, and corresponds to a pressure wave which is purely a sine wave with the exception of very pronounced tooth ripples of the orders which would be established in a three-phase alternator with six slots per pole per phase. The values of the ordinates of the wave have been deduced from trigonometrical tables; otherwise all the necessary calculations have been performed on an ordinary 10-inch slide rule.

The table shows well that, provided the ordinates are measured correctly, remarkably accurate results can be obtained; in addition, the results completely substantiate the theory deduced earlier in the present paper. The only harmonics within the scope of the table that are affected by the presence of the very high harmonics, the 35th and 37th, in the pressure wave, are the 17th and 15th. The value obtained for the amplitude of the fundamental is exact, and for all the other harmonics, with the exception of the two just mentioned, the amplitude is zero within the limits of accuracy of the calculations, the largest value being 0·1 per cent of the amplitude of the fundamental.

The amplitude obtained for the 15th harmonic agrees within 0·1 per cent with that of the actual 37th, i.e. (52 − 15)th, and is of opposite sign; similarly, the amplitude obtained for the 17th is equal to the actual value for the 35th harmonic and is of opposite sign. An inspection of the actual wave-shape at once indicates that the 15th and 17th harmonics cannot possibly be present to any marked extent; hence, in using the schedule in a case such as this, the values obtained for the 15th and 17th harmonics would unhesitatingly be assigned to the (52 − 15)th and the (52 − 17)th, i.e. the 37th and the 35th respectively. With three-phase star-connected machines, one could, of course, be certain that any large value deduced for the 15th harmonic must be due to such a higher harmonic.

The amplitudes of the two high harmonics were taken to be equal, as this corresponds to the normal case of tooth ripples.

Even for the analysis of wave-forms that are apparently devoid of all except the lower harmonics, the author prefers to carry the subdivision as far as in the above schedule, since smooth waves are often rich in higher harmonics, e.g. the case of the rectangular wave, and it is impossible to say with any certainty that all harmonics above a certain order are negligible, unless a fairly high value is taken for that order. For example, it is quite impossible by inspecting a wave to determine that the first, third and fifth are present, and the seventh and ninth are absent. So when a very limited number of ordinates is used the results obtained cannot be regarded with any confidence. When six ordinates per half period are taken, an error occurs in the value deduced for the fundamental if the eleventh or thirteenth is present; an error in the third if the ninth is present, and an error in the fifth if the seventh is present. The values obtained are really:

$$\begin{aligned}
A_1' &= A_1 - (A_{11} - A_{13}) - (A_{23} - A_{25}) - \ldots\\
A_3' &= A_3 - (A_9 - A_{15}) - (A_{21} - A_{27}) - \ldots\\
A_5' &= A_5 - (A_7 - A_{17}) - (A_{19} - A_{29}) - \ldots
\end{aligned}$$

Occasionally, however, circumstances warrant the use of a smaller number of ordinates than 26; for such cases, the number of ordinates favoured by the author is 14, a number which gives theoretically accurately results up to the 13th harmonic, provided that no harmonics higher than the 13th are present, and which therefore is suited for dealing with the pressure waves of alternators having six slots per pole; a schedule for 14 ordinates per half period is given in Appendix 4.

(3) Review of, and Comparison with, Other Methods of Practical Harmonic Analysis.

Wedmore's method.—One of the earliest methods put forward for the practical analysis of wave-forms is the graphical method due to E. B. Wedmore.*

In brief, this method consists in determining the wave-form representing the joint effect of all the harmonics of the orders Mp, M being any integer, by dividing the whole period of the wave into p equal parts which are then superposed and added together. Thus, to find the wave representing the combined effect of all the harmonics of the third order, the complete period is divided into three equal parts, which are then superposed and added together as in Fig. 4, the resulting wave representing three times the combined effect of all the harmonics of the third order.

The principle involved in this method is, simply,

* E. B. Wedmore: "A Graphical Method of Analysing Harmonic Curves," *Electrician*, 1895, vol. 35, p. 512.

that in a p-phase mesh-connected system the sum of the harmonics of all orders is zero, except for those of the orders Mp.

In applying this method, the even harmonics are first separated out by dividing the complete period into two equal parts which, when superposed and added, give a wave which represents twice the effect of all the even harmonics. By subtracting one half of this latter wave from the original wave-form, the sum of all the odd harmonics is obtained. The wave representing the third harmonic and odd multiples of the third is then obtained by the method indicated above; then the joint effect of the harmonics of the fifth order is obtained, and so on, these resulting waves being further analysed if necessary.

Whilst this method is very laborious if it is required to carry the analysis very far, it is extremely illuminative and valuable from an instructional point of view, and all students of the subject would be well advised to analyse an actual wave-form into its component harmonics by this method. It has the advantage that it is not necessary to consider separately the sine and

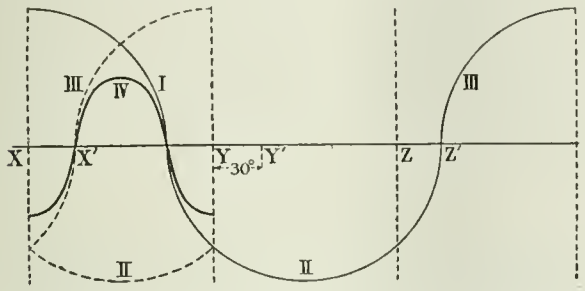


FIG. 4.—Illustrating Wedmore's graphical method of harmonic analysis applied to determine the harmonics of the third order for a "circular" wave.

cosine terms, the correct amplitude and phase being at once obtained from the resulting wave. In practice, too, the method might well be used when it is only required to find the wave-form representing the joint effect of all the harmonics of one particular order, e.g. the joint effect of the third, ninth, fifteenth, etc., harmonics.

For the complete analysis of a wave-form, however, graphical methods are too laborious for use in practice, and either mechanical analysers are used, or the amplitudes of the various harmonics are calculated from measurements of selected ordinates. From the values of suitably selected ordinates it is possible by mere addition to obtain fairly accurate values of the amplitudes of the various harmonics, although it will be seen that the method is more laborious and less accurate than the Runge method.

*Fischer Hinnen's, and Thompson's method.**—In this method, the amplitudes of the various harmonics are deduced by the mere addition of certain selected ordinates. The mathematical theory of this method is given in the paper read by Thompson before the Physical Society, and has been amplified by Russell.* The principle of the method follows at once from a consideration of Wedmore's graphical method.† Considering first only the cosine terms—such cosine terms only are shown in Fig. 4—it follows at once from a consideration of the figure that it is only necessary to add the ordinates of the original wave at the points X, Y, and Z to obtain a series containing the amplitudes of all the harmonics of the third order.

Similarly, for the sine terms it is only necessary to add the ordinates of the original wave at the points X', Y', and Z', displaced from X, Y, and Z respectively by $90°/3$, to obtain a series containing the amplitudes of all the sine terms of the third order.

In general, for odd harmonics, by adding together the values of p ordinates displaced by $2\pi/p$, starting from and including the chosen origin, we have

$$B_p + B_{3p} + B_{5p} + \ldots = y_0 + y_{2\pi/p} + y_{4\pi/p} + \ldots \text{ to } p \text{ terms}$$

Similarly for the sine terms, the first ordinate now being erected $90°/p$ from the chosen origin, we have

$$A_p - A_{3p} + A_{5p} - A_{7p} + - \ldots = y_{\pi/2p} + y_{5\pi/2p} + y_{9\pi/2p} + \ldots \text{ to } p \text{ terms}$$

As in the case of the Wedmore method, then, this method does not determine directly the amplitude of the individual harmonics, but the "peak" effect of all the harmonics of any particular order.

It is therefore necessary to determine first the value of the harmonic of the highest order that is being investigated, and then to work downwards as indicated in the following table, which is drawn up for dealing with odd harmonics up to the ninth order.

TABLE 3.

Harmonic Analysis by the Averaging of Selected Ordinates.
(Odd Harmonics only, up to and including the Ninth.)

$$A'_9 = \tfrac{1}{9}(y_{10°} + y_{50°} + y_{90°} + y_{130°} + y_{170°} + y_{210°} + y_{250°} + y_{290°} + y_{330°});$$

$$B'_9 = \tfrac{1}{9}(y_0 + y_{40°} + y_{80°} + y_{120°} + y_{160°} + y_{200°} + y_{240°} + y_{280°} + y_{320°}).$$

$$A'_7 = \tfrac{1}{7}(y_{13°} + y_{64°} + y_{116°} + y_{167°} + y_{219°} + y_{270°} + y_{321°});$$

$$B'_7 = \tfrac{1}{7}(y_0 + y_{51°} + y_{103°} + y_{154°} + y_{206°} + y_{257°} + y_{308°}).$$

$$A'_5 = \tfrac{1}{5}(y_{18°} + y_{90°} + y_{162°} + y_{234°} + y_{306°});$$

$$B'_5 = \tfrac{1}{5}(y_0 + y_{72°} + y_{144°} + y_{216°} + y_{288°}).$$

$$A'_3 = \tfrac{1}{3}(y_{30°} + y_{150°} + y_{270°}) + A'_9;$$

$$B'_3 = \tfrac{1}{3}(y_0 + y_{120°} + y_{240°}) - B'_9.$$

$$A'_1 = y_{90°} + A'_3 - A'_5 + A'_7 - A'_9;$$

$$B'_1 = y_0 - B'_3 - B'_5 - B'_7 - B'_9.$$

* FISCHER HINNEN: *Electrotechnik und Maschinenbau*, 1909, vol. 27, p. 335. S. P. THOMPSON: "A New Method of Approximate Harmonic Analysis by Selected Ordinates," *Proceedings of the Physical Society*, 1910–11, vol. 23, p. 334.

* A. RUSSELL: "Practical Harmonic Analysis," *Proceedings of the Physical Society*, 1914, vol. 27, p. 157, and "A Treatise on the Theory of Alternating Currents," 2nd edn., vol. 2, p. 128.

† The principle of the method also follows from a consideration of the expression for the amplitude of the 13th harmonic in the schedule already given.

The above table is somewhat modified from that given by Thompson in his paper * before the Physical Society, but is the same in principle.

The accuracy of the results obtained by this method is obviously dependent upon the presence or absence of harmonics higher than those included in the analysis table; the lower harmonics are most likely to be affected, the fundamental being affected by every harmonic higher than those included in the table, the third by every such higher harmonic of the third order, and so on. At first, this method appears very attractive owing to the fact that it is not necessary to multiply the various ordinates by the corresponding sines. (These multiplications, however, are so readily carried out on a slide rule that the average technical engineer regards a multiplication as, if anything, a simpler matter than ordinary addition.) Actually, however, the labour involved by this method is much greater than that involved by the Runge method, in addition to the results being less accurate.

For example, if this method of adding selected ordinates were employed to analyse a wave containing only odd harmonics, the analysis being carried up to the 25th harmonic, to correspond to the schedule given on page 500, it would be necessary to read off from the wave-form the values of the ordinate at no less than 294 different places, involving the expenditure of much more time than would be necessary to complete the analysis of the wave by means of the schedule. Any error in the value of the higher harmonics deduced by this method also results in an error in the value obtained for the fundamental harmonic.

So far as electrical engineers are concerned, therefore, this method does not compare favourably with the Runge method, which is more accurate and much less laborious.

For the approximate analysis of crank-effort diagrams, etc., waves containing pronounced lower harmonics, both odd and even, where it is not required to carry the analysis beyond the third or fourth harmonic, the method might be used, but Wedmore's graphical method would be preferable, as it would give a good indication of the importance of harmonics higher than the fourth. If it is required to analyse such a wave with fair accuracy further than the fourth harmonic, the original Runge method could again be used to advantage.

Russell's method.—In his paper before the Physical Society, Dr. Russell showed how Weddle's rule can be utilized for the determination of the values of the various Fourier integrals concerned in the harmonic analysis of a wave-form.

Weddle's rule is employed to determine the area corresponding to $\int_0^x y\,dx$; instead of utilizing the mean value of a number of equidistant ordinates, various ordinates are "loaded," and the area (see Fig. 5) is taken as:

$$\int_0^x y\,dx \doteq \frac{x}{20}\,[y_0 + y_2 + y_4 + y_6 + 5\,(y_1 + y_5) + 6y_3]$$

This method involves the division of the half period of the wave-form into some multiple of six parts and, so far as the application of Weddle's rule to the case under consideration is concerned, the result is that, instead of taking the series as given in equations (1) and (2) on page 493, various of the terms are "loaded."

For example, to determine the value of the fundamental sine term when the half period is divided into six equal parts, the amplitude is taken as

$$\begin{aligned} A_1' &= 2 \times \tfrac{1}{20}\{0 + \sin 60^\circ(y_{60^\circ}) + \sin 120^\circ\,(y_{120^\circ}) + 0 \\ &\quad + 5[\sin 30^\circ\,(y_{30^\circ}) + \sin 150^\circ\,(y_{150^\circ})] + 6\,(y_{90^\circ})]\} \\ &= \tfrac{1}{20}\{\sqrt{3}\,(y_{60^\circ} + y_{120^\circ}) + 5(y_{30^\circ} + y_{150^\circ}) + 12y_{90^\circ}\} \end{aligned}$$

For Weddle's rule to hold good it is necessary to subdivide the Fourier integrals, taking the limits at the points of discontinuity; this involves the use of a number of ordinates increasing in proportion to the order of the harmonic under investigation. Thus 18 ordinates per half period are necessary for the third harmonic, 30 for the fifth, 42 for the seventh, and, in general, $6p$ ordinates for the pth harmonic.

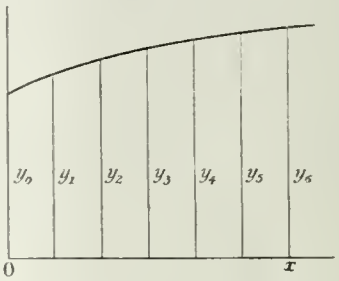


FIG. 5.

The values for the amplitudes of the various harmonics are then: *

$$20A_1' = \sqrt{3}(y_{60^\circ} + y_{120^\circ}) + 5(y_{30^\circ} + y_{150^\circ}) + 12y_{90^\circ}$$

$$20B_1' = y_{60^\circ} - y_{120^\circ} + 4y_0 + 5\sqrt{3}\,(y_{30^\circ} - y_{150^\circ})$$

$$\begin{aligned} 60A_3' = [&\sqrt{3}(y_{20^\circ} + y_{40^\circ} - y_{80^\circ} - y_{100^\circ} + y_{140^\circ} + y_{160^\circ}) \\ &+ 5(y_{10^\circ} + y_{50^\circ} - y_{70^\circ} - y_{110^\circ} + y_{130^\circ} + y_{150^\circ}) \\ &+ 12(y_{30^\circ} - y_{90^\circ} + y_{150^\circ})] \end{aligned}$$

$$\begin{aligned} 60\,B_3' = [&(y_{20^\circ} - y_{40^\circ} - y_{80^\circ} + y_{100^\circ} + y_{140^\circ} - y_{160^\circ}) \\ &+ 4(y_0 - y_{60^\circ} + y_{120^\circ}) \\ &+ 5\sqrt{3}(y_{10^\circ} - y_{50^\circ} - y_{70^\circ} \\ &+ y_{110^\circ} + y_{130^\circ} - y_{170^\circ})] \end{aligned}$$

$$\begin{aligned} p(20A_p') = [&\sqrt{3}(y_{2\gamma} + y_{4\gamma} - y_{8\gamma} - y_{10\gamma} + \ldots + y_{(\pi-2\gamma)}) \\ &+ 5(y_\gamma + y_{5\gamma} - y_{7\gamma} - y_{11\gamma} + \ldots + y_{(\pi-\gamma)}) \\ &+ 12(y_{3\gamma} - y_{9\gamma} + \ldots + y_{(\pi-3\gamma)})] \end{aligned}$$

where $\gamma = \pi/(6p)$

$$\begin{aligned} p(20B_p') = [&(y_{2\gamma} - y_{4\gamma} - y_{8\gamma} + y_{10\gamma} + \ldots + y_{\pi-4\gamma} - y_{\pi-2\gamma}) \\ &+ 4(y_0 - y_{6\gamma} + y_{12\gamma} - \ldots + y_{\pi-6\gamma}) \\ &+ 5\sqrt{3}(y_\gamma - y_{5\gamma} - y_{7\gamma} + y_{11\gamma} + \ldots \\ &+ y_{\pi-5\gamma} - y_{\pi-\gamma})] \end{aligned}$$

* *Proceedings of the Physical Society*, 1910–11, vol. 23, p. 341.

* A. RUSSELL: "A Treatise on the Theory of Alternating Currents," 2nd edn., vol. 2, p. 140.

By taking the origin at a point where the wave crosses the axis, as suggested by Russell, y_0 becomes zero and can be omitted from the expressions for the amplitudes of the cosine harmonics.

For greater accuracy, Russell suggests the use of 12 ordinates per half period for the fundamental, and $12p$ for the pth harmonic. The values deduced for the amplitudes of the fundamental sine and cosine terms respectively are then :

$$A_1' = [\tfrac{1}{40}(y_{30°} + 4y_{90°} + y_{150°}) + \frac{\sqrt{3}}{40}(y_{60°} + y_{120°}) + \tfrac{1}{4}\{\sin 15°(y_{15°} + y_{165°}) + \cos 15°(y_{75°} + y_{105°})\} + \frac{3\sqrt{2}}{20}(y_{45°} + y_{135°})]$$

$$B_1' = [\tfrac{1}{40}(4y_0 + y_{60°} - y_{120°}) + \frac{\sqrt{3}}{40}(y_{30°} - y_{150°}) + \tfrac{1}{4}\{\cos 15°(y_{15°} - y_{165°}) + \sin 15°(y_{75°} - y_{105°})\} + \frac{3\sqrt{2}}{20}(y_{45°} - y_{135°})]$$

Applied to standard wave-forms such as the rectangular wave shown in Fig. 2, and to other smooth waves, Russell's method gives remarkably accurate results, undoubtedly more accurate than those obtainable with the same number of ordinates by the Runge method. But when applied to the analysis of wave-forms such as are often met with in practice, the method does not in many cases give such accurate results, even for the lower harmonics, as are obtained with the Runge method. For example, take the case of a wave represented by :

$$y = 100 \sin \theta - 15 \sin 5\theta$$

Taking six ordinates per half period, the amplitude of the fundamental by Russell's method becomes :

$$A_1' = 89{\cdot}5$$

By taking 12 ordinates per half period, no error would result in the value of the fundamental as determined by the Russell method, provided the wave contained no harmonics higher than the fifth.

It is not possible in practice to use as many as $6p$ ordinates per half period if it is required to carry the analysis to harmonics of fairly high orders, for example to $p = 7$ or more. It therefore becomes necessary in applying Russell's method to limit the number of ordinates per half period to 18, 24 or, in especially favourable cases, to 30 ; just as in the Runge method it is not possible to carry the subdivision of the period much beyond the amount chosen by the present author, viz. to 26 parts.

It follows, then, that in the practical application of the Russell method to the analysis of alternating wave-forms, since a limited number of ordinates is used, the value obtained for the various Fourier integrals cannot possibly be correct in all cases. Just as in the case of the Perry and Runge methods, an infinite number of waves may pass through the measured points. The Perry and Runge methods, however, give the amplitudes of one particular wave passing through all of the measured points and, in addition, give the correct amplitudes of the wave under investigation for all cases where the number of ordinates per half period exceeds the highest order of the harmonics present in the wave. With the possible exception of one or two harmonics of very high order, this latter condition is normally fulfilled for the waves occurring in electrical practice if n, the number of ordinates, is greater than 20.

In many cases—probably in the majority of cases for waves normally met with in practice—the Russell method does *not* result in the computed wave passing through all of the measured ordinates.*

Taking, as before, n to be the total number of ordinates per half period, n now being a multiple of 6 in order that Weddle's rule can be applied, it can readily be shown † that the value computed for the pth sine term by the Russell method is :

$$A_p' = \mathbf{A_p} - 0{\cdot}1(A_{2n/3-p} - A_{2n/3+p}) + 0{\cdot}6(A_{n-p} - A_{n+p}) - 0{\cdot}1(A_{4n/3-p} - A_{4n/3+p}) - (\mathbf{A_{2n-p}} - \mathbf{A_{2n+p}}) - 0{\cdot}1(A_{8n/3-p} - A_{8n/3+p}) + 0{\cdot}6(A_{3n-p} - A_{3n+p}) - 0{\cdot}1(A_{10n/3-p} - A_{10n/3+p}) - (\mathbf{A_{4n-p}} - \mathbf{A_{4n+p}}) \quad . \quad (8)$$

It is shown in the Appendix that the actual analysis computed by the Russell method, owing to various of the terms being "loaded," corresponds to the joint effect of three separate analyses carried out by the Runge method, the one with the full number n of ordinates, the second with half the full number, i.e. with $n/2$, mid-ordinates, and the third with one-third of the full number of ordinates, i.e. with $n/3$ ordinates. As a result, the wave-form corresponding to the analysis computed by the Russell method does not of necessity pass through all the measured points, but must pass through one-third of these points, i.e. the third, sixth, ninth, etc.

It will be noticed from (8) that the harmonics of the orders $2Mn \pm p$ have exactly the same effect upon the value deduced for the amplitude of the pth harmonic with the Russell method as with the Perry and Runge methods. But, in addition, the value computed by the Russell method includes two other series of harmonics containing harmonics lower than the nth, and for the case of harmonics of orders greater than $n/3$ the value computed is affected by the presence of harmonics of lower orders than the harmonic under investigation.

Thus, when $n = 18$, the value computed for the amplitude of the fundamental sine term by the Russell method is :

$$A_1' = \mathbf{A_1} - 0{\cdot}1(A_{11} - A_{13}) + 0{\cdot}6(A_{17} - A_{19}) - 0{\cdot}1(A_{23} - A_{25}) - (\mathbf{A_{35}} - \mathbf{A_{37}}) - 0{\cdot}1(A_{47} - A_{49}) + 0{\cdot}6(A_{53} - A_{55}) - 0{\cdot}1(A_{59} - A_{61}) - (\mathbf{A_{71}} - \mathbf{A_{73}}) - \ldots$$

as compared with

$$A_1' = A_1 - (A_{35} - A_{37}) - (A_{71} - A_{73}) - \ldots$$

for the Perry or Runge methods.

The possibilities of error, therefore, in the analysis of wave-forms in which harmonics beyond a certain order are negligible whereas those of a low order may

* By taking a very large number of ordinates for the determination of the higher harmonics, $12p$, for example, sufficiently accurate results can be obtained if it is possible to measure correctly the many ordinates.

† Cf. Appendix 2.

be of importance, are much greater when the Russell method is used with a limited number of ordinates than when the Runge method is used with the same number of ordinates.

Many of the wave-forms met with in electrical practice, although containing only harmonics of comparatively low orders, have quite a large number of points of discontinuity, and for Weddle's rule to give accurate results it is necessary, as pointed out by Russell, to divide the wave into intervals bounded by the neighbouring points of discontinuity and to apply Weddle's rule to each of these separate intervals, in order to obtain accurate results for the fundamental harmonic. For those of higher orders, further subdivision is necessary, since the Fourier integral concerned is

$$\int_0^{\pi} y \sin p\theta \, d\theta$$

In practice, it is not possible to carry the subdivision very far.

For the analysis of very smooth waves where the smoothness really represents the joint action of infinite series of high harmonics, the joint effect of the higher harmonics upon the value computed for the amplitude of the fundamental and for the lower harmonics is much less with the Russell method than with the Perry or Runge methods. The Russell method would therefore appear to be particularly of value in those cases where the wave to be analysed, although smooth, is very rich in higher harmonics well beyond the order corresponding to the number of ordinates per half period, and for which the accuracy obtainable by means of the schedule given in the present paper is insufficient in the case of the lower harmonics. The large number of higher harmonics affecting the computed values of the various amplitudes with the Russell method, as compared with the Runge method, generally results, in the case of smooth waves, in a more complete elimination of their total effect.

For the analysis of the majority of wave-forms met with in electrical practice, containing only a relatively limited number of fairly low harmonics, and generally not by any means smooth, and with a degree of subdivision of the wave-length of the same order as that given in the schedule on page 500, the labour involved in the use of the Russell method would not only exceed that necessary to complete the analysis by means of the schedule, but in the majority of cases the results would be less accurate.

General Comparison of the Runge Method, and its Thompson Simplification, with Other Methods.

It would therefore appear that, for all normal wave-shapes met with in electrical practice, the Runge method outlined in Part (2) of this paper, and the use of the further simplified schedule there given, necessitates less expenditure of time than any other of the methods discussed would require to carry the analysis of the wave-form up to the same order of the harmonics. At the same time, the results obtained by this method will, in general, be much more accurate than those obtained by any other method, the method being, in the majority of cases, exact for all practical purposes. The possible inaccuracies that may obtain can, with the information given in the present paper, readily be located and allowed for.

For rectilinear wave-forms, and for other waves where the exact values of the Fourier integrals concerned can readily be calculated, there is no justification for using any method involving a limited number of ordinates.

For other smooth waves, rich in harmonics of very high orders, the Russell method may be expected to give more exact results than the Runge method. (In this connection, however, it must not be assumed that because a wave is smooth and at the same time deviates appreciably from the sinusoidal form, it necessarily contains harmonics of very high orders.) For all other cases, the method outlined in this present paper will give results which may be expected to be much more accurate than those obtainable by the Russell method.

For waves containing both odd and even harmonics, where it is only required to carry out an approximate analysis up to about the fourth harmonic, although the Runge method is well suited, the Wedmore graphical method has much in its favour.

In conclusion, as a result of the present investigation into the accuracy of the results obtained by basing the harmonic analysis of wave-forms upon the values of a limited number of ordinates, any lack of confidence that there would appear to have been in these results, so far as the analysis of wave-forms occurring in electrical practice is concerned, may certainly be banished. This lack of confidence has arisen, no doubt, partly from a lack of knowledge of the actual error that may be produced by given combinations of circumstances, and partly by the quite apparent errors that have occurred by the ill-advised use of too small a number of ordinates in the attempt to reduce too much the time necessary to carry out the analysis.

APPENDIX I.

The Accuracy of a Modified Form of the Thompson Method of Harmonic Analysis, in which the Calculations are Based upon the Mid-ordinates of the Various Sections.

P. Kemp has, in his paper on "Harmonic Analysis," * based his calculations upon the mid-ordinates of the various sections as in Fig. 3, and has found that, for two particular cases of rectilinear wave-forms, the error obtained is less than with the ordinary application of the Thompson method.

To place correctly the mid-ordinates involves, of course, the division of the half period into twice as many parts as the number of ordinates chosen, and at the same time the method involves twice as many multiplications as the normal method, owing to the fact that it is no longer possible to make use of Runge's beautiful method of grouping the harmonics in pairs. It is desirable, nevertheless, to investigate the theoretical accuracy of

* *Loc. cit.*

this modification. Dealing only with sine terms, and using the same symbols as previously, the effect of the qth harmonic upon the value obtained for the amplitude of the pth harmonic is:

$$A'_{p_q} = \frac{2A_q}{n}\sum\Big(\sin p\frac{\pi}{2n}\sin q\frac{\pi}{2n} + \sin 3p\frac{\pi}{2n}\sin 3q\frac{\pi}{2n} + \sin 5p\frac{\pi}{2n}\sin 5q\frac{\pi}{2n} + \ldots \text{ to } n \text{ terms}\Big)$$

$$= \frac{A_q}{n}\left\{\frac{\cos\frac{1}{2}(q-p)\pi\sin\frac{1}{2}(q-p)\pi}{\sin[(q-p)\pi/2n]} - \frac{\cos\frac{1}{2}(q+p)\pi\sin\frac{1}{2}(q+p)\pi}{\sin[(q+p)\pi/2n]}\right\}$$

$$= \frac{A_q}{n}\left\{\frac{1}{2}\frac{\sin(q-p)\pi}{\sin[(q-p)\pi/2n]} - \frac{1}{2}\frac{\sin(q+p)\pi}{\sin[(q+p)\pi/2n]}\right\}$$

This expression is zero unless $p = q$, or $(q \pm p)$ is a multiple of $2n$.

When $q = p$, $\quad A'_{p_q} = A_q = A_p$.
When $(q-p) = 2Mn$, $A'_{p_q} = -A_q$ when M is odd,
and $\quad = +A_q$ when M is even.
When $(q+p) = 2Mn$, $A'_{p_q} = +A_q$ when M is odd,
and $\quad = -A_q$ when M is even.

Hence:

$$A'_p = A_p + (A_{2n-p} - A_{2n+p}) - (A_{4n-p} - A_{4n+p}) + (A_{6n-p} - A_{6n+p}) - \ldots + \ldots \quad (7)$$

The harmonics which may introduce errors, therefore, include all those which may give rise to errors with the original choice of ordinates used by Perry, Runge, and Thompson. The signs for half of the terms included in the value computed for the amplitude of the pth harmonic are, however, opposite to those occurring with the normal choice of ordinates. It is thus obvious that in certain circumstances greater error will be introduced by using mid-ordinates than by applying the method normally, the actual sign of the various higher harmonics determining the issue.

For the case of 18 ordinates per half period, the following represent the values computed for the various harmonics, if mid-ordinates are used:

$$A'_1 = A_1 + (A_{35} - A_{37}) - (A_{71} - A_{73}) + (A_{107} - A_{109}) - (A_{143} - A_{145}) + \ldots$$
$$A'_3 = A_3 + (A_{33} - A_{39}) - (A_{69} - A_{75}) + (A_{105} - A_{111}) - (A_{141} - A_{147}) + \ldots$$
. . . . . .
$$A'_{17} = A_{17} + (A_{19} - A_{53}) - (A_{55} - A_{89}) + (A_{91} - A_{125}) - (A_{127} - A_{161}) + \ldots$$

It would appear, then, that in cases where the original Perry choice of ordinates would give rise to error in the value computed for any particular harmonic, viz. when the wave-form contains harmonics higher than the number of ordinates per half period, and, in particular, those of the orders $2Mn \pm p$, the use of mid-ordinates may also be expected to result in errors, sometimes greater and sometimes less than with the normal choice of ordinates.

For example, suppose 18 ordinates per half period were used, and the following three waves analysed:

$$e_1 = 100\sin\theta - 5\sin 35\theta + 5\sin 37\theta$$
$$e_2 = 100\sin\theta + 5\sin 37\theta - 5\sin 73\theta$$
$$e_3 = 100\sin\theta - 5\sin 35\theta + 5\sin 73\theta$$

With the normal choice of ordinates, the value computed for the fundamental harmonic of e_1 would be 10 per cent too high, and for the mid-ordinate method the value would be 10 per cent too low.

For e_2, the value computed for the fundamental would be correct with the normal method, and 10 per cent too low for the mid-ordinate method, whereas for e_3 the value computed is correct for the mid-ordinate method, and 10 per cent too high for the normal method.

The extra labour involved in the increased subdivision and number of multiplications necessary with the mid-ordinate method does not, therefore, appear to be warranted.

The above expressions can readily be checked by applying them to any particular case of individual harmonics, or of standard wave-forms such as the rectangular wave; for this latter case the values calculated by means of the above series have been found to accord perfectly with the values tabulated by P. Kemp in his paper.

For the cosine terms it can in a similar manner be shown that the value computed for the amplitude of the pth harmonic is:

$$B'_p = B_p - \{(B_{2n-p}+B_{2n+p}) + (B_{6n-p}+B_{6n+p}) + \ldots\} + \{(B_{4n-p}+B_{4n+p}) + (B_{8n-p}+B_{8n+p}) + \ldots\}$$
. . . . .

It may be mentioned that the schedule given on page 500 affords a means of comparing the normal method with the mid-ordinate method.

In this schedule the odd ordinates are grouped together in one column and the even ordinates in another. The column containing the odd ordinates can thus be utilized for the mid-ordinate method, and the other column for the normal method, there being 13 ordinates per half period in each case.

The sum of the "odd" column gives $\frac{1}{2}(A'_p + A_{26-p})$
The sum of the "even" column gives $\frac{1}{2}(A'_p - A_{26-p})$

APPENDIX 2.

The Accuracy of the Russell Method of Harmonic Analysis.

In applying Russell's method to the actual analysis of a wave-form containing only odd harmonics, the half period is divided into m equal parts, each of which is, in turn, divided into 6 equal parts, so that the total number of subdivisions n is equal to $6m$. In determining the value of the Fourier integrals, Weddle's rule is

applied to each of the sections, the value computed for the amplitude of the pth sine term being:

$$A'_p=\frac{2}{m}\times\frac{1}{20}\Big\{\Big[\Big(y_0\sin 0+y_2\sin\frac{2p\pi}{n}+y_4\sin\frac{4p\pi}{n}+y_6\sin\frac{6p\pi}{n}\Big)+5\Big(y_1\sin\frac{p\pi}{n}+y_5\sin\frac{5p\pi}{n}\Big)+6y_3\sin\frac{3p\pi}{n}\Big]+\Big[\Big(y_6\sin\frac{6p\pi}{n}+y_8\sin\frac{8p\pi}{n}+y_{10}\sin\frac{10p\pi}{n}+y_{12}\sin\frac{12p\pi}{n}\Big)+5\Big(y_7\sin\frac{7p\pi}{n}+y_{11}\sin\frac{11p\pi}{n}\Big)+6y_9\sin\frac{9p\pi}{n}\Big]+\ldots+\text{to } m \text{ terms}\Big\}$$

$$=\frac{2}{m}\times\frac{1}{20}\Big\{\Big(y_0\sin 0+y_1\sin\frac{p\pi}{n}+y_2\sin\frac{2p\pi}{n}+\ldots+y_{n-1}\sin\frac{(n-1)p\pi}{n}\Big)+4\Big(y_1\sin\frac{p\pi}{n}+y_3\sin\frac{3p\pi}{n}+\ldots+y_{n-1}\sin\frac{(n-1)p\pi}{n}\Big)+\Big(y_3\sin\frac{3p\pi}{n}+y_6\sin\frac{6p\pi}{n}+y_9\sin\frac{9p\pi}{n}+\ldots y_n\sin p\pi\Big)\Big\}$$

The ordinates contained respectively in these three component series are placed as indicated in Fig. 6.

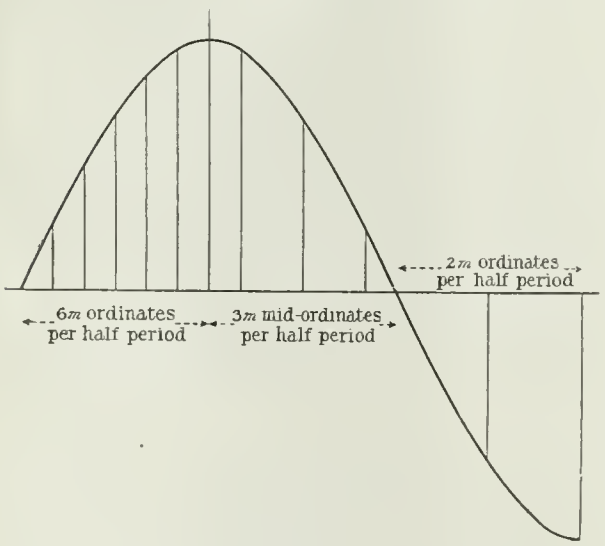


FIG. 6.

For the first series, the spacing corresponds exactly to that obtaining in the ordinary Perry or Runge method; the number of ordinates per half period being $n = 6m$.

For the second series, the spacing of the ordinates corresponds to that advocated by P. Kemp, i.e. to the mid-ordinates, the number of ordinates per half period now being $\frac{1}{2}n = 3m$.

For the third series the spacing is as shown in Fig. 6, and corresponds again to that in the ordinary Perry method, the number of ordinates being $n/3 = 2m$.

The actual values corresponding to the above three series can, therefore, at once be derived from (4), page 494, and from (7), page 507. Hence, the value computed for the pth sine term by the Russell method is:

$$A'_p=\frac{1}{20}\Big\{6[A_p-(A_{2n-p}-A_{2n+p})-(A_{4n-p}-A_{4n+p})-\ldots]+12[A_p+(A_{n-p}-A_{n+p})-(A_{2n-p}-A_{2n+p})+(A_{3n-p}-A_{3n+p})+\ldots]+2[A_p-(A_{2n/3-p}-A_{2n/3+p})-(A_{4n/3-p}-A_{4n/3+p})-\ldots]\Big\}$$

$$=\mathbf{A_p}-0\cdot1(A_{2n/3-p}-A_{2n/3+p})+0\cdot6(A_{n-p}-A_{n+p})-0\cdot1(A_{4n/3-p}-A_{4n/3+p})-(\mathbf{A_{2n-p}}-\mathbf{A_{2n+p}})-0\cdot1(A_{8n/3-p}-A_{8n/3+p})+0\cdot6(A_{3n-p}-A_{3n+p})-0\cdot1(A_{10n/3-p}-A_{10n/3+p})-(\mathbf{A_{4n-p}}-\mathbf{A_{4n+p}})-\ldots$$

Similarly for the cosine terms, the value computed for the amplitude of the pth cosine term is:

$$B'_p=\frac{2}{m}\times\frac{1}{20}\Big\{\Big[y_0\cos 0+y_1\cos\frac{p\pi}{n}+y_2\cos\frac{2p\pi}{n}+\ldots+y_{n-1}\cos\frac{(n-1)p\pi}{n}\Big]+4\Big[y_1\cos\frac{p\pi}{n}+y_3\cos\frac{3p\pi}{n}+y_5\cos\frac{5p\pi}{n}+\ldots+y_{n-1}\cos\frac{(n-1)p\pi}{n}\Big]+\Big[y_3\cos\frac{3p\pi}{n}+y_6\cos\frac{6p\pi}{n}+y_9\cos\frac{9p\pi}{n}+\ldots+y_n\cos p\pi\Big]\Big\}$$

$$=\mathbf{B_p}+0\cdot1(B_{2n/3-p}+B_{2n/3+p})-0\cdot6(B_{n-p}+B_{n+p})+0\cdot1(B_{4n/3-p}+B_{4n/3+p})+(\mathbf{B_{2n-p}}+\mathbf{B_{2n+p}})+\ldots$$

In the Russell method, therefore, not only may errors be introduced as in the Perry and Runge methods due to the presence of harmonics of higher orders than the nth, n being the number of ordinates per half period, but, in addition, errors may be introduced by the presence of harmonics of lower orders than the nth. For example, with 12 ordinates per half period, the value computed for the seventh sine term when a pure sine wave is analysed is 10 per cent of the value of the fundamental; for the ninth a similar value is computed, and for the eleventh the value computed is 60 per cent of that of the fundamental. The use of the Perry or Runge methods with the same number of ordinates would enable a correct result to be obtained. With 18

ordinates per half period, the values computed for the amplitudes of the various sine terms are:

$$\begin{aligned}A_1' = \mathbf{A_1} &- 0{\cdot}1(A_{11} - A_{13}) + 0{\cdot}6\,(A_{17} - A_{19}) \\ &- 0{\cdot}1(A_{23} - A_{25}) - (\mathbf{A_{35}} - \mathbf{A_{37}}) \\ &- 0{\cdot}1(A_{47} - A_{49}) + 0{\cdot}6(A_{53} - A_{55}) \\ &- 0{\cdot}1(A_{59} - A_{61}) - (\mathbf{A_{71}} - \mathbf{A_{72}}) - \ldots \\ A_3' = \mathbf{A_3} &- 0{\cdot}1(A_9 - A_{15}) + 0{\cdot}6(A_{15} - A_{21}) \\ &- 0{\cdot}1(A_{21} - A_{27}) - (\mathbf{A_{33}} - \mathbf{A_{39}}) - \ldots \\ A_5' = \mathbf{A_5} &- 0{\cdot}1(A_7 - A_{17}) + 0{\cdot}6(A_{13} - A_{23}) \\ &- 0{\cdot}1(A_{19} - A_{29}) - (\mathbf{A_{31}} - \mathbf{A_{41}}) - \ldots \\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ A_{11}' = \mathbf{A_{11}} &- 0{\cdot}1(A_1 - A_{23}) + 0{\cdot}6(A_7 - A_{29}) \\ &- 0{\cdot}1(A_{13} - A_{35}) - (\mathbf{A_{25}} - \mathbf{A_{47}}) - \ldots\end{aligned}$$

The terms shown in heavy type represent exactly those occurring in the computed value of the various amplitudes when the Perry method, or any of its modifications, is used.

If only six ordinates per half period are used, every harmonic present will influence the value computed for the amplitude of the fundamental harmonic, the value computed for the fundamental sine term being:

$$\begin{aligned}A_1' = \mathbf{A_1} &- 0{\cdot}1(A_3 - A_5) + 0{\cdot}6(A_5 - A_7) \\ &- 0{\cdot}1(A_7 - A_9) - (\mathbf{A_{11}} - \mathbf{A_{12}}) - \ldots\end{aligned}$$

Conversely, it follows that with this limited number of ordinates the value computed for any of the higher harmonics will be influenced by the value of the fundamental harmonic.

APPENDIX 3.

The Accuracy of the Perry and Runge Methods of Harmonic Analysis in the Case of Waves containing Even, as well as Odd, Harmonics.

Occasionally the electrical engineer has to deal with waves containing even harmonics. The theory given in Part (I) of the present paper has been developed solely for the case of odd harmonics, since in the majority of cases the wave to be analysed is the pressure wave of an alternating-current dynamo, or of an alternating-current system, or an alternating-current wave.

It may happen that, due to some irregularity, even harmonics are set up, and in such cases it is desirable to analyse the wave-form by means of the Runge method.

A consideration of the proofs given for the case of odd harmonics will suffice to show that the results obtained in (4) and (6) hold good also for the case of even harmonics, i.e.

$$A_p' = A_p + (A_{2n+p} - A_{2n-p}) + (A_{4n+p} - A_{4n-p}) + \ldots$$
$$B_p' = B_p + (B_{2n+p} + B_{2n-p}) + (B_{4n+p} + B_{4n-p}) + \ldots$$

whether p is odd or even.

APPENDIX 4.

Schedule for the Analysis of Wave-forms, containing Odd Harmonics only, up to the 13th.

This schedule is on exactly the same lines as that given in Part (2) of the present paper. It is shown completed for the analysis of a wave containing cosine terms only, and represented by

$$y = 1\,000\cos\theta + 100\cos 3\theta - 50\cos 5\theta + 20\cos 11\theta + 20\cos 13\theta.$$

For use with this schedule it is desirable to have available the following information relating to the effect of the higher harmonics upon the value computed for the amplitude of any particular harmonic. Neglecting harmonics higher than the 41st, we have:

$$\begin{aligned}A_1' &= A_1 - A_{27} + A_{29}; & B_1' &= B_1 + B_{27} + B_{29} \\ A_3' &= A_3 - A_{25} + A_{31}; & B_3' &= B_3 + B_{25} + B_{31} \\ A_5' &= A_5 - A_{23} + A_{33}; & B_5' &= B_5 + B_{23} + B_{33} \\ A_7' &= A_7 - A_{21} + A_{35}; & B_7' &= B_7 + B_{21} + B_{35} \\ A_9' &= A_9 - A_{19} + A_{37}; & B_9' &= B_9 + B_{19} + B_{37} \\ A_{11}' &= A_{11} - A_{17} + A_{39}; & B_{11}' &= B_{11} + B_{17} + B_{39} \\ A_{13}' &= A_{13} - A_{15} + A_{41}; & B_{13}' &= B_{13} + B_{15} + B_{41}\end{aligned}$$

The difference between the mid-ordinate and the normal method, using 7 ordinates per half period, is again well illustrated by the columns containing respectively the odd and the even ordinates.

SCHEDULE FOR ANALYSING WAVE-FORMS UP TO THE 13TH HARMONIC (ODD HARMONICS ONLY).

SCHEDULE A: FOR SEPARATING OUT THE SINE AND COSINE TERMS.

Ordinates of Wave	y_0 *1 090*	y_1 *996*	y_2 *977*	y_3 *780*	y_4 *538*	y_5 *307*	y_6 *106*	y_7 *0*
Ordinates of Wave	×	y_{13} *− 996*	y_{12} *− 977*	y_{11} *− 780*	y_{10} *− 538*	y_9 *− 307*	y_8 *− 106*	×
Sum—Sine Terms	×	1 /	2 /	3 /	4 /	5 /	6 /	7 /
Difference—Cosine Terms	7′ *1 090*	6′ *1 992*	5′ *1 954*	4′ *1 560*	3′ *1 076*	2′ *614*	1′ *212*	×

NOTE.—The half period is divided into 14 equal parts; y_0 is the ordinate at the chosen origin; y_1, y_2, etc., are the ordinates at the points of division.

SCHEDULE B: FOR ANALYSING EITHER THE SINE OR THE COSINE TERMS.

Schedule used for: *Cosine Terms.*

Multiplying Factors	θ and 13θ				3θ and 11θ				5θ and 9θ				7θ
	Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates		Odd Ordinates		Even Ordinates		
	+	−	+	−	+	−	+	−	+	−	+	−	
0·0318	1 *7*					−5 *62*				−3 *34*			
0·0620			2 *38*				4 *97*				6 *123*		
0·0890	3 *96*				1 *19*					−5 *174*			
0·1117			4 *174*					−6 *222*			2 *68*		
0·1287	5 *251*				3 *138*				1 *27*				
0·1393			6 *278*				2 *86*					−4 *217*	
0·1428	7 *156*					−7 *156*			7 *156*				0·1428 {Sum of ordinates 1 + 5) − (Sum of ordinates 3 + 7)} / (212 + 1 954) − (1 076 + 1 090) = A_7 or − B_7 / 2 166 / 2 166
Individual Sums of + and − columns ..	*510*		*490*		*157*	*218*	*183*	*222*	*183*	*208*	*191*	*217*	
Sum of Odd Ordinates and Sum of Even Ordinates	*510*		*490*		*− 61*		*− 39*		*− 25*		*− 26*		
Σ Odd Ordinates .. + Σ Even Ordinates ..	A_1 = (or B_1 = *1 000*)				A_3 = (or − B_3 = *− 100*)				A_5 = (or B_5 = *− 51*)				A_7 = (or − B_7 = /)
Σ Odd Ordinates .. − Σ Even Ordinates ..	A_{13} = (or B_{13} = *20*)				A_{11} = (or − B_{11} = *− 22*)				A_9 = (or B_9 = /)				

Computed Analysis *: $y = 1\,000 \cos\theta + 100 \cos 3\theta - 51 \cos 5\theta + 22 \cos 11\theta + 20 \cos 13\theta$.

* The ordinates y_0, y_1, etc., have been calculated for the wave: $y = 1\,000 \cos\theta + 100 \cos 3\theta - 50 \cos 5\theta + 20 \cos 11\theta + 20 \cos 13\theta$.

SOME DEVELOPMENTS IN MULTI-SPEED CASCADE INDUCTION MOTORS.

By F. Creedy, Associate Member.

(*Paper first received* 24 *August, and in final form,* 5 *November,* 1920; *and read before the* North Midland Centre 12 *April,* 1921.)

Summary.

The paper is divided into four Sections and an Appendix.

(1) Section I reviews previous work and shows that earlier conceptions were capable of very wide generalization. By this means many new combinations are brought to light, both in principle and in construction distinct from any hitherto described.

(2) Section II is a discussion of the theory of the cascade motor from a new point of view, which avoids the difficulties that have hitherto prevented the subject becoming known in its full generality.

(3) Section III and the Appendix describe some new rotor windings which remove some existing limitations of the cascade type. Hitherto single-speed machines running faster than 500 r.p.m. (on 50 periods) could not be built, but the present paper describes a machine capable of running at 750 r.p.m. With three speeds in the ratios 1 500/750/500 there are often either too few speeds or too great an interval between them. Apparatus is described giving as many more speeds as desired, with smaller intervals between them, particularly a machine giving speeds in the ratios 750/500/375/300 and having the characteristics of known cascade machines at every speed.

(4) Section IV describes stator windings corresponding to the above rotor windings.

SECTION I.

General Principles.

One of the most urgent problems at the present day in connection with the further development of the alternating-current system of electric power supply is that of the development of a satisfactory type of variable-speed alternating-current motor. It has often been pointed out to the author by operating engineers that in many cases the steam engine holds its place against the electric motor solely, or mainly, on account of its usefulness as a variable-speed machine, and that its supersession depends entirely upon the development of a satisfactory motor having somewhat similar characteristics.

Numerous methods of solving this problem have been suggested, notably those depending on polyphase commutator machines, those involving machines capable of running with a varying number of poles, and lastly the internal cascade machine with a single winding on each member as invented and developed by Mr. L. J. Hunt.

Without discussing the two former types in detail the author believes that the polyphase commutator machine is only likely to find application in small sizes, say less than 50 h.p. As regards the multi-speed polyphase motor capable of running with several different numbers of poles, the author believes this to present very great possibilities indeed and to be capable perhaps of giving a greater number of efficient speeds having smaller intervals between them than even the cascade motor.

The present paper, however, is devoted to the cascade type, leaving a very much greater volume of research relating to the pole-changing type for publication on another occasion. There can be no doubt that the cascade motor has a wide field of application and, if we really wish to solve the variable-speed alternating-current motor problem, every type of suggested solution must be thoroughly investigated. It will be generally agreed that the results obtained by Mr. Hunt in the cascade motor are a landmark in the history of the development of the induction machine and, were it not for exigencies of space, the author would like to express more fully his admiration of the patience and ingenuity shown in bringing the cascade motor to its present position. As Mr. Hunt himself says (page 426) in his paper referred to below, "cascade operation is full of possibilities"; and the present paper is an endeavour to realize some of these possibilities by viewing the matter from an independent standpoint. The principal references on the subject will be: L. J. Hunt: "A New Type of Induction Motor," *Journal I.E.E.*, 1906, vol. 39, p. 648; and "The 'Cascade' Induction Motor," *ibid.*, 1914, vol. 52, p. 406; also British Patents 6 224/1906, 15 711/1906, and 16 170/1909.

Passing briefly in review the general principles of the cascade motor it is known that, if the primary winding of an induction motor be connected to the secondary of another, the same machines being coupled together mechanically, they will run at a speed corresponding to the sum of the numbers of poles in the two machines for a particular direction of relative rotation of the fields, and at a speed corresponding to the difference between these numbers for another direction of relative rotation.

The objection to the use of two distinct machines in a cascade set is the great expense involved and the poor characteristics of the set. In order to obviate this it has been proposed by several inventors to combine the two cascade motors into one machine, the difficulty in accomplishing this being due to the fact that, unless proper precautions are taken, there will be mutual induction between the two machines. The only practical method of avoiding this is to wind the two machines for dissimilar numbers of poles, a method which, according to Mr. Hunt, was first put forward by Mr. F. Lydall. A motor built on this principle will have two distinct sets of windings, two on the stator and two on the rotor. Such machines have been built and experimented with by Mr. Hunt, but have serious

disadvantages due to the high leakage and resistance of the different windings and also, one may add, to the difficulties of ventilation. The essential improvement introduced by Hunt is to show how such a motor may be built with only one winding on each member.

Relative numbers of poles in the two fields.—Certain limitations are introduced by the fact that we have two distinct fields operating in the same magnetic structure, because not every combination of numbers of poles is capable of operating correctly under these conditions. Hunt has stated two conditions which limit the possible pole combinations :—

(*a*) The numbers of poles in the two fields must be so chosen that their windings are mutually non-inductive.

(*b*) The two fields when superposed must not produce an unbalanced radial pull on the rotor.

Passing over the first condition, let us consider what pole combinations are necessary in order to avoid an unbalanced radial pull on the rotor. Perhaps the clearest statement that Hunt has given of a rule to satisfy this condition is in his Patent 16 170/1909, page 5, line 8: "For satisfactory working and symmetrical distribution of the forces, the two numbers of poles for the rotor winding must be such that when these numbers are divided by their greatest common factor, the one dividend must be odd and the other even, whilst the common factor itself must be greater than 2."

On page 408 of his paper in Vol. 52 of the *Journal* he states that, to satisfy the condition of magnetic balance, the greatest common factor between the two numbers of poles must be a number greater than 2. Any two numbers of poles must have a common factor of 2, since they are necessarily even. By the Hunt rule they are also to have a common factor other than 2, and hence it is clear arithmetically that their highest common factor must be a multiple of 2. More concisely stated this simply means that the two numbers of pairs of poles shall have a common factor. The effect of this is that the circumference is divided into two or more identical zones or sections, so that any values of magnetic density, current, etc., which occur at any one point occur also at a diametrically opposite point or at 3, 4, 5, etc., equally spaced points, according as the common factor between the numbers of pairs of poles is 2, 3, 4, 5 or any other number.

The effect of this is, of course, to ensure magnetic balance, since identically the same magnetic pull as occurs at any one point will occur at two or more equally spaced points. We shall see below that this condition, while correct and sufficient, is by no means necessary. In the first place it is clear that, if the necessary condition of magnetic balance is that the same magnetic density must occur at two or more equally spaced points round the circumference (the condition to which Hunt's rule leads us), then this condition must be satisfied not only by cascade machines but by every other type of machine if it is to be magnetically balanced. But the briefest study will show that it is not satisfied, for instance, by a 2-pole induction motor or any other 2-pole machine, since there is no point in such a machine which has at every instant the same magnetic density as any given point. Hence it is clear that Hunt's rule cannot be perfectly general.

The explanation of this apparent difficulty is as follows :—

The magnetic attraction at any point on the circumference is proportional not to the magnetic density but to its square. Hence not merely points having equal density, but those having equal and opposite densities, will give rise to the same magnetic attraction. In the case of the 2-pole machine diametrically opposite points have equal and opposite magnetic densities, and hence the squares of these two densities are equal and the magnetic pull at diametrically opposite points is the same. The most general condition is, therefore, that at every instant any value of the square of the magnetic density which appears at any point shall also appear at one or more other points equally distributed round the circumference with respect to the first.

For convenience in application it is necessary to translate this generalized rule into a relation between numbers of poles, and we find that we may distinguish two distinct ranges of cascade motors which will be magnetically balanced :—

(*a*) The Hunt type in which the numbers of pairs of poles have a common factor.

(*b*) A new type in which the numbers of pairs of poles are prime to one another and both odd.

In fact, in all machines having odd numbers of pairs of poles diametrically opposite points have equal and opposite magnetic densities. Hence if two fields both having an odd number of pairs of poles are superposed, and the values of the densities due to each at any given point are X and Y, say, so that the resultant density at that point is $X + Y$, then at a diametrically opposite point the densities due to each field will be $-X$ and $-Y$, and the resultant density $-(X + Y)$. The squares of the densities at the two diametrically opposite points will thus be equal. Thus we find it is possible to generalize Hunt's rule for magnetic balance, and thereby to disclose an entirely new range of cascade machines, many of which have very valuable properties.

The cascade machine has hitherto been considered to be essentially a low-speed type. This arises from the facts that the number of poles in the cascade motor must be the sum of the number of poles of two distinct machines, and that both numbers of pairs of poles are sufficiently high to permit of there being a common factor between them. The new range of machines does not necessitate there being any common factor between the two numbers of pairs of poles. Hence it permits of the production of machines of a higher speed than has hitherto been possible. Described below in detail is a combination having 2 and 6 poles which gives a cascade speed corresponding to 8 poles, i.e. 50 per cent higher than the highest cascade speed obtainable from machines built under Hunt's rule.

We now have to consider whether any limitation in the pole combinations is required in order to enable the windings to be mutually non-inductive. Where a winding of the type described by Hunt is used as a stator winding, it is necessary that the numbers of

pairs of poles when divided by their greatest common factor shall be odd in one case and even in the other. But it is possible to design types of stator winding other than that described by Mr. Hunt to which this limitation does not apply, and examples of these will be found below. Obviously, since in the new type of mechanically balanced machines both numbers of pairs of poles must be odd, the condition imposed by Mr. Hunt's stator winding would not be met, and it is necessary to have recourse to other methods, which will be found described below.

In the general case where two distinct stator windings are used it may readily be shown that almost any two stator windings containing no closed circuits will be mutually non-inductive, there being only a few exceptions practically confined to cases where the number of poles in one case is a direct multiple of that in the other. Thus, by using two distinct windings on the primary member, neither of which contains any closed circuits, we can produce an internal cascade motor capable of operating on practically any combination of poles, with the possible exception of poles which are exact multiples of one another. Therefore, in studying the rotor windings as we shall do below, we need not be handicapped by the thought that pole combinations which give rise to a possible rotor winding cannot be used because an appropriate stator winding cannot be found for them.

Rotor windings.—It will be generally agreed that the rotor winding is both that part of the internal cascade machine which is most difficult to understand, and also the part which differentiates it from every other class of apparatus. An attempt has therefore been made to consider it from an entirely novel and very fundamental standpoint, in a manner quite independent of any other method. This has the advantage of making clear some very interesting features of these windings. It is shown below that a winding adapted to any given cascade speed can operate with any balanced combination of fields which gives rise to the given cascade speed. For instance, a rotor adapted to run with a 20-pole cascade speed can operate with either a combination of fields giving 8 and 12 poles, or a combination of fields giving 16 and 4 poles. Now, considered as a variable-speed motor, perhaps the most striking feature of the internal cascade machine is that when the slip-rings are short-circuited the motor rises to the speed corresponding to the number of poles for which the primary is wound. Thus, a machine having a cascade speed corresponding to 12 poles and a primary wound for 8 poles will, when the slip-rings are short-circuited, rise from the 12-pole speed to the 8-pole speed. If the primary is arranged for 4 poles, however, it will rise from the 12-pole speed to the 4-pole speed. The same characteristic is retained in the case just mentioned. For instance, if the stator winding is arranged to give either 4, 8, 12 or 16 poles, the motor will rise from its 20-pole cascade speed to 16-pole speed, to 12-pole speed, to 8-pole speed, or to 4-pole speed, according to which of these numbers of poles the stator winding is arranged for. Hence, instead of the three-speed cascade motor as hitherto developed it is found possible by this means to obtain a five-speed cascade motor. Similarly, a cascade motor arranged to give a cascade speed corresponding to 28 poles can be arranged with the same rotor winding to operate on 4, 8, 12, 16, 20 and 24 poles, rising to any one of these speeds, on short-circuiting the slip-rings, according to which of them the stator winding is arranged for.

SECTION II.

Principle of the Secondary Windings.

It has been mentioned that the essential part of the cascade motor is its rotor winding, and it is accordingly essential that this should be clearly understood. One of the clearest ways of regarding the subject seems to be the following. Many people are familiar with the acoustic phenomenon known as "beats," and most electrical engineers are acquainted both with the method of synchronizing an alternator by means of the alteration in the flickering of a lamp as synchronism is approached, and with the explanation thereof. The compound winding of the cascade rotor is an application of this phenomenon of "beats" in another field. "Beats" in music or the flickering of the synchronizing lamp are due to the following cause.

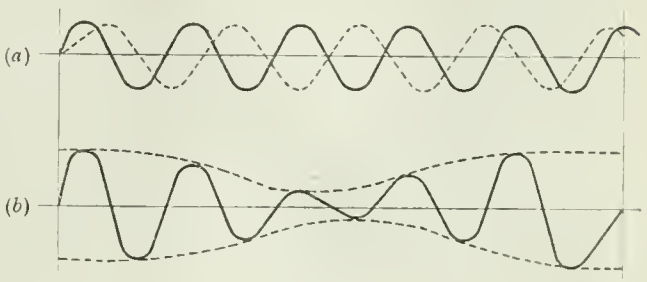


Fig. 1.

Let there be two E.M.F. waves of different frequency which are plotted in Fig. 1, in which the ordinates represent E.M.F., and the abscissæ time. Starting with both waves approximately in phase (passing through zero together), after a certain number of periods, due to the difference of periodic time, the two waves become opposite in phase and the amplitude of the resultant wave [Fig. 1 (*b*)], which was first of all the sum of the amplitudes of the two component waves, now becomes their difference. After a further number of periods the waves are in phase again, and the resultant is again the sum of their amplitudes. The period of this cycle or "beat" is the difference between the periods of the two component waves.

As an application of this phenomenon will be described below, it is desirable to dwell upon it further.

Another method of studying the subject which is often adopted is that of the clock diagram, whereby each wave is represented by a revolving vector whose projection on a vertical axis, say, represents its value at any moment in one complete revolution. Since the periods of the two waves are unequal the rates of revolution of the two vectors are also unequal.

Fig. 2 shows the two vectors, the resultant being the vector sum of the two (projected on the vertical as usual). Clearly the resultant is a maximum when the two vectors have the same direction, and a minimum when they have opposite directions. The relative speed of the two vectors is clearly also the difference of their actual speeds, and hence the frequency of the beat will be the difference between the frequencies of rotation of the two vectors. Thus, when the two vectors have accomplished a relative movement of 180°, which is clearly a half-period of relative motion,

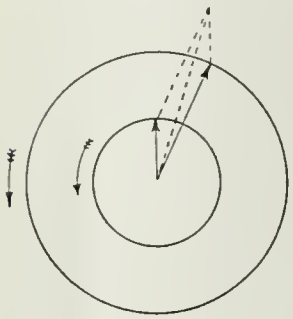


Fig. 2.

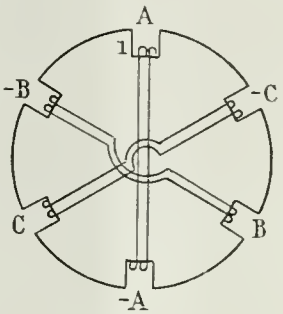


Fig. 3.

the resultant curve has changed from maximum to minimum.

Now let us see how the above remarks apply to the cascade motor. Consider first a plain drum armature or rotor with 6 slots (Fig. 3) arranged for three-phase currents represented as vectors in Fig. 4. Let us set out the ampere-conductors per slot in the form of a vector diagram. In slot 1 the ampere-conductors are in phase A, and may be represented by a vertical vector of suitable length. Similarly in slots 3 and 5 the ampere-conductors are in phases B and C respectively, and may

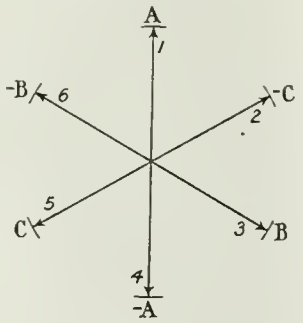


Fig. 4.

be represented by vectors of the same length, since there is the same number of conductors in each slot, making 120° and 240° respectively with the first. In slot 4 we have the return conductors of slot 1. Hence the ampere-conductors of slot 4 are equal and opposite to those of slot 1, and similarly those of slots 6 and 2 are equal and opposite to those of 3 and 5.

Examining the diagram obtained by this process we see that the vector for each slot is of equal magnitude and set out from a common origin, and that on making the circuit of the rotor from slot to slot the vectors corresponding to slot 1 may be regarded as rotating and moving through equal angles for equal distances travelled through, as measured round the circumference. With only 6 slots the vector proceeds by steps, but clearly in an ideal machine we should have an infinite number of slots each carrying currents differing in phase by a very small angle proportional to the distance from slot to slot. In this case, as we travel uniformly round the rotor, the ampere-conductor vector in our diagram would also rotate uniformly.

So far we have only considered a 2-pole rotor. Now suppose we have 12 slots and a 4-pole rotor (Fig. 5).

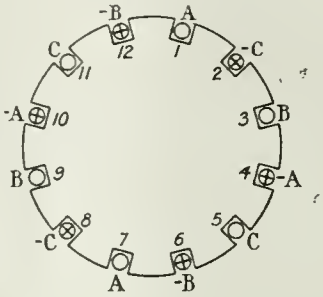


Fig. 5.

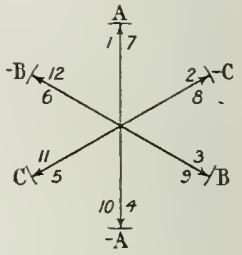


Fig. 6.

The vector diagram remains exactly the same as before, but the revolving vector makes a complete revolution in travelling from slot 1 to slot 7, or two complete revolutions travelling round the whole circumference. Each vector therefore represents the ampere-conductors in two slots such as 1 and 7, 2 and 8, etc., instead of in one (see Fig. 6). In general it is clear that for n pairs of poles the vector makes n complete revolutions while we go once round the rotor. Comparing therefore the same rotor wound for, say, 2 and 4 poles, we see that with the 4-pole winding for a given distance, say one-twelfth, travelled round the circum-

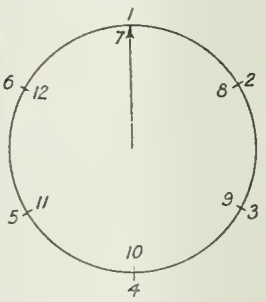


Fig. 7.

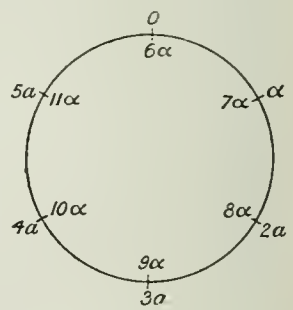


Fig. 8.

ference, there is double the angular displacement of the rotating vector compared with that on 2 poles. In other words, on 4 poles the rotating vector revolves at twice its 2-pole speed (see Fig. 7).

This fact enables us to distinguish a rotating vector drawn in this way from an apparently similar one representing the rotating flux or ampere-turns due to such a rotor on a time basis. Such a rotating vector revolves only half as fast in the 4-pole case as in the 2-pole, the speed being inversely proportional to the number of poles, while the speed of the vector drawn

in the above-mentioned manner is directly proportional thereto. In all matters of this kind the utmost clearness as to fundamentals is essential, and by proceeding to deduce results without making sure of fundamentals serious errors may be committed. Therefore no apology is needed for carefully distinguishing the present diagram from a time diagram.

A convenient practical method of drawing a diagram of the above type is as follows :—

Draw a circle of radius equal to the length of the rotating vector; divide the circumference into any

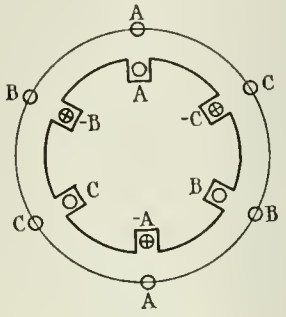


Fig. 9.

suitable number of divisions, and number these divisions 1 to n. Determine the angular displacement corresponding to any one division. This will be

$$\frac{(360^\circ \times \text{number of pairs of poles})}{\text{Number of divisions}} = a$$

Starting from any point mark off angular distances a, $2a$, . . . na (which will be a multiple of 360°) along the circumference of the circle representing the revolving vector. Mark the points so obtained 1, 2 . . . n, corresponding to the number of divisions of the circumference. For instance, with $n = 12$ and 2 pairs of poles, $a = 60^\circ$, and we get the result shown in Fig. 8, in which it is clearly unnecessary to repeat the angle in marking each division.

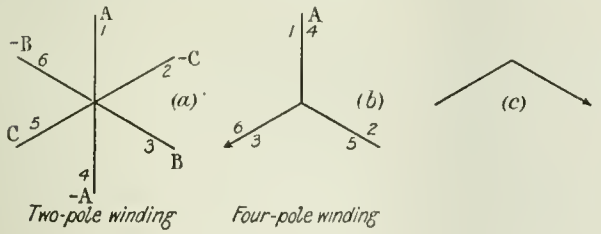


Fig. 10.

Having explained how we may draw a vector diagram in the form of a rotating vector to represent the ampere-conductors of a polyphase winding on an ideal machine having a very large number of slots and phases, we may proceed to apply this diagram to the cascade motor.

Returning to the 6-slot rotor of Fig. 3, let us now suppose that, in addition to the 2-pole winding we considered before, the rotor is fitted with a 4-pole winding as shown in the outer circle of Fig. 9, the six bars composing it being imagined joined in star at one end of the armature and to the terminals at the other. The vector diagrams corresponding to these two windings are shown in Fig. 10. If we consider any particular slot, say slot 2 for instance, the resultant ampere-conductors in it are the vector sum of those due to the 2-pole winding and to the 4-pole winding. That is to say, we have to add the vectors from Figs. 10 (*a*) and 10 (*b*) which correspond to the same point on the circumference, i.e. the vectors marked 2 in the above diagrams, whose resultant is shown in Fig. 10 (*c*).

The general rule, therefore, for finding the resultant vector of ampere-conductors for any number of windings on the same member is to take the resultant of all the vectors of the original windings corresponding to the same point on the armature.

Let us apply this to the case where the ampere-conductors in both windings are represented by uniformly revolving vectors, which for the sake of clearness we may suppose to revolve, one at twice the rate of the

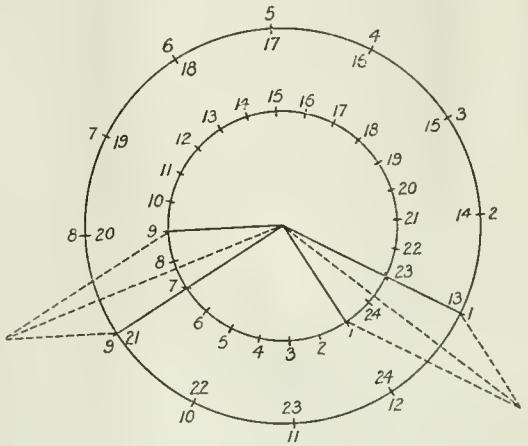


Fig. 11.

other, in opposite directions. By choosing a particular ratio of magnitude, i.e. the 4-pole vector equal to 1·732 times the 2-pole vector, we get a case corresponding to one of the cases discussed by Mr. Hunt, which will accordingly permit us to compare our method with his.

Fig. 11 shows a diagram proportioned and arranged in this manner, the inner or 2-pole vector rotating clockwise with half the speed of the outer. The numbers on the inner circle show that the circumference of the rotor has been divided into 24 equal parts independently of the number of slots (if any). The same numbers appear on the outer circle angularly displaced by an amount twice as great. A particular phase appears twice at points diametrically opposite on the rotor, so that it is clear that the vector makes two complete revolutions in the same time that the inner vector makes one. The circumference is divided into a definite number of parts to enable us to identify the vectors on the two circles which correspond to the same point on the rotor. The number of divisions is chosen merely to give a sufficient number of points to enable us to draw a smooth resultant curve, and not with any reference to the number of slots.

It will be seen that we have now arrived at a diagram

identical in principle with Fig. 2, by which we illustrated the "beats" of a synchronizing lamp, except that in the former the different positions of the rotating vector correspond to different instants of time instead of to different points on the circumference of a rotor. Fig. 2 is a "time" diagram and Fig. 11 a "space" diagram, otherwise they are identical in principle.

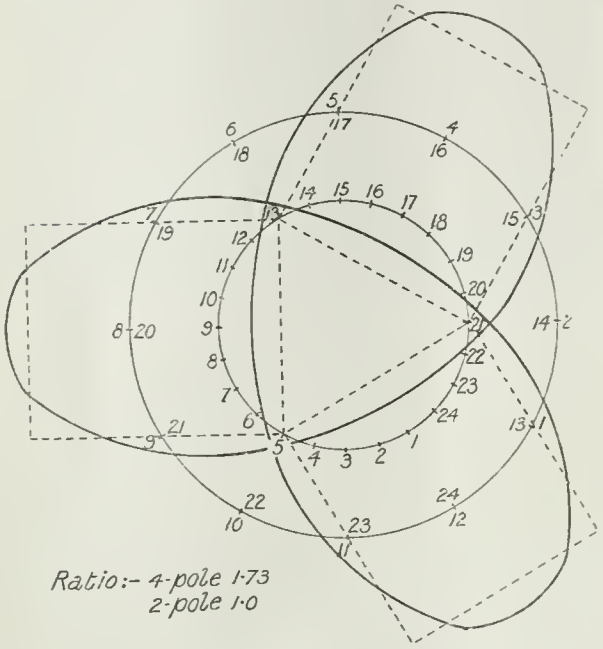


Fig. 12.

By means of the two rotating vectors of Fig. 11 let us plot out the resultant curve of ampere-conductors round the rotor due to 2-pole and 4-pole windings rotating in opposite directions. This is done by first forming the vector sum of the two component vectors corresponding to point 1, marking its extremity as the resultant at point 1; next forming the resultant of the two vectors corresponding to point 2, marking its extremity as the resultant at point 2; and so on round the circumference. In this way we plot out point by point a curve having three symmetrical lobes, which gives us the resultant ampere-conductors both in magnitude and in phase at any point round the circumference. This curve is shown in Fig. 12, and as it is of the utmost interest and importance we shall devote some space to studying it and its relation to other methods of regarding the subject.

Consider, for instance, Hunt's investigation given in his paper above referred to (p. 411 *et seq.*). In this paper he considers 4-pole and 2-pole windings rotating in opposite directions, as we have done (for the present investigation has been modelled to give results as comparable as possible with his). He chooses a definite number of slots (12) and winds into these slots two distinct windings, so connected and of such pitch that

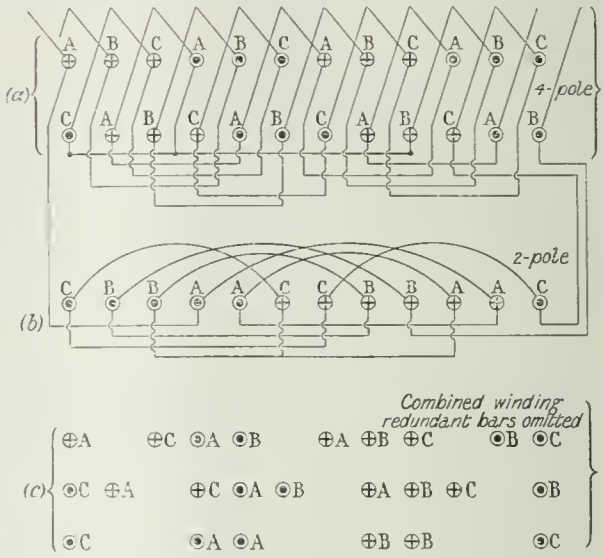


Fig. 13.

the ampere-conductors per slot of the 4-pole winding are 1·732 times those of the 2-pole winding. These two windings are connected in series in such a manner that the R.M.S. currents in all conductors are equal in magnitude, and a symbol is attached to each conductor indicating its phase.

A diagram of these two windings in series is given

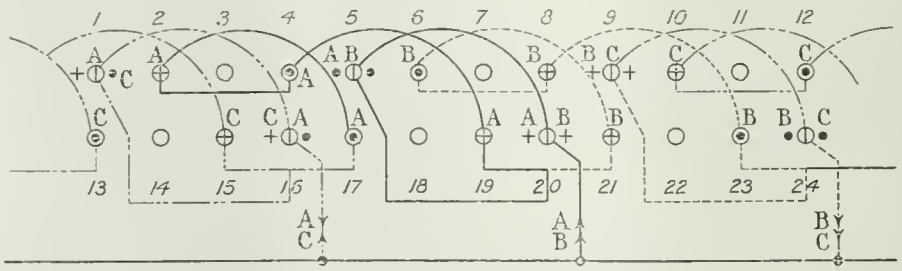


Fig. 13 (*a*).

in Fig. 13 [Figs. 7 (*a*) and 7 (*b*), p. 412 of Hunt's paper]. By a suitable arrangement of the relative position of the two windings Hunt then succeeds in showing that in certain slots there are bars carrying equal and opposite currents which he then cancels. This process, it is clear, is one of finding the resultant ampere-conductors of two distinct component windings at a par-

ticular slot. That is to say, Hunt's process and the one illustrated above are the same in principle.

Nothing could be more conclusive or more graphic than Hunt's process where it is applicable, but the existence of so many different variables, for instance (1) number of slots, (2) conductors per slot on the first winding, (3) conductors per slot on the second winding, (4) pitch of first winding, (5) pitch of second winding, (6) relative displacement between the two windings, (7) number of phases, (8) connection of windings (star, mesh, etc.), makes it a process very limited and difficult to apply. On the other hand, the vector method of drawing the resultant eliminates nearly all of these variables and enables us to study cases where the use of Hunt's method is impossible, or is at least extremely difficult of application.

We shall now, for comparison with Fig. 12, draw a diagram of ampere-conductors represented as vectors for the case of an armature having 12 slots and carrying the winding developed by Hunt. Hunt's method of indicating the phases of his currents differs so widely from the vector method that it is necessary first of all to prepare a little dictionary of symbols whereby we may translate his symbolism into vector diagrams, thus :—

Symbol	Magnitude	Phase
A ⊕	1·0	0°
B ⊕	1·0	60°
C ⊕	1·0	120°
A ⊙	1·0	180°
B ⊙	1·0	240°
C ⊙	1·0	300°

That is to say, where Hunt marks a bar as in the left-hand column, the phase of the bar is that shown in the right-hand column.

ductors per bar will be 1·73 times those in any one conductor. We also note that the phase difference between conductors in adjacent slots is always 60°. In the 2-pole winding adjacent slots have the same currents in them in pairs, as 2 and 3, 4 and 5, etc.

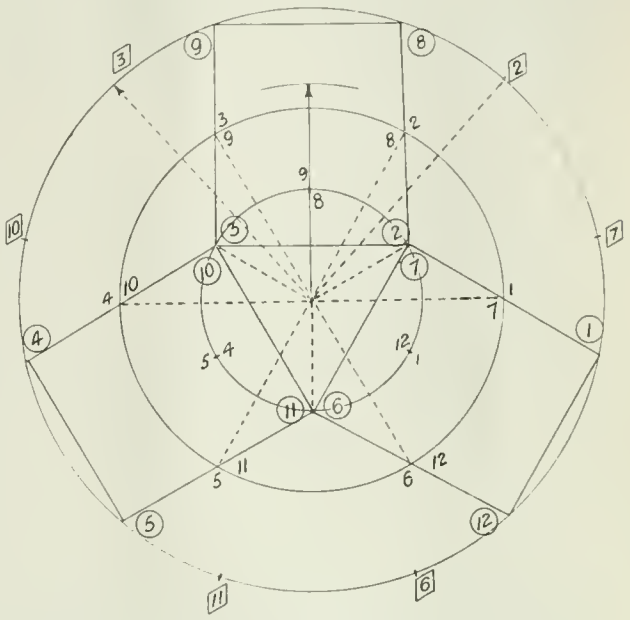
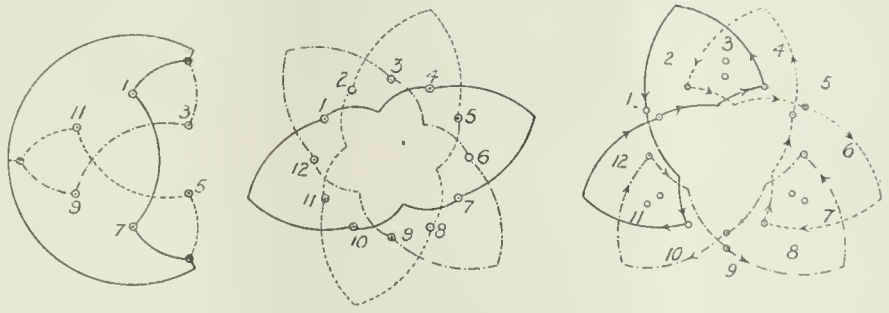


Fig. 14.

From these data we may draw Fig. 14, containing two concentric circles of radii in the ratio of 1·73 : 1, their circumferences being numbered in the manner shown. By producing the resultant of vectors to points on the two circles similarly numbered, and joining the extremities of these vectors, we get Fig. 14, which corresponds to Fig. 12, except that the vectors change from one value to the next in steps instead of gradually. In Fig. 12 this polygon is shown dotted



(a) (b) (c)

Fig. 15.

With the aid of this table we may draw the vector diagram corresponding to each of Hunt's component windings. First of all we note, as Hunt points out (see p. 412 of his paper), that the phase difference between top and bottom conductors in the 4-pole winding is always 60°. Hence the resultant ampere-con- and superposed on the 3-lobed figure derived from the previous calculation. It will be seen that this polygon represents an approximation as close as can be obtained with only 12 slots. In a practical case there would be more slots, say not less than 48, which would give a still closer approximation.

Hunt has shown so conclusively the identity of his two component windings with his resultant winding that it is unnecessary for us to compare again this latter with our ideal 3-lobed figure. Already, however, we begin to see what is the essential characteristic of the cascade rotor, viz. that certain parts of the armature have a greater number of ampere-conductors per slot than others.

In addition to the winding we have just analysed,

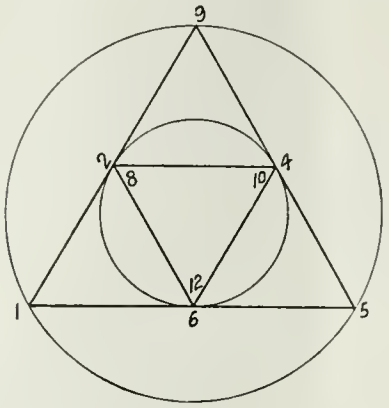


Fig. 16.

which is the one chiefly employed in practice, Hunt has described, chiefly in his patents, several other windings. It will be instructive to draw corresponding ampere-conductor diagrams for these windings also,

Referring therefore to Hunt's British Patent No. 15 711/1906 and his Figs. 1, 2 and 3 (reproduced in Fig. 15 here), let us endeavour to translate the resultant diagram [Fig. 15 (*c*)] into a vector diagram in the same manner as before.

The same table as previously used will serve to translate Hunt's symbols into vectors.

In the polygon (triangle) obtained in Fig. 16 we clearly have an approximation to a 3-lobed figure such as that of Fig. 12.

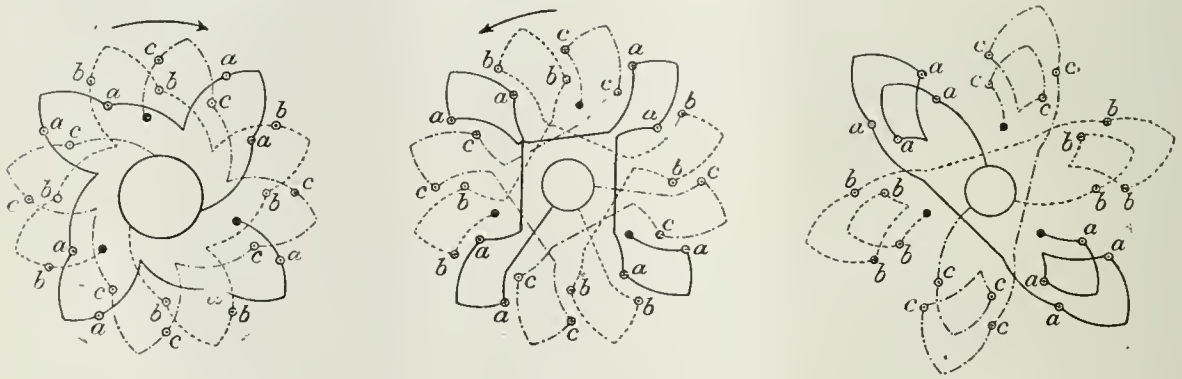
Another curious winding is shown in Hunt's Figs. 4, 5 and 6 (reproduced here in Fig. 17), (a) and (b) representing the components, and (c) the resultant winding

Another 3-lobed figure of a rather imperfect type is shown in Fig. 18, derived from Fig. 17 (*c*).

It should now be obvious from the examples we have discussed that *any* winding which yields a vector diagram approximating sufficiently closely to our original lobed figure can be used as a cascade secondary. For instance, (c) in Fig. 17 may be much improved by the addition of some further conductors, thereby making it practically equal to Fig. 14. Diagrams of this improved winding and its vector diagram are shown in Fig. 19.

Fig. 19 is a vector diagram and diagram of connections for a winding having 36 slots connected as shown, which has merely been built up empirically so as to give a vector polygon as close as possible to the ideal curve, which it approaches very closely. No attention has been paid to determining whether such a winding can be arrived at by superposing two other windings, and the principal object of explaining it is to make it clear that this is not in any sense a necessary condition, and that the sole condition to be met is that it shall conform as closely as possible to the ideal curve. From the diagram of connections it will be seen how the fundamental characteristic of the cascade rotor winding, i.e. that of having a periodic variation corresponding to the "beats" or ampere-conductors per slot, is shown, since the number of conductors per slot varies from 1 to 3.

We have thus shown how actual windings may be constructed in various ways to give a distribution of ampere-conductors per slot approximating to that of the 3-lobed curve derived from two uniformly rotating vectors, one of which revolves through double the angle traversed by the other for equal displacements along the peripher of the rotor.



(*a*) (*b*) (*c*)

Fig. 17.

Two questions at once suggest themselves. It has been pointed out above that cascade motors may be built for many other ratios of numbers of poles than 2 : 1. What curves will arise in these cases and how can we fit windings to them, and what determines the number of maxima or lobes? Let us now consider these questions, taking the second one first.

It is clear in the first place that the maxima can only occur when the two revolving vectors coincide in direction. Consider again the case where one vector is

revolving twice as fast as the other and in the opposite direction, and assume that at a certain point in the

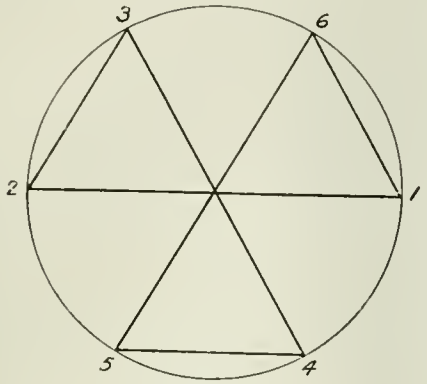


FIG. 18.

periphery the two vectors coincide in direction and the resultant curve therefore has a maximum. This is shown in Fig. 20 (*a*). Now consider Fig. 20 (*b*) where the slower, say the 2-pole, vector has moved through 120°. Since the faster vector, say the 4-pole, goes twice as fast it will have moved through 240° in the opposite direction, and will therefore have arrived at the same point. Hence we have another maximum. Similarly, when the slower vector has moved through 240° [Fig. 20 (*c*)] the faster vector will have moved through 480° (360° + 120°) in the opposite direction, and they will again coincide in direction, giving a third maximum. Finally, when the slower one has moved through 360° the faster one will have traversed 720° and we return to the position from which we started. This explains why, in the case of a 2 : 1 speed ratio, we have a 3-lobed curve.

Let us now endeavour to express this in the form of a general rule, taking first the case of vectors revolving in opposite directions. When one of the vectors has traversed an angle θ assume that the other has traversed an angle $\theta \times b/a$, where b/a is the ratio of the numbers of pairs of poles of the two fields reduced to their lowest terms by dividing by any common factor. Then, if when $\theta = 0$ the two vectors coincide, they will again coincide when

$$\theta = 360° - (\theta \times b/a)$$

i.e. when $\theta = \dfrac{a \times 360°}{a + b}$ or $\dfrac{360°}{1 + (b/a)}$

This will be called the characteristic angle.

Thus the angle between successive maxima is

$$\frac{a \times 360°}{(a + b)}$$

The number of maxima will be the smallest number by which this angle can be multiplied to make it a multiple of 360°.

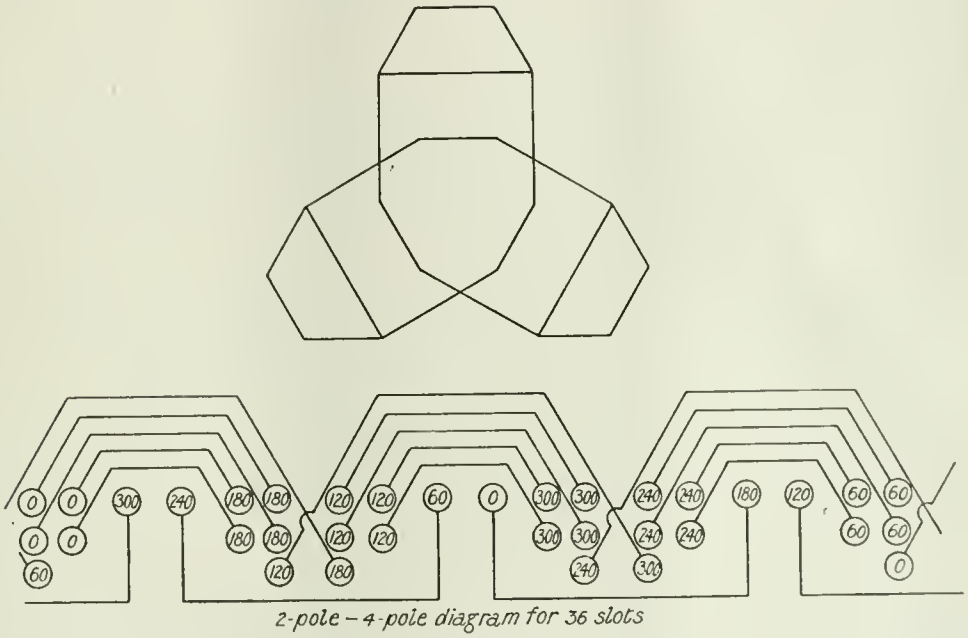


FIG. 19.

This number will be $(a + b)$, since a and b have no common factor.

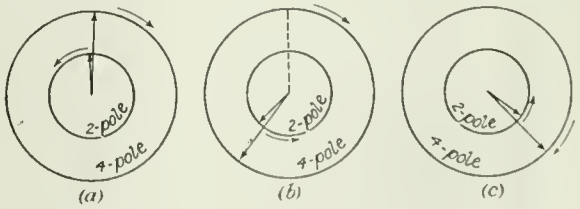


FIG. 20.

Thus our final rule is :—

The number of maxima in the curve of resultant ampere-conductors is the sum of the numbers of pairs of poles

in the two oppositely revolving component fields, after any common factor has been divided out.

A few numerical examples may make this rule clearer.

In the case we have just considered (Hunt's case) $b/a = 2/1$, $\therefore\ \theta = \dfrac{360^\circ}{(2+1)} = 120^\circ$, and there will be $(2+1) = 3$ maxima.

Take the case of 4 and 6 poles or multiples thereof, which will also be balanced as has been shown above. Here $b/a = 3/2$, $\therefore\ \theta = \dfrac{2 \times 360^\circ}{(2+3)} = 144^\circ$, and the number of maxima is $(2+3) = 5$.

Here we notice for the first time a fact that it will be necessary for us to investigate fully, viz. that curves having the same number of maxima can correspond to different pole combinations. For instance, take the case of 2 and 8 poles or multiples thereof, another balanced combination. Here

$$b/a = 4/1, \quad \therefore\ \theta = \frac{360^\circ}{(4+1)} = 72^\circ$$

and the number of maxima is again 5. Before investigating this, however, we must deal briefly with the case where both fields revolve in the same direction.

If both vectors rotate in the same direction but at different rates, starting from a position of coincidence they will again coincide when the faster has turned through the same angle as the slower plus some multiple of 360°, that is, when the difference between the two angles is a multiple of 360°.

Expressing the above statement as a formula we get

$$\theta - \frac{\theta b}{a} = 360^\circ, \ \therefore\ \theta = \frac{a \times 360^\circ}{(a-b)}$$

Take the case of 2 and 6 poles, also shown to be balanced. Here $b/a = 3/1$, $\therefore\ \theta = 360^\circ/2 = 180^\circ$, and the number of maxima for the same reason as before is $(a - b) = 2$. The negative sign refers only to direction of rotation, and may be neglected. Thus, for vectors rotating the same way our final rule is :—

The number of maxima in the curve of resultant ampere-conductors is the difference between the numbers of pairs of poles in the two similarly revolving component fields, after any common factor has been divided out.

As regards the magnitude of these maxima, it is clear that this will be equal to the arithmetical sum of the magnitudes of the two component vectors, and the minimum value to their arithmetical difference. Hence, if they are equal the minimum value will be zero.

Several instances occur in the course of the present paper where it is necessary to work out the difference of two equal vectors, E.M.F.'s or currents for instance, having a given phase difference. For two equal vectors the well-known formula for the third side of a triangle having two sides a, b, and an included angle of θ; viz. $a^2 + b^2 - 2ab\cos\theta = c^2$ reduces to $c = 2a\sin\frac{1}{2}\theta$. For instance, the E.M.F.'s in the two bars forming one turn of any coil differ in phase by an angle equal to (180° × coil pitch)/(pole pitch) = α, say, and the resultant E.M.F. per turn is $E_1 = 2E$(per bar) × sin (90° × coil pitch)/(pole pitch), the latter quantity being frequently called the chord factor.

Again, if two turns are connected in series reversed, as in the case of turns round adjacent N and S poles of a machine, then the E.M.F.'s in the two turns will differ in phase by $\gamma = 180^\circ \times$ (pitch of corresponding bars of the two turns)/(pole pitch), and the resultant E.M.F. of the two turns will be $E_2 = 2E_1 \sin\frac{1}{2}\gamma = 4E$ (per bar) $\sin\frac{1}{2}\alpha \sin\frac{1}{2}\gamma$. The same formula enables us to calculate the star currents in the windings, each of which forms the resultant of two mesh currents and is used in several places in the present paper.

Ratio of the Fluxes.

In several of the windings, notably the Hunt star-mesh winding, each phase consists of two coils joined in series and in opposition. This is clearly shown in Fig. 21, in which the three circuits in the Hunt winding are shown by different kinds of line. It will be worth while, therefore, for us to study how to determine

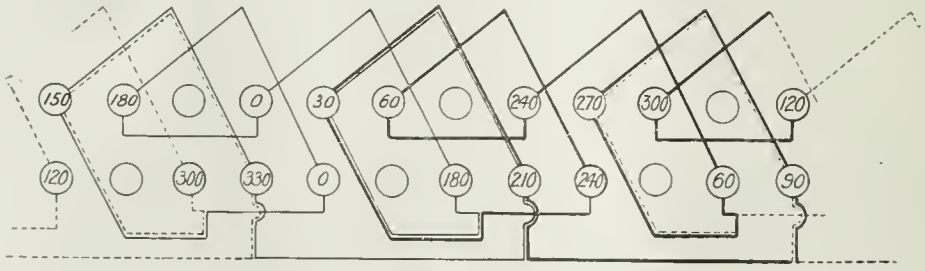


Fig. 21.

the ratio of the fluxes in such a circuit. We have just seen that the E.M.F. whatever the number of poles is

$$E_2 = 4E \text{ (per bar) } \sin\tfrac{1}{2}\alpha \sin\tfrac{1}{2}\gamma \text{ (per circuit)},$$

where $\alpha = (180^\circ \times$ coil pitch$)/($pole pitch$)$, and $\gamma = (180^\circ \times$ pitch of corresponding bars$)/($pole pitch$)$.

Corresponding bars, for instance, might be those marked 30° and 240° in the top layer of Fig. 21.

It is clear that α and γ will be different for the two distinct numbers of poles of the cascade machine, and that the sum of the E.M.F.'s due to the two fluxes must be zero. Hence, if B_1, B_2 are the maximum densities due to the two fluxes, we have

Max. E.M.F. per bar $= V_1 B_1$ for the first flux;
Max. E.M.F. per bar $= V_2 B_2$ for the second flux;

V_1 and V_2 being the speeds of the fluxes relative to the rotor. The equation determining B_1/B_2 will be

$$V_1 B_1 \sin\tfrac{1}{2}\alpha_1 \sin\tfrac{1}{2}\gamma_1 = V_2 B_2 \sin\tfrac{1}{2}\alpha_2 \sin\tfrac{1}{2}\gamma_2$$

This ignores the breadth coefficient of the coils, which we shall have to discuss directly.

In some windings, for instance that shown in Fig. 13, we have

Coil pitch = pitch of corresponding bars in the two coils.

In this case $\alpha = \gamma$, and the formula becomes

$$V_1 B_1 \sin^2 \tfrac{1}{2}\alpha_1 = V_2 B_2 \sin^2 \tfrac{1}{2}\alpha_2$$

Taking this formula first, let us apply it to certain instances :—

In the table below, the values of these quantities are calculated. Applying these to the various cases considered we get :—

Case (1).—$B_1 = 0{\cdot}895 B_2$. Hunt states on page 413 of his paper : " The flux per pole of the 2-pole field is equal to 1·73 times the 4-pole flux." This gives us $B_1 = 0{\cdot}866 B_2$, i.e. the value to which we approximate as the number of slots gets larger. Hence our results are in practical agreement with those of Hunt.

Case (2).—From the previous calculation corrected $B_1 = 0{\cdot}915 B_2$.

Case	No. of Slots	Pitch of Coil	Poles	α_1	Sin $\frac{1}{2}\alpha$	V	$(\text{Sin } \frac{1}{2}\alpha)^2$	δ	Sin $(180 - \delta 2)$
1	12	1—4	2	90°	0·707	2	0·5	30	0·965
	12	1—4	4	180°	1·0	1	1	60	0·866
2	20	1—4	4	108°	0·81	3	0·655	36	0·95
	20	1—4	6	162°	0·99	2	0·98	54	0·88
3	20	1—4	2	54°	0·455	4	0·207	18	0·99
	20	1—4	8	216°	0·96	1	0·92	72	0·81
4	16	1—4	2	67·6°	0·557	3	0·301	$22\frac{1}{2}$	0·98
	16	1—4	6	203°	0·98	1	0·96	$67\frac{1}{2}$	0·83

Case (1).—$B_1 \times 2 \times 0{\cdot}5 = B_2 \times 1 \times 1$; $B_1 = B_2$.

This is Hunt's case, for one of his windings.

Case (2).—$B_1 \times 3 \times 0{\cdot}655 = B_2 \times 2 \times 0{\cdot}98$; $B_1 = 0{\cdot}99 B_2$.
Case (3).—$B_1 \times 4 \times 0{\cdot}207 = B_2 \times 1 \times 0{\cdot}92$; $B_1 = 1{\cdot}11 B_2$.

Cases (2) and (3) are the cases discussed below, in which identically the same winding is operated on two different combinations of poles, enabling us to get 4, 8, 12 and 16 poles with a 20-pole cascade speed. The calculation just made shows that the ratio B_1/B_2 is about the same on both pole combinations.

Case (4).—$B_1 \times 0{\cdot}301 \times 3 = B_2 \times 0{\cdot}96 \times 1$; $B_1 = 1{\cdot}06 B_2$.

This is an example of the new type of magnetically balanced machine described below.

These results will be somewhat modified by the influence of " breadth factor " when the coils in series extend over several slots instead of one only. We assumed above E (per bar) $= VB$. In the Hunt winding, for instance (Fig. 13), it will be noted that each section between the star point and the point where the winding reverses, i.e. the point where the coil joined to a collector ring is tapped in, consists of two turns in neighbouring slots spaced apart therefore by one-twelfth of the circumference, or 30° of phase on 2 poles and 60° on 4 poles. The E.M.F.'s in it will therefore differ in phase by these amounts. Calling these angles of angular spacing δ_1 and δ_2 our final formula becomes

$$V_1 B_1 \sin \tfrac{1}{2}\alpha_1 \sin \tfrac{1}{2}(180 - \delta_1) = V_2 B_2 \sin \tfrac{1}{2}(\alpha_2) \sin \tfrac{1}{2}(180 - \delta_2)$$

Case (3).—From the previous calculation corrected $B_1 = 0{\cdot}915 B_2$.

The result of this correction is to show that the ratio of the densities is identical in the cases where the same winding is operated on two distinct combinations of poles.

Case (4).—This gives $B_1 = 0{\cdot}9 B_2$.

It is perhaps desirable to point out that the ratio of the diameters of the two circles on which the ideal curve of ampere-conductors is based is merely a measure of the ratio of ampere-conductors per slot in two component windings, and *not* of the fluxes or magnetic densities. These calculations, in fact, will be sufficient to show clearly the principle on which the determination of the relative densities must be based. Every winding will consist of a number of exactly similar closed circuits. Calculate the E.M.F. in one such circuit due to the flux of one number of poles. Then calculate it as due to the flux of the other, and equate the two. This will give an equation determining the relative densities or fluxes per pole.

SECTION III.

Construction of the Secondary Windings.

One of the most important features of the cascade motor is the possibility of changing from cascade speed to any of the basic speeds, and several improved methods of carrying this out may be explained. The two salient characteristics of the cascade rotor are :—

(1) Both the fields on which it operates rotate with respect to it at speeds much greater than that of slip.

(1) The ampere-conductors per slot are different at different points of the circumference.

Since in practice it is not convenient to make the slots of different sizes, they must be of a size to accommodate the maximum ampere-conductors required, and this involves some of them being only partially filled. It is well known that, if a cascade set of two machines be brought up to the synchronous speed of the primary machine, it will continue to operate and carry load at that speed, the second machine, which now receives only slip frequency, merely acting as an impedance, with some self-induction, in the secondary of the first. In the case of the internal cascade motor, in which the currents corresponding to the second motor circulate in the windings of the first, and this second motor has no actual independent existence, this possibility receives its full development because at slip frequency the secondary E.M.F. is chiefly consumed by resistance. There is, therefore, little occasion for any secondary flux to be generated, and consequently the secondary machine does not act appreciably as an impedance in the secondary of the primary machine. Of course, since all the slots are not filled, such a winding will not give the same torque as one of normal type. The problem, therefore, is to cause the motor to change from cascade speed to one of the basic speeds, and it will not do this unless some further means are adopted. What we require, in order to do this, is some means of stopping the rotation of the fluxes with respect to the rotor. One effective means of doing this is to apply another winding quite independent of the cascade winding which, when it is short-circuited through slip-rings, will stop the rotations of the fluxes with respect to the secondary member. Winding (4) in Appendix A illustrates this method. Another method is to introduce further connections into the existing cascade winding, thereby producing further closed circuits which will no longer be equal in number to the sum of the numbers of pairs of poles of both fields. Winding (5) in Appendix A is an example of this.

In certain cases, as for instance in Hunt's star-mesh winding, good results may be obtained by what is in effect a combination of both these methods, while in others it is necessary to use them separately. One convenient way of combining the two methods which is sometimes applicable, is to close each phase of the cascade winding through a phase of the auxiliary winding, instead of short-circuiting it. We have already described the Hunt star-mesh (Fig. 21). To apply to this winding the two distinct methods described above, in this manner, we should connect the upper bars in slots 3, 7 and 11 to the junction points of sections 2 and 4, 6 and 8, 10 and 12, and short-circuit the lower bars of these three sections, thereby bringing about the closing of the required circuits with only three slip-rings. Let us consider the nature of the winding so produced (Fig. 22), and similarly for any increased number of phases.

The diagram showing the circuits in skeleton form (Fig. 22) indicates that there are 6 circuits, each of the star members of the winding being common to two of these circuits. With 4 poles the mesh-connected or even-numbered coils will differ in phase by 120°, and with 2 poles by 60°. The current in the star-connected coils, being the resultant of that in the two mesh-connected coils between which it lies, will in the case of 4 poles be equal to that in the mesh-connected coils, and in the case of 2 poles will be 1·732 times as great. Fig. 22 shows a 12-slot winding fully drawn out similar to Hunt's Fig. 7, one complete circuit of the six which exist when the slip-rings are short-circuited being shown in heavy lines. In Fig. 21 for the sake of comparison the same winding is shown as operating on cascade, the three circuits of which it is essentially composed being shown by three different types of line. It will be seen that every star coil is common to two circuits.

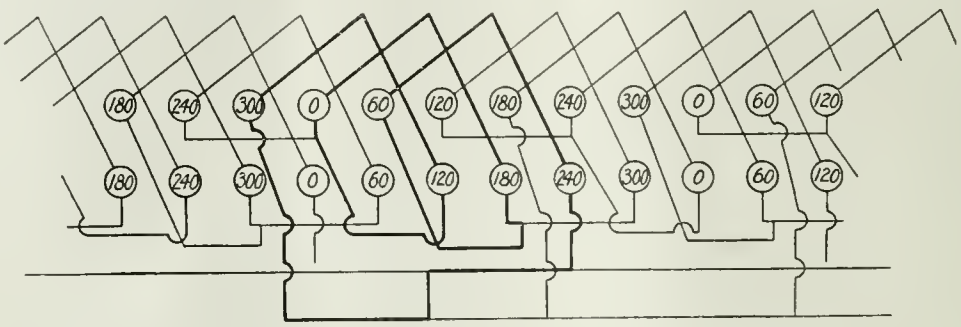


Fig. 22.

We have hitherto dealt chiefly with windings having three phases, largely in order to enable us to compare our results with those of Hunt. We shall now describe a number of windings adapted for, say, five phases, both for the sake of variety and because windings having a higher number of phases lead to a very important generalization already alluded to above, whereby we are enabled to obtain a greater number of speeds in the cascade motor. In connection with these five-phase windings the most appropriate method of obtaining the basic numbers of poles will be described. Diagrams will also be given of the vector polygon for each winding, showing how nearly this polygon approximates to the ideal curve.

We have seen that any winding having the appropriate number of phases is compatible with the existence

of two fluxes of the numbers of poles given by the rule above stated, and that one of the chief characteristics in such a winding must be the variation of the ampere-conductors per slot from point to point round the circumference, the number of maxima and minima

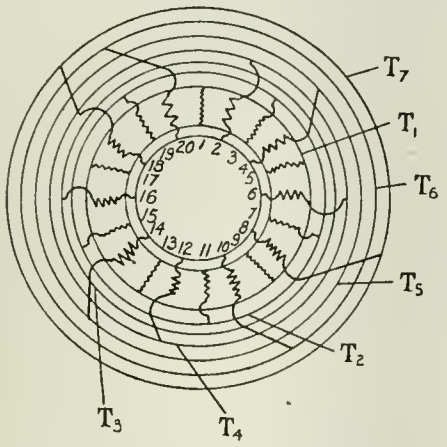


Fig. 23.

being equal to the number of phases. We have already seen that the number of phases required is equal to the sum of the numbers of pairs of poles in the two fields. A single-phase winding for $(m + n)$ pairs of poles is a winding having the requisite characteristics, i.e. consisting of $(m + n)$ sections capable of being the seat of a balanced system of $(m + n)$-phase currents and also having $(m + n)$ maxima and minima. Every phase will be repeated twice to comply with Hunt's rule for magnetic balance. Such a single-phase winding is shown in Fig. 17 (*c*).

Clearly, the phase difference between adjacent sections on $(m + n)$ pairs of poles will be $\frac{360}{2(m + n)} \times (m + n) = 180°$ (since when repeated twice there will be $2(m + n)$ sections), which proves the winding to be single-phase. The phases of the cascade winding may be connected in many ways, either in star or mesh, or short-circuited independently on themselves. A convenient standard arrangement which may be used for many windings is connected in star, and if alternate terminals are joined to each single-phase terminal (as T_1 and T_2, Fig 23) they form a parallel type single-phase winding with $(m + n)$ local circuits, in which the single-phase currents will flow from each terminal through the star to the next. This is shown by the odd-numbered coils of Fig. 23 as connected to rings 1 and 2. Such a single-phase winding forms a convenient starting-point in developing various types of cascade winding. We shall illustrate these for the case of 4 and 6 pairs of poles throughout, but it will be clear that the same principles are applicable to any other combination of poles modified for cascade work.

It will now be desirable to describe a number of windings adapted for cascade working, and also how they may be modified to enable them to operate on any of the basic number of poles.

Winding (1).—Of the various types of winding described in the present paper the following is one of the most interesting and important, and we shall therefore devote considerable space to it. It is shown in Fig. 24 (*a*) and described in detail below. The distribution of ampere-conductors produced by it is superposed on the ideal curve of Fig. 25 [(*a*) and (*b*) , showing that the approximation is quite as close as in any of the machines as described and built by Hunt. In this connection, as a

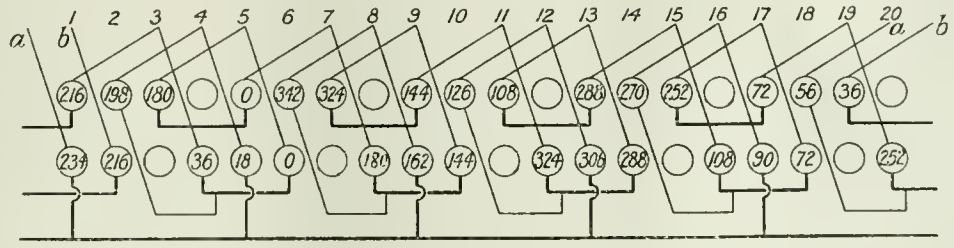


Fig. 24 (*a*).

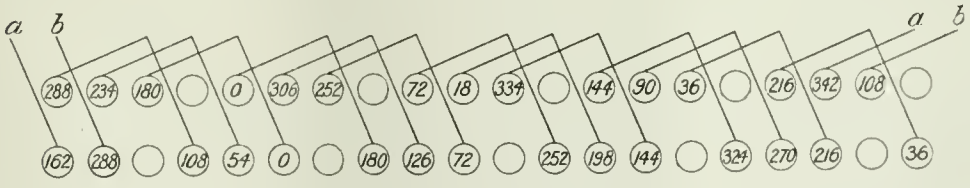


Fig. 24 (*b*).

Fig. 24 (*c*).

preliminary step, a somewhat clearer and simpler form of connection diagram is desirable. Hunt usually denotes a single section (which may in practice consist of one or several coils in series) in a 2-layer drum winding by two small circles, one in the top and one in the bottom layers, joined by a curved line. It serves considerably to clarify the diagram, in the author's opinion, if we merely join these circles by a straight line as short as convenient, and distinguish the lower layer by using a blackened instead of a plain circle (see Fig. 26). The symbols formed in this way may be numbered successively round the circumference in order to show their relative position in the winding, and interconnected to form a key diagram (see Fig. 27), of which (*a*) is identical with Fig. 21, and (*b*) with Fig. 24 (*a*).

Consider any point where three sections are joined together, say 12, 1 and 2 (Fig. 27). Suppose in the first

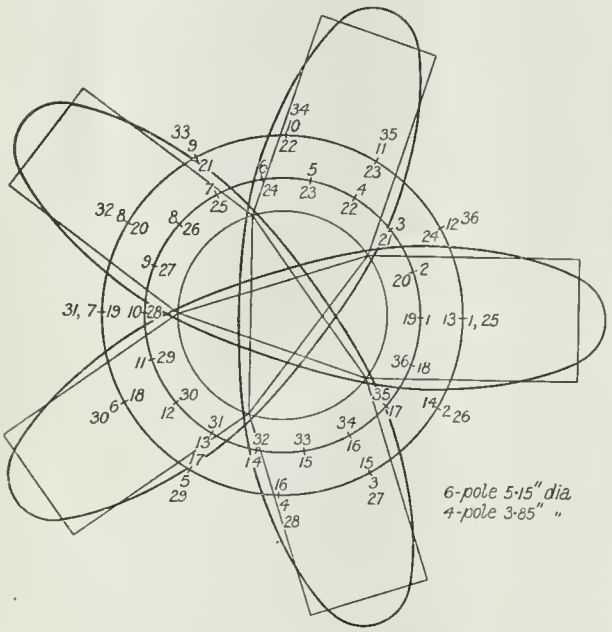


Fig. 25 (*a*).

place that the same currents flow in the same directions in 12 and 2. Then, from the connection of the circuits, it is clear that their arithmetical sum will flow in section 1. If, however, these currents are displaced in phase by an angle θ, then their resultant or vector sum will flow in section 1. The resultant (not difference) of two unit currents making an angle θ is $2 \sin \frac{1}{2}(180 - \theta)$. Calling the currents in 12 and 2 the mesh currents and that in 1 the star current we have

$$\text{star current} = 2 \sin \tfrac{1}{2}(180 - \theta) \times \text{mesh current}$$

Applying this to the case in which sections 12 and 2 are 120° out of phase, we get the familiar result

$$\text{star current} = 2 \sin 60° \times \text{mesh current}$$

The winding shown in Figs. 27 (*b*) or 24 (*a*) is made up of units consisting of two coils in series reversed, 10 units of this character being connected in mesh instead of in star. Numbering the turns in the different slots from 1 to 20, we note that all the even-numbered coils are connected in mesh, alternate ones being mutually reversed, thus the turn lying in slots 1 and 4 is connected in series with that lying in slots 3 and 6 by a connection between the lower conductors of slots 4 and 6, which has the effect of causing mutual reversal of the two turns. The turn in slots 3 and 6 is again connected in series with that in slots 5 and 8 by a connection between the upper conductors of slots 3 and 5, and so on. In this way all the even-numbered coils are connected. Of the odd-numbered coils, only those whose number, deducting 1, is divisible by four are connected to the two even-numbered coils between each of which such coils lie. The upper bar of such odd-numbered coils is connected to the junction of the two lower bars of the two coils between which it lies.

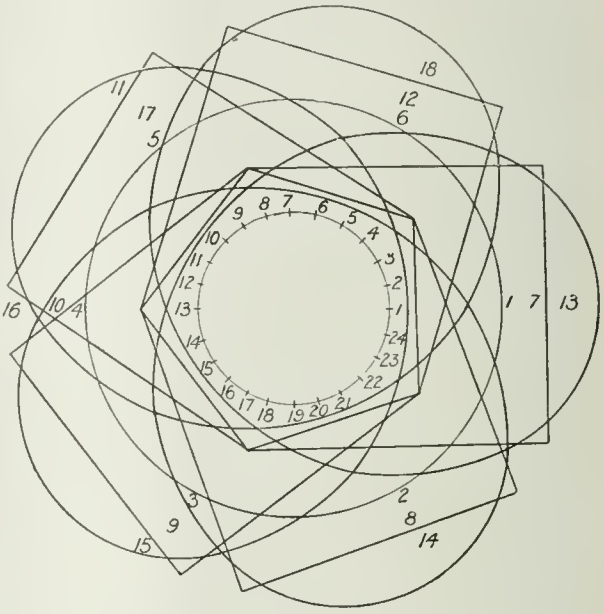


Fig. 25 (*b*).

The winding of Fig. 24 (*a*) as arranged to fill 20 slots, contains five closed circuits as shown in Fig. 27 (*b*). For instance, the five circuits contain sections 1, 2, 4, 5; 5, 6, 8, 9; 9, 10, 12, 13; 13, 14, 16, 17; 17, 18, 20, 1. These five circuits are evidently distributed evenly round the circumference, and each carries one phase of a balanced five-phase system of currents. A five-phase system is capable of two orders among its phases, viz. 0°, 72°, 144°, 216°, 288°, and 0°, 144°, 288°, 72°, 216°, and these two orders give rise to two distinct cases, both of which we must study. Five of the sections—the star sections—are each common to two of the closed circuits, and if we consider that the positive direction of circulation of all the currents round the closed circuits is clockwise, the current of one section will be flowing inward when that of the next (say 72° displaced) is flowing outward. The difference between a vector at 0° and another equal to it at 72° will be 2 sin 36°

= 1·17. If the vectors had been at an angle of 144° we should have had 2 sin 72° = 1·9; or if they had been at 120°, 2 sin 60° = 1·732. This shows that if the phase difference between adjacent circuits is 72° (the 2- and 8-pole cases) the star current will be 1·17 times the mesh current, while if it be 144° (the 4- and 6-pole cases) it will be 1·9 times the mesh current. In Hunt's case the ratio is 1·732.

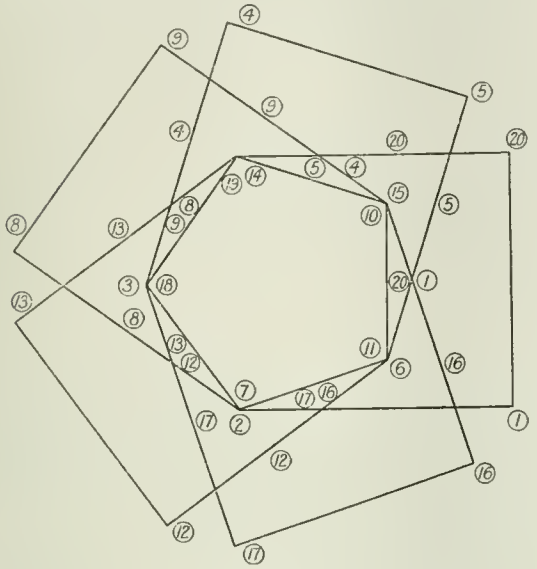


Fig. 25 (*c*).

Drawing out the 20-slot winding for each of the two cases as in Fig. 24 (*b*) choose five equally spaced bars, say the top bars in slots 5, 9, 13, 17, 1, and mark them with angles, say 0°, 72°, 144°, 216°, 288°. The connections of the winding now enable us to mark the angles corresponding to all the other bars. For instance, the top bar in slot 3 and the bottom bar in slot 8 must be opposite in phase to the top bar in slot 5, and simi-

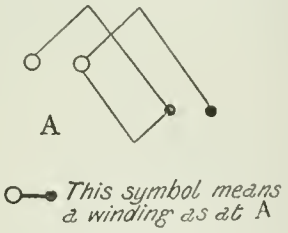


Fig. 26.

larly for other bars bearing the same relation to the remaining bars already marked. The bottom bar in slot 6 must also be opposite in phase to the top bar in slot 3, and similarly for corresponding bars. We have now marked all but the star bars. From the way these are connected, it is clear that the bottom bar in slot 9 is intermediate in phase between the bottom bars in slots 8 and 10, since it carries the sum of two equal currents flowing in these two bars. We accordingly mark it 126° = ½(72° + 180°) and similarly for corresponding bars. The top bar in slot 2 must be opposite in phase to this, and so on similarly.

In Fig. 24 (*a*) an angle of 144° has been substituted for 72°, the marking being otherwise carried out in the same way.

Having prepared the marked diagrams, we next draw two circles of relative radii corresponding to the currents in the mesh and star bars respectively, calculated as above from the phases of the sections, and divide both into as many parts as may be necessary to give us all the angles we require (20 in this case). Take the "mesh" circle (the inner one as a rule); at the point corresponding to the angle with which a bar is marked insert the slot number of that bar. For instance, if a bar in slot 1 is marked 0° we mark the number 1 opposite the point on the circle we have chosen to represent 0°. Having marked all the slot numbers carrying mesh bars in this way, we do the same on the other circle for the star bars. We then draw the vector sum of all vectors drawn from the centre to either of the circles and marked with the same slot number, and mark its extremity with that slot number, which is the resultant value of the ampere-conductors per slot. In this way we construct one point corresponding to each slot. Joining these points in order, we get Figs. 25 (*a*) and (*b*), the former representing the order 0°, 144°, 288°, and the latter the order 0°, 72°, 144°.

We have described in detail how the ideal curve is drawn; therefore it is only necessary now to point out how the radii of the oppositely revolving vectors of this curve may be obtained from the polygonal diagram corresponding to a finite number of slots. To do this draw circles tangential to the polygon both internally and externally. The maximum of the nearest ideal curve will be slightly greater than the radius of the outer circle, and the minimum slightly less than that of the inner —how much, can only be estimated. We have already seen that the maximum value of the ideal curve was equal to the arithmetical sum of the oppositely revolving vectors, and its minimum to their arithmetical difference, so they can be calculated from this data. It may be worth mentioning also that the side of the polygon between two angles marked with two neighbouring slot numbers represents in magnitude and phase the magnetomotive force acting in the tooth between the two given slots. As will be seen, the approximation on 2 and 8 pairs of poles [Fig. 25 (*b*)] is quite as close as on 4 and 6 pairs of poles [Fig. 25 (*a*)], which proves from another standpoint that the same winding can operate with both pairs of fields.

In Fig. 24 (*c*) the phases of the currents in all conductors with a winding short-circuited in a suitable manner such as that described above, operating on 8 poles, are shown. These phases are arrived at by attributing to conductors spaced apart by one-tenth of the circumference phases differing by 144°, when the connections of the winding enable us to determine the phases of all the other conductors. Averaging the phase of the two conductors in each slot, we find that they differ uniformly by 108°, and hence the winding is quite satisfactory as an 8-pole winding.

Winding (2).—In order to complete the description of the secondary windings it will now be desirable to

describe a winding adapted for the novel case of 2 and 6 poles, which depends on the new rule worked out above as regards magnetic balance.

Working out the number of phases corresponding to this case from the rule given above, we find that the winding must be adapted for four phases. It has been pointed out that any of the windings described above may be adapted for any other number of phases, and hence the five-phase windings previously described may be reduced to four-phase windings by omitting

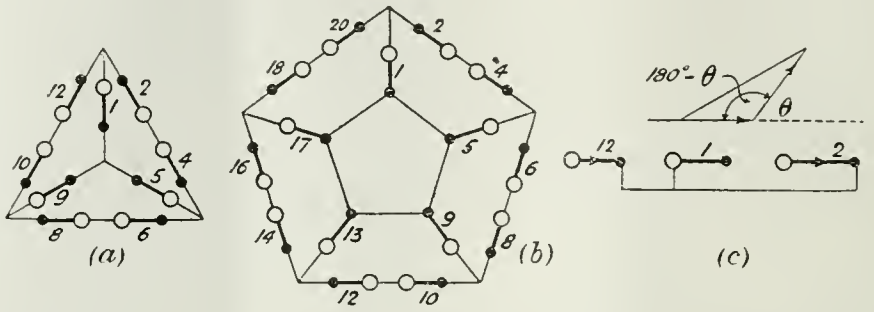


Fig. 27.

one-fifth of their conductors. For instance, windings described for five phases in 40 slots may also be arranged for four phases in 32 slots, the pitch of the coils, etc., remaining the same. Just as in the winding for 40 slots each of the five phases was repeated twice and the winding was therefore adapted for 4 and 6 pairs of poles, so in a 32-slot winding each of the four phases

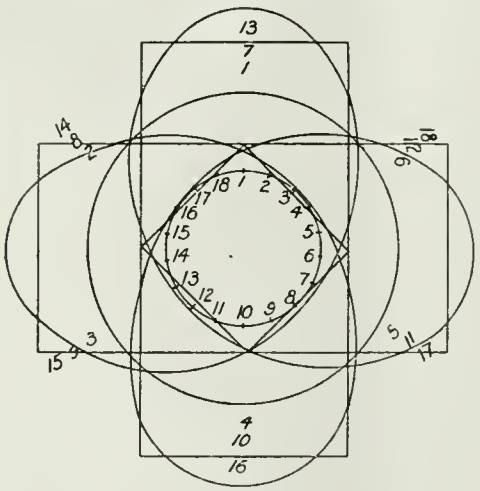


Fig. 28.

would be repeated twice and the winding would be adapted for 2 and 6 pairs of poles. But in the cases we are now studying this is not necessary, as a winding for 1 and 3 pairs of poles is magnetically balanced, unlike a winding for 2 and 3 pairs of poles. Hence one-half of the 32-slot winding can be omitted, and an adequate four-phase winding could be wound for 16 slots only.

On working out the diagram of ampere-conductors for this winding it will be found quite as close to the ideal curve as that of any of the other windings. It is shown superposed on the ideal curve in Fig. 28.

SECTION IV.

Stator Windings.

It is necessary now for the sake of completeness to describe stator windings corresponding to, and capable of use with, the rotor windings already described. In the case of a machine having a single pair of fields, that is, having only two distinct numbers of poles, it is always possible to use two distinct windings.

It may be shown without difficulty that any number of sections equally spaced round the circumference of a machine will have induced in them a balanced system of polyphase E.M.F.'s on any number of pairs of poles, which is not a multiple of the number of sections. Hence, if all these sections are connected in series, the sum of the E.M.F.'s will be zero. Thus, to obtain two mutually non-inductive windings for different numbers of poles, all that is necessary in general is to arrange them, say, in star, devoid of parallel circuits, and then, with a few exceptions practically confined to cases where one number of poles is a direct multiple of the other, they will be quite mutually non-inductive.

The advantage of a single winding is of course very

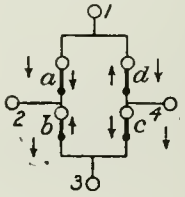


Fig. 29.

great, and it is desirable therefore to describe how such a single winding may be used in several of the cases described above. In particular, where it is desired to use two or more pairs of basic numbers of poles requiring primary windings having three or more speeds, it would often be very inconvenient to use distinct windings for all these numbers of poles.

Let us confine ourselves firstly to the Hunt type of magnetically balanced machine in which the primary and

secondary numbers of pairs of poles when divided by their greatest common factor give in one case an even, and in the other an odd, number as quotient. Consider four sections arranged equidistantly round the circumference, which we may refer to as A, B, C, and D (see Fig. 29), and consider the phase differences between them on different numbers of poles. These are shown in the table below.

No. of Poles	Section			
	A	B	C	D
4	0	180	0	180
8	0	0	0	0
12	0	180	0	180
16	0	0	0	0

We have here chosen the numbers of poles corresponding to the rotor winding having five phases, which has been fully described above, as by describing this winding

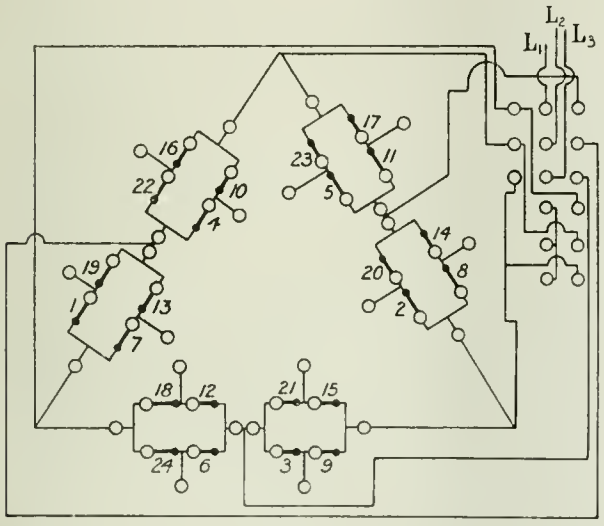


Fig. 30 (*a*).

we complete the description of the cascade machine having these characteristics. Connect the four sections A, B, C, D, as follows :—

A in series with B and D in series with C. The pairs AB and DC are now connected in parallel to form a closed circuit, in which the beginning of section A is connected to the beginning of section D, and the end of section B to the end of section C. Four terminals are now brought out from the junction points of each pair of sections. Such a closed circuit we may call a Hunt unit, and it has the following properties, which may be verified by reference to the table immediately above. With 8 and 16 poles, sections A and B are in phase and likewise sections C and D, hence the E.M.F.'s in A and B are added arithmetically and their sums are exactly equal to the sum of those in C and D. With 4 and 12 poles A and B will be opposite in phase and likewise C and D, and consequently the E.M.F.'s round the closed circuit will be zero. Let us number the terminals as follows :—

Terminal 1 between Sections A and D.
Terminal 2 between Sections A and B.
Terminal 3 between Sections B and C.
Terminal 4 between Sections C and D.

If now we connect terminals 1 and 3 in series with the line, and a resistance across terminals C and D, the primary currents for 8 and 16 poles will flow between 1 and 3, and the secondary currents between 2 and 4. If we wish to make 4 and 12 the numbers of primary poles, we must cause the primary currents to flow between terminals 2 and 4. Tracing the circuits between 2 and 4, we note that B and C form one branch and A and D the other, these being connected in series and opposition. It will be seen from the table that sections B and C are opposite in phase on 4 and 12 poles, and hence their E.M.F.'s will be added if these two sections are connected in opposition. For the same reason as described previously, secondary currents capable of producing an 8- and 16-pole field will not circulate round the closed circuit, but can flow only between terminals 1 and 3.

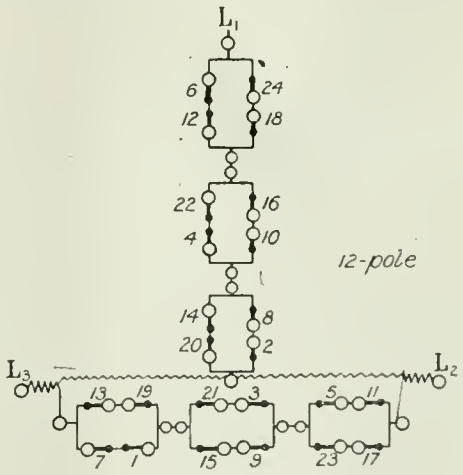


Fig. 30 (*b*).

Hence, if we can build up a winding adapted for, say, 8, 12 and 16 poles (neglecting the case of 4 poles which will be seldom required) out of these units we shall have a winding which will answer the required purpose. To do this we require a winding containing at least 24 sections, giving rise to six Hunt units (see Fig. 30). In such a winding, between sections A and B of any given unit, that is, any two neighbouring sections, there will be sections belonging to five other units, making six in all. We may call these units one to six, numbering them in order from section A of unit 1 to section B of the same unit. Unit 1 is thus the unit containing section 1.

To connect these six units for 16 poles we may connect

units 1 and 4, 2 and 5, 3 and 6 in series and these three sets in mesh. To obtain 8 poles connect units 1 and 4, 2 and 5, 3 and 6 in parallel and connect these pairs in star. To obtain 12 poles connect units 1, 3, and 5 in series, and 2, 4 and 6 in series. Connect units 2, 4 and 6 between one line and the central point of an auto-transformer connected across the other two lines. Connect units 1, 3 and 5 across tappings on the same

FIG. 31.

FIG. 32.

auto-transformer adapted to give a voltage 0·866 times the line voltage. Care must of course be taken, as has been done in Fig. 29, to see that the North and South poles of each phase on all numbers of poles are in correct sequence.

We now come to the new type of magnetically balanced machine in which both the numbers of pairs of poles are odd, and we shall describe the case of 2

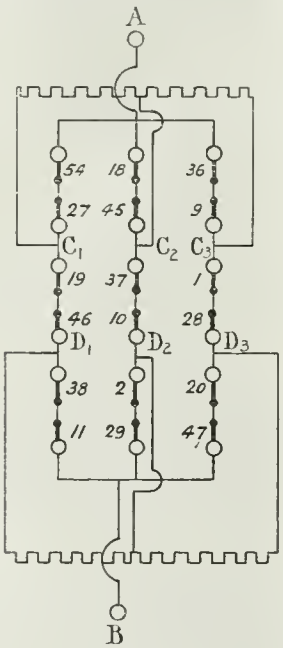


FIG. 33.

and 6 poles, as this is likely to prove one of the most important practically. In the case of 2 and 6 poles, opposite sections will be opposite in phase on both the primary and secondary numbers of poles (we here assume that the primary number of poles is 6), and therefore the Hunt winding is inapplicable, but it is possible to make the sum of the secondary E.M.F.'s equal to zero round the local circuits by methods other than those used by Hunt.

To form one branch of the circuit, let us take three instead of two equally spaced sections in series. On 6 poles all these three sections will be in phase, and the three E.M.F.'s will consequently be added arithmetically (see Fig. 31). On 2 poles they will have phases 0°, 120°, and 240°, and the sum of their E.M.F.'s will therefore be zero, as shown in Fig. 32.

Circuits therefore made up of any number of branches each consisting of three equally spaced sections will have zero E.M.F. round them on 2 poles, and can be used as branches of a primary circuit on 6 poles.

In order to obtain a winding which can act satisfactorily as a secondary on 2 poles, it is necessary to have three such circuits in parallel, and this may at first sight give rise to difficulty.

A satisfactory winding may, however, be constructed as follows, 54 sections, each of which may conveniently occupy one slot, being required. In the table below are shown the phases of these 54 sections both on 2 poles and on 6 poles, each phase of the 6-pole winding being constructed from 18 of these sections, 6 sections in each branch of a three-parallel winding. Each of these 6

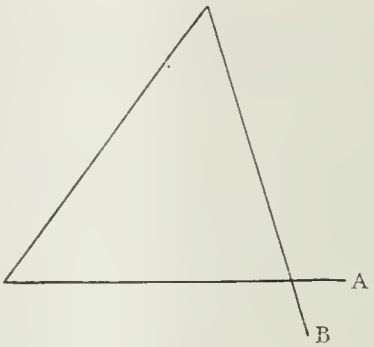


FIG. 34.

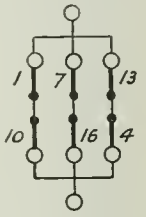


FIG. 35.

sections forms a complete 6-pole winding in itself, being connected in the order shown in Fig. 33, in which is also shown the primary terminals A and B, and the secondary terminals C_1, C_2, C_3, and D_1, D_2, D_3. Between the junction point A, and C_1, C_2, C_3, each branch consists of two diametrically opposite sections in series connected in opposition so that their voltages add on both the 2-pole and 6-pole circuits. The three branches are exactly equally spaced round the circumference, so that C_1, C_2, C_3 are precisely equipotential on 6 poles. On 2 poles, however, the three branches form a balanced three-phase circuit, which fulfils the condition that the sum of the currents measured towards a junction point such as A shall be zero. The sections between C_1, C_2, C_3 and D_1, D_2, D_3 and between D_1, D_2, D_3 and B are arranged among themselves in a precisely similar way.

Now consider the sections in any one branch; sections 54 and 27 reversed give phase 0° on 2 poles; sections 19 and 46 reversed give nearly phase 120°, in fact $126\frac{2}{3}°$; sections 38 and 11 reversed give nearly 240°, in fact $253\frac{2}{3}°$. By the use of overlapping sections, several sections being wound into one slot, these slight variations in phase angle can be avoided. But, as will be seen directly, this complication would not be justified.

If we take the sum of three equal vectors having the exact phase angles mentioned above (see Fig. 34), it will not be precisely zero, but will nevertheless be only about 6 per cent of the E.M.F. across any branch, say from A to B.

Consider how a cascade motor having this primary winding will operate when C_1, C_2, C_3, D_1, D_2, D_3 are short-circuited. Since they are exactly equipotential points with respect to the 6-pole E.M.F.'s, no effect will be produced on the 6-pole primary currents. Each of the three local secondary circuits is exactly balanced within itself, and no currents from the one need be closed through any of the others. Hence, both as a primary and as a secondary winding it is quite as effective as any of the windings described by Hunt. During the starting period only, when C_1, C_2, C_3, D_1, D_2, D_3 are open, there will be a residual three-phase E.M.F. of about 6 per cent the magnitude of that in the circuit between A and B, thus causing an induced current to flow round the closed circuit between A and B, having a resistance three times that of the local circuits between A and C_1, C_2, C_3. This residual current, therefore, will be fully effective in producing starting torque. When we commence to close the rheostats further, a much larger current will begin to flow through the circuit so established, and a powerful starting torque is produced, until when they are completely short-circuited the motor operates as above. The effect of this residual current is not serious, and may be further diminished by connecting only corresponding branches together, forming thereby three star points instead of one, and connecting an additional rheostat in circuit with them.

Were it not for the desirability of giving resistance starting in many cases, a much simpler type of cascade winding for a 2- and 6-pole motor might be employed making use of only 18 sections, i.e. 6 per phase, each containing three parallels, and each branch of the parallels consisting of two diametrically opposite sections connected in series and opposition. With 2 poles the three parallels in any one phase form a balanced three-phase circuit short-circuited on itself, while with 6 poles it is one phase of a normal three-phase winding. Its precise arrangement is shown in Fig. 35. For many two-speed motors, which do not require heavy starting torque, this winding may be preferable to the former one.

Alternate sections in series are relatively reversed as shown in Fig. 40, so that, although sections 54 and 27, for instance, are shown in the table as having phases 0° and 180°, the E.M.F.'s in these sections are arithmetically added owing to the manner of connection.

Section	Phase		Section	Phase		Section	Phase	
	Two Poles	Six Poles		Two Poles	Six Poles		Two Poles	Six Poles
Terminal A			A			A		
54	0°	0°	18	120°	0°	36	240°	0°
27	180°	180°	45	300°	180°	9	60°	180°
Terminal C_1			C_2			C_3		
19	$126\frac{2}{3}°$	20°	37	$246\frac{2}{3}°$	20°	1	$6\frac{2}{3}°$	20°
46	$306\frac{2}{3}°$	200°	10	$66\frac{2}{3}°$	200°	28	$186\frac{2}{3}°$	200°
Terminal D_1			D_2			D_3		
38	$253\frac{1}{3}°$	40°	2	$13\frac{1}{3}°$	40°	20	$133\frac{1}{3}°$	40°
11	$73\frac{1}{3}°$	220°	29	$193\frac{1}{3}°$	220°	47	$313\frac{1}{3}°$	220°
Terminal B			B			B		

APPENDIX A.

In the following appendix is given a description of a number of windings built in accordance with the above described principles, which may be found useful for a variety of purposes.

Winding (3).—Take a winding having 60 slots containing numbers of conductors varying from 1 to 3, having the phase shown in the annexed table, and all carrying equal currents. Only one-half of this winding need be considered (slots 1 to 30) since the remainder merely repeats it for the sake of magnetic balance. Connecting in series all conductors having opposite phases, such as 0° and 180°, it will be found that this winding is in the main a single-phase concentric winding arranged for five pairs of poles (see Fig. 36) (the sum

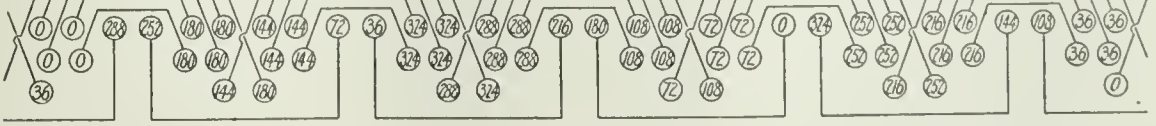


Fig. 36.

of two and three pairs of poles) all the poles of which may be short-circuited on themselves (although diametrically opposite coils may be placed in series) so that they may carry currents in the different phases required by the given combination of numbers of poles. If the resultant ampere-conductors of this winding be set out as a vector diagram (Fig. 37) it will be seen that a five-lobed curve is generated, which is very close indeed to the ideal curve already drawn [Fig. 25 (*a*)]. All these phases may be independently short-circuited or preferably connected in star, alternate ones being connected to each single-phase terminal, as described above in connection with

PHASE OF CONDUCTORS.

Slot		Top	Middle	Bottom
1	31	0	0	36
2	32	0	0	—
3	33	288	—	—
4	34	252	—	—
5	35	180	180	—
6	36	180	180	144
7	37	144	144	180
8	38	144	144	180
9	39	72	—	—
10	40	336	—	—
11	41	324	324	—
12	42	324	324	288
13	43	288	288	324
14	44	288	288	—
15	45	216	—	—
16	46	180	—	—
17	47	108	108	—
18	48	108	108	72
19	49	72	72	108
20	50	72	72	—
21	51	0	—	—
22	52	324	—	—
23	53	252	252	—
24	54	252	252	216
25	55	216	216	252
26	56	216	216	—
27	57	144	—	—
28	58	108	—	—
29	59	36	36	—
30	60	36	0	—

Fig. 23, when they will form a 10-pole single-phase winding of the type mentioned above. The two single-phase terminals will be short-circuited when the winding is being used as a cascade, and, as mentioned above, the diagram only shows one-half of it occupying 30 slots, since the other half is exactly similar. It will be desirable, before terminating the description of this winding, to explain how it can be adapted to act on either of the basic numbers of poles. It is clear from the table that there is room in the slots for another almost exactly similar winding which, since the original winding is practically a single-phase concentric winding for 10 poles, makes altogether a two-phase 10-pole concentric winding. Such a winding has 20 sections, each of which is short-circuited on itself. Now the phase difference between 20 equally spaced sections on 12 poles, say, will be $(360 \times 6)/20 = 108°$, and currents having this phase difference will circulate in adjacent sections if these are independently short-circuited.

In many types of standard polyphase rotor the phase difference between adjacent sections is 120°, and hence a phase difference of 108° is clearly not too great. The same argument applies even more strongly to the case of 8 poles. Clearly the 10 parallels of the second phase winding cannot be permanently joined up, as they would prevent the operation of the machine on cascade speed. Diametrically opposite coils of the second star-connected winding may, however, be permanently connected in parallel, for instance terminals T_3 to T_7 (Fig. 23) where the even-numbered coils represent the extra winding, and five slip-rings are used to make the other

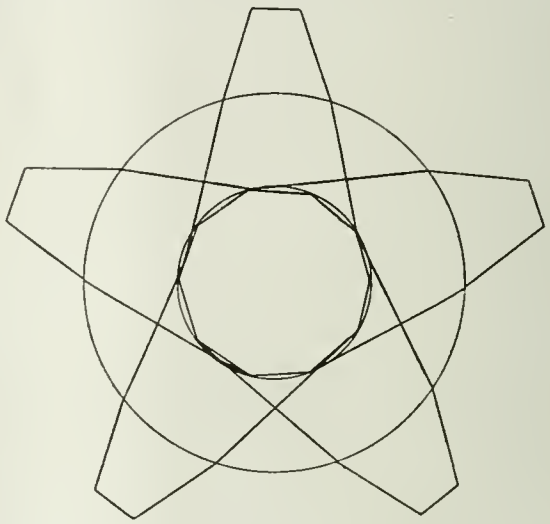


FIG. 37.

parallel connections when required. Fig. 37 shows the vector polygon corresponding to this winding. It will be seen that it is remarkably close to the ideal curve shown for the case of five phases in Fig. 25 (*a*). This winding is clearly a generalization to five phases of the winding shown in Fig. 19.

Winding (4).—Another convenient winding for the same purpose is a two-coil-per-slot drum winding having 40 slots (see Fig. 38). A winding having a pitch one to four is wound in these slots, two full and two blank coming alternately, if we consider the upper conductors only. The winding is divided into 10 units, each unit being made up as follows. The turn consisting of the upper conductor of slot 1 and the lower of slot 4 is connected in series reversed with the turn consisting of the upper conductor of slot 4 and the lower of slot 7 by a connection between the lower conductors of slots 4 and 7. Ten circuits similar to this are formed, having their initial conductors in slots 1, 5, 9, 13, 17, 21, 25, 29, 33 and 37. These 10 circuits are connected in star, for instance, alternate ones being joined to the two single-phase terminals, T_1 and T_2

in the diagram, these being short-circuited when the winding is used on cascade. To enable this winding to operate on its basic numbers of poles of 8 and 12, the first method described above may be used, viz. that of a second winding in the empty slots, in order to prevent rotation of the two fields with respect to the rotor. This can clearly be done, the winding being joined in star similar to the cascade winding, diametrically opposite coils being connected permanently together and five slip-rings being used to short-circuit the five circuits thereby obtained, as in Fig. 23.

the top conductor in slot 1 has phase 0°, the lower conductor in slot 4 must have phase 180°, the lower conductor in slot 7 must have phase 0°, while the upper conductor in slot 4 must also have phase 180°. As regards the extra winding, the phases of this will be intermediate between those of the cascade winding. Its initial conductors will lie in slots 3, 7, 11, etc., and these should therefore be marked with phase 72°, 252°, and so on, when the same process as before enables us to get the phases of the other conductors. Having done this we may mark below each slot, which of course

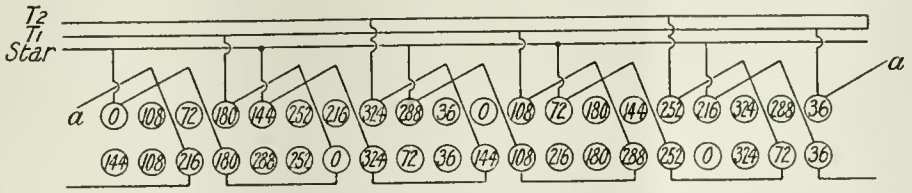


FIG. 38.

Fig. 38 shows this winding, the circles denoting the individual conductors, the straight lines the connections between them, and the figures inscribed in the circles the phases of the currents in each conductor. The conductors joined up by the connections shown represent the cascade winding, and those not joined by connections, the extra winding for stopping the rotation of the fields with respect to the rotor. Clearly, since

contains two conductors, the average phase of the two, and it will be found that the average phase of the conductors per slot will vary uniformly round the circumference, the phase difference between adjacent slots being $(360 \times 4)/40 = 36°$, and thus the winding is a uniform 8-pole winding. This process may also be repeated on 12 poles, giving an exactly similar result.

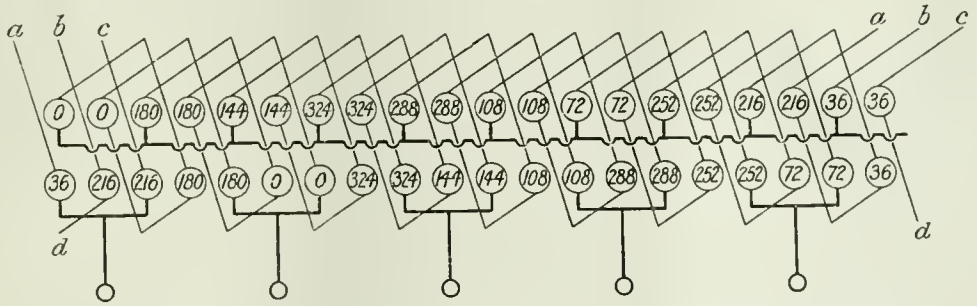


FIG. 39 (*a*).

FIG. 39 (*b*).

the cascade winding is unaltered whether we are running at the cascade speed or any other speed, the relative phases of the currents in it will not vary.

The phases in the currents in the different conductors may be obtained as follows:—

The 10 units of which the winding is composed carry five-phase currents, as has been pointed out, diametrically opposite units being in phase. Hence, if we mark the conductors in the slots 1, 5, etc., mentioned above with phase 0°, 144°, 288°, 216°, and 72°, in order, the connections of the circuit will enable us to obtain the phases of all the other conductors. For instance, if

The best way to prevent the rotation of the fields with respect to this winding is to insert entirely distinct windings in the empty slots, this second winding being connected up and short-circuited precisely like the first, when the machine is running on its basic pole speeds.

Winding (5).—This winding is a variation of winding (4), having double the number of conductors, and is formed by connecting the following pairs in series to form a unit in a manner similar to that previously described. The turn consisting of the upper conductor of slot 1 and the lower of slot 4 is connected directly

in series with that consisting of the upper of slot 2 and the lower of slot 5. The lower of slot 4 is connected to the lower of slot 7, and the turn consisting of the lower of slot 7 and the upper of slot 4 is connected directly in series with that consisting of the lower of slot 6 and the upper of slot 3. There will thus be eight conductors in series instead of four, and all the slots are now filled by a winding consisting of pairs of coils alternately reversed as described above. In this case the number of conductors does not vary from slot to slot, the variation of ampere-conductors being due to the varying

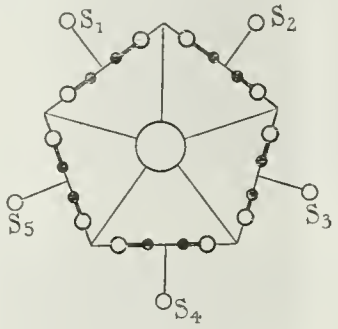


Fig. 40.

phase difference between the upper and lower conductors in any slot, the currents in the conductors in some slots being nearly in phase and in others widely out of phase.

Assuming that conductors differing in position by one-tenth of the circumference are 144° out of phase, as before, the connections of the winding enable us to fill in the phases of all the other conductors, and we can readily draw the vector diagram of resultant ampere-

and the second method described above must be employed, namely that of producing short-circuits between parts of the cascade winding. Each unit of this winding consists of two turns in series connected in opposition to another pair of turns in series, the whole four turns then being short-circuited. Introducing a short-circuit between the first pair of turns and the second pair of

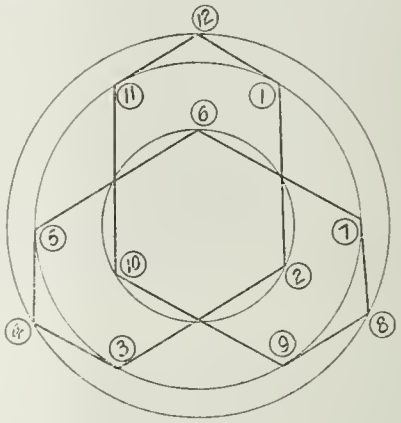


Fig. 41.

turns, i.e. joining S_1, S_2, S_3, S_4, S_5 to the common star-point or even to one another, we have what is required, and when this has been done every pair of turns on the winding is short-circuited independently. By connecting opposite sections together permanently, this also involves 5 slip-rings. There will now be, as in winding (4), 20 sections each independently short-circuited and, for the same reason as already mentioned, this is sufficient to make a satisfactory 8- or 12-pole

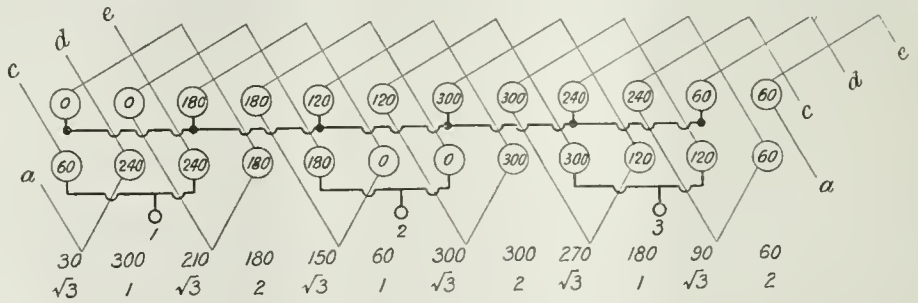


Fig. 42.

conductors, which will be found to be very close to the ideal curve. It may be connected in exactly the same way as winding (4), but is shown in Fig. 39 with both terminals of every set of 8 conductors joined to a common point.

Since with this winding all the slots are full, obviously a second winding cannot be used to get the basic speeds,

winding. The diagrammatic scheme of the winding is shown in Fig. 40.

Fig. 39 (*a*) shows the phases of the conductors in all the slots when the winding is operating on 8 poles, whilst Fig. 41 shows the vector diagram corresponding to this winding for the case of three phases. The winding for the three-phase case is shown in Fig. 42.

Discussion before the North Midland Centre, at Leeds, 12 April, 1921.

Mr. W. E. Burnand: I do not think there is a very large field for the cascade motor. It is not cheap to make, and from its construction I should say that it

neither was as efficient nor had as good a power factor as the induction motor, but at the same time it is worth the extra expense in some cases. The fact that a pro-

portion of the power has to undergo two transformations in a not very efficient transformer must lead to additional losses, and the considerable slip of the rotor with respect to the field again increases the losses. The unequal distribution of the ampere-conductors leads to a further additional loss and expense. In the case of a breakdown the winder will have great difficulty in carrying out the repairs, owing to the complicated nature of the windings. I should like to ask the author whether he actually constructed the four-speed motor described in the paper, and, if so, how it works in practice.

Mr. A. Pickersgill: I agree with the previous speaker that the field for the multi-speed induction motor is very limited because, owing to the complication of the windings in the event of a breakdown, a repair would take a considerable time to execute, as the winders would have to be specially trained in the construction of this type of machine. From the point of view of distribution these motors would not be desirable, as the power factor would, I think, be very low. With regard to the general application of this type of motor to fans for collieries, I think that if the colliery owners provide any for the mines, the best plan would be to run them at a high speed during the week-end and clear the pits of the foul air as much as possible while the miner is away. If anyone can invent an induction motor, either of the squirrel-cage or of the slip-ring type, that will give a good power factor from full load to half load, he will do more for the industry than by trying to develop a motor that has a very small field of application.

Mr. S. E. Hall: I do not agree with the two previous speakers as to there being no field for the cascade induction motor. If a commercial motor can be designed there are various applications for it, although when a large range of speeds is required the connections become complicated, and this is a great objection. At the same time, if the speed steps can be limited to a reasonable amount I think there are many applications in the industry for this motor. Is it possible to get a 15- or 20-h.p. motor to run at normal speed, twice and three times normal speed, and if necessary an intermediate speed, by means of an external resistance? Then the question of cost must be considered. Obviously it cannot compare with that of an ordinary induction motor, but is it possible to obtain such a motor? In the particular case under consideration the speed of the motor has to change from normal to high, perhaps 10 to 12 times per minute. Is that possible? In steelworks there is a demand for induction motors which would give perhaps three or four variable speeds without the use of resistances. Although the field for this type of motor is limited in comparison with that of the ordinary induction motor and direct-current motor, there is still a field for it. I should like to know whether the author can offer any suggestion in regard to the particular case I have mentioned.

Mr. E. Balmford: I should be glad if the author would give some more information as to the controller required for the cascade induction motor, and also some idea as to the number and size of the cables between the motor and controller. Does the resistance of such cables in any way affect the performance of the motor? I should also like to know whether, at any given speed within the range of the motor, its characteristics under varying conditions of load are similar to those of an ordinary induction motor, and, if not, the extent of the variation therefrom. What is the relation between the speed and output of the motor, and what is its reliability under working conditions?

Mr. S. D. Jones: The cost of repairs on this type of motor would be very considerable, and together with the first cost must be fairly heavy and would only be justified in special cases. In the case of the motor breaking down through a bad short-circuit, the winding would be so complicated that it would be very difficult to have it rewound in an ordinary repair shop. For this reason, I think simplicity should be aimed at and, if possible, any variable speed should be obtained mechanically rather than by complicated electrical methods. I have in mind a large works where variable speeds and "inching" are required for many machines. The most practical method of obtaining these speeds electrically is by the use of direct current, but this is not available. I visited several fabric printing works in Lancashire and found they were using slip-ring induction motors. In every case they were well satisfied with the results, although a large amount of power was being wasted in resistances to obtain the speeds. These works are installing constant-speed induction motors and obtaining the variable speed by means of the Williams–Janney hydraulic variable-speed gear. This consists of two oil pumps, one of which is driven at a constant speed by the motor. The stroke of its piston can be altered so that the amount of oil delivered to the second pump, and consequently also the speed, can be varied; this second pump is mechanically connected to the driven machine. The efficiency claimed is from 80 to 85 per cent, and I hope that all the machines totalling about 300 kW requiring variable speed will eventually be driven in this way. The method is expensive, but not nearly so much as a three-phase commutator motor, and is sound mechanically.

Mr. J. C. B. Ingleby: I think there is a great field for a variable-speed motor in connection with the propulsion of ships. I should like the author to give me some information on the following points. First, with regard to the nominal speeds of this motor, are they very definite or can intermediate speeds be obtained by resistance or other means? Secondly, if a heavy load is thrown on the motor does the speed drop to the next lower nominal speed? Thirdly, how do the weights and costs of a three-speed machine compare with those of an ordinary slip-ring induction motor built for the lowest speed; and what is the comparative cost of the switchgear for this motor? To judge from the paper, the motor is complicated. Fourthly, is the air-gap flux density fairly constant at each speed, and what is the percentage of overload torque? Also, how does this maximum overload torque vary at the different speeds, and can full-load starting torque be obtained without short-circuiting the rotor?

Mr. L. J. Hunt (*communicated*): There is a vast amount of work yet to be done and I would suggest that the iron losses of these machines would be a very

fitting subject for research. The results of commercial tests are very interesting, and it will probably surprise many designers to learn that the so-called " core losses " of these machines are often less when the iron is carrying the fluxes of both fields than when only the main field is present. A particularly interesting case recently came to my notice. The machine was wound for (14 + 28) poles, the two speeds being 428 and 142 r.p.m. on a 50-period circuit. With normal voltage the core losses at cascade speed were 20 per cent less than those at 14-pole speed. In Section 1 of the paper the author deals with the possible combination of poles which can be used, and shows that it is possible to build a motor in which the numbers of pairs of poles are prime to one another and both odd. The author's discovery of a practical stator winding suitable for (6 + 2)-pole motors is of great importance and renders possible the building of a commercial machine without slip-rings for 750 r.p.m. The author and I have worked out the design of a 50-b.h.p., 750-r.p.m., 50-period motor, and its calculated characteristics are very good. Judged by the results obtained with the 12-pole machine, I think that the prospects of the 8-pole cascade machine are distinctly encouraging. In Section 2 the author has developed

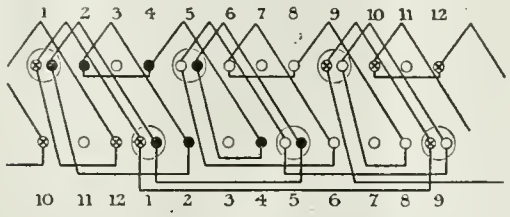


FIG. A.

a very good method of illustrating the theory of the rotor windings. That this method will be of great use to investigators is shown by the fact that, by its use, the author has been able to prove that the windings which are already in use can be made suitable for other combinations of poles by the simple expedient of adding a certain number of coils. Some years ago it was suggested that it might be possible to build a cascade rotor with a true squirrel-cage winding, but an investigation showed that this could not be done commercially because in an (8 + 4)-pole motor there were only six symmetrically placed points on the rotor at which the voltages induced by the 4-pole field would be opposite in phase to those induced by the 8-pole field. This meant that the rotor could have only six bars. The author's rule gives the number of points round the circumference of the rotor where the voltages due to the two fields will be opposite in phase. In practice, instead of using a squirrel cage having the number of bars given by the author's rule, we substitute for each bar a group of coils of one particular phase. It is only because copper can be saved and a more convenient winding developed that the star-mesh construction is used. The machine would work just as well if each phase were kept entirely separate as shown in Fig. A, which represents the winding shown in Fig. 13 (*a*) with the " star " conductors replaced by two bars. We now have three separate similar windings, each of which represents a single bar of the fundamental squirrel cage. Now the author by his rule finds that a (6 + 2)-pole rotor would have a 4-bar squirrel cage, and therefore he adds another section to that shown in Fig. 13 (*a*) and obtains a four-phase winding with 16 slots. When he wants a (12 + 8)- or a (16 + 4)-pole winding his rule tells him that he will have five sections. He therefore adds two sections to the 12-slot winding and obtains a five-phase construction with 20 slots. But it can be shown that this type of winding is not limited to these numbers of sections. By adding further slots and coils, the winding can be developed for other cascade speeds without consideration of magnetic balance. Four slots are required for each section and we can write down at once the number of poles (cascade) which can be obtained by increasing the number of sections of the winding.

Number of Sections	Number of Slots	Number of Poles (Cascade)
3	12	6
4	16	8
5	20	10
6 (3 repeated twice)	24	12
7	28	14
8 (4 repeated twice)	32	16
9 (3 repeated three times)	36	18
And so on		

In each case the key diagrams (of the type shown in Fig. 27) will have the common characteristic that, with all the slots filled, the coil numbers will run consecutively, i.e. starting with a star coil numbered 1, the next mesh coil will be 2, the next star coil 3, and so on round the diagram. When only cascade operation is required, alternate star coils, e.g. 3, 7, 11 ; or 1, 5, 9, etc., will be omitted. The key diagram of a 7-section winding would be a 7-sided figure with 7 star branches connected to the neutral, and a similar number of star branches to the slip-rings if a non-cascade speed is desired. Similarly, an 11-section winding would have 11 sides and two sets of 11 star branches. The author's discovery that a winding of this type is suitable for any two numbers of poles the sum of which equals the total number of poles for which the winding is designed, is of great interest and possibly of considerable value. In developing cascade windings I have always built up a combination winding from its components, and as the author's windings cannot be resolved into two sinusoidally distributed windings it is clear that they could not have been worked out by my method. The author's view that what really matters is the distribution of the resultant ampere-turns and not that of the components, may prove correct and can easily be decided by a test.

Mr. C. H. Stubbings (*communicated*) : The author points out that the Hunt rotor winding for (4 + 2) poles consists of three circuits, each carrying one of a three-phase system of currents. He has also arrived at the

conclusion that 4-pole and 2-pole fields superposed give three lobes, 6 poles and 2 poles give four lobes, and 6 poles and 4 poles give five lobes. Associating these three lobes with the three circuits of the Hunt (4 + 2)-pole winding, he states that, to obtain a winding for 6 and 2 poles, four more slots must be added, making four sections carrying four-phase currents, and, in order to obtain one for (6 + 4) poles, still another four slots must be added, giving five such sections carrying five-phase currents. The author, in my opinion, has failed to establish that he is justified in this assumption, and although I have spent some time in endeavouring to analyse the winding so obtained I have not yet succeeded in convincing myself that it is justifiable. From the author's general theorems it would appear to follow, but I have come to the conclusion that the only argument that holds with a cascade winding is a detailed mathematical investigation based on the most fundamental principles. For example, one would suppose that two fields of maximum values 1 and 0·866 (taking the case of the Hunt winding) would give a total maximum value of 1·866, but this does not appear to be the case. I have worked out the resultant field taking four slots per pole per phase, and considering every 10 degrees of the 4-pole field, and I find that at every instant the maximum value of the combined field is 1·75 times that of the 4-pole field alone at that instant, the resultant field giving a balance of electromotive forces around the closed circuits of the rotor. The author has considered ampere-conductors in obtaining Fig. 12, but a consideration of the combined flux in the air-gap gives three similar lobes if the maximum density at each of the 48 points of the circumference be taken. From the description of the method of obtaining Fig. 12, it would appear that the curve is merely the locus of the resultant vector of ampere-conductors and, although it clearly indicates that there are three maximum points, it does not show the actual variation of the ampere-conductors around the circumference. Possibly it would be instructive to plot the value of the resultant opposite the slot itself, giving a diagram from which actual values could be read. Referring to the rule given on page 520 for determining the number of maxima required in the curve of ampere-conductors, we should obtain with a (16 + 8)-pole rotor winding the value 3, but this rotor winding is merely an extension of the winding for (4 + 2) poles, and would have 12 maxima and carry three-phase and not twelve-phase currents. Similarly, a winding for (8 + 4) poles has six sections in which three-phase currents circulate, giving six lobes of ampere-conductors. The author's winding for (6 + 4) poles merely omits one section, but it has not been proved that the currents would then become five-phase currents, still giving a balance round these circuits. Indeed, the winding is still the Hunt winding of Fig. 13 (*a*) extended, and this winding was built up from the two three-phase windings of Fig. 13. If the phase of these currents be now altered to give a five-phase system, it would appear that Fig. 13 (*a*) will not now be the resultant of Fig. 13 or, if five-phase currents flow, they will not be confined each to its particular circuit. In other words, can the author analyse his 5-section winding carrying five-phase currents into its two components of 6-pole and 4-pole fields? Considering the stator windings, we find two types proposed by the author. The first shown in Fig. 30 (*a*) seems to require a considerable number of leads and terminals and, if an auto-transformer is used in conjunction with it, appears to give 8, 12 and 16 poles. The second (Fig. 33) requires a winding split up into three parallel paths, and a somewhat cumbersome form of three-phase controller. If each section occupies one slot, we have three slots per pole per phase, a useful number, but the next alternative seems to be six slots per pole per phase. It is doubtful whether the result of getting the higher basic speed justifies a further complication of the windings, for simplicity in the design of electrical apparatus certainly commends itself to customers, who are not always in a position to have repairs executed by a man trained to connect the right coils to the proper terminals. The author has undoubtedly made a most valuable contribution to the study of cascade single windings, but he has not succeeded in breaking away from Mr. Hunt's windings, and further has not clearly shown that they are adaptable to the new conditions.

Mr. F. Creedy (*in reply*): In writing the paper, it must be admitted that I took for granted the various contributions by Mr. Hunt on the same subject. The present paper was intended not to announce the discovery of a wholly new type of electric motor, but to show how some of the limitations of the existing type of cascade motor could be removed. From the discussion it would appear as if I had taken too much for granted with respect to the general knowledge existing of the older types of cascade motor and Mr. Hunt's work in general. Perhaps it would be as well if I were to explain more clearly how the work described in the present paper is related to that already accomplished. Machines wound for three speeds in the ratios 6/3/2 have been built for many years, and two-speed machines with speed ratios of 3/2 and 3/1 are absolutely a standard feature of the alternating-current motor market, being built by quite a number of well-known firms under Mr. Hunt's patents. In addition, single-speed machines having the characteristics of a slip-ring induction motor but with short-circuited rotors are in use in very large numbers, running at speeds not exceeding 500 r.p.m. at 50 periods. The question as to whether there is a large field for the cascade motor therefore does not arise at all, as it was settled long ago, the motors having been on the market at least 14 years. Very large numbers of these motors are in daily use, especially of the single-speed type, i.e. the type described above as having no slip-rings. They are in use very largely in collieries, quarries, etc., for driving all classes of machines, particularly haulage gears, crushers, etc., and for many other purposes, including printing presses, paying-out gear in cable ships, etc.

We now come to the special development represented by the present paper. The existing type of cascade motor suffers from certain limitations owing to the fact that it is confined to the ratios of speed just mentioned, and cannot be built for any others, and because

the single-speed type which is so very widely used cannot be built for speeds higher than 500 r.p.m. It is these limitations which the present paper seeks to remove by the introduction of a single-speed motor without slip-rings running at 750 r.p.m., and of methods whereby we are no longer limited to particular ratios of speed in the cascade machine, but can obtain practically any ratio we desire. Having in this way removed any obstacles that may at present impede the advance of the cascade motor, we may expect it to occupy a field even more extended than it does at present.

I shall now reply in more detail to the various points raised in the discussion. It is quite a mistake to suppose that the transformation of power which occurs in the cascade motor leads to greater losses than in motors of the more usual type, since the no-load losses in the cascade motor are practically the same at cascade and non-cascade speed and, further, the ampere-conductors of the rotor are increased by less than 7 per cent. As regards the power factor, it has been repeatedly demonstrated that it is in the case of the cascade machine higher than in the case of a normal machine operating at the same speed, since to obtain, say, 500 r.p.m. with a normal type of machine we require 12 poles with a correspondingly high leakage, whereas with a cascade machine we obtain the same result with 8 poles.

In reply to Mr. Burnand as to supposed difficulties in repair, I may remark that the present paper is really a designer's paper intended to elucidate points of theoretical difficulty in the study of these motors, and is therefore full of rather complicated diagrams. The diagram of even a simple 2-pole direct-current drum winding if drawn out in full is very complicated, and would lead one to suppose at first sight that such a machine would be too complicated to be a practical success. But we all know this is not so, and I think that some speakers have fallen into some such mistake as the above and have confused diagrams drawn for theoretical elucidation with practical requirements imposed on the winder.

With regard to Mr. Pickersgill's remarks, I must again express my surprise at the apparent extremely limited knowledge of the existing position of the cascade motor. It has been pointed out already that what Mr. Pickersgill desires to see, namely "an induction motor either of the squirrel-cage or of the slip-ring type which will give a good power factor from full load to half load," is exactly what has been accomplished by the cascade motor, which gives a materially better power factor than the ordinary induction motor. Another large field for these machines is in the driving of rolling mills, compressors, etc. With regard to the former, two-speed motors ranging in size from 150 to 900 h.p. have proved very successful, and have been built for speeds as low as 80 r.p.m.

A good many of the points raised by Mr. Hall have already been dealt with. There is no difficulty in building a motor having two speeds in practically any desired ratio with intermediate speeds obtained by resistance control. Cascade machines have been tested for driving live roller gear reversing 30 to 35 times per minute, so that there is no doubt that the requirements which he suggests, i.e. the change from normal to high speed perhaps 10 or 12 times per minute, can readily be attained. Where more than two speeds are required the methods described in the paper enable us to obtain practically any number of speeds. The control of machines having any number of speeds up to four is by no means complicated.

In reply to Mr. Balmford, I may say that the controller required for the two-speed cascade motor is of the simplest possible type, consisting merely of an ordinary rheostat connected across the slip-rings. When this rheostat is open-circuited the machine runs at the lower speed. By gradually closing the rheostat the speed of the machine is steadily raised from its lower to its higher value, and I think it will be agreed that no simpler form of control can possibly be conceived. The number of cables is, of course, equal to the number of slip-rings, and their capacity depends on the size of the motor. The general characteristics of the machine when running at any of its fixed speeds are similar to those of the ordinary induction motor, with the exception that the slip between no load and full load is considerably less, and the machine is capable of running continuously, when required, at a crawling speed of about 2 per cent of full speed. The machine is started by means of a rheostat of any desired type attached to tappings on the stator windings. The relation between the speed and the output of the motor is the same as that in an ordinary induction motor, and, since constructionally the two are practically identical, the reliability is fully as high, and in the case of the single-speed motor certainly much higher.

In reply to Mr. Jones, I may deal with the remarks of those speakers who have asserted that the machine is difficult to build and repair in case of breakdown occurring due to short-circuit or other difficulty. A motor supplied to a mine on the West Coast of Africa and in which a breakdown occurred was successfully repaired by ordinary native labour. These natives, as is well known, do not make any special claim for abnormal intelligence, and I fear that any suggestion to the effect that English workmen are unable to do similar work will not meet with a very warm reception. Mr. Jones mentioned the application of this machine to printing work, and I may say that it is already in use in many cases. I have already described the extreme simplicity of control with this type of two-speed machine and its quality of creeping at 2 per cent of normal speed, which for many purposes is very well adapted for printing work. The Williams-Janney hydraulic variable-speed gear is very interesting but also very expensive, as Mr. Jones points out, and in many cases multi-speed induction motors not only of the cascade but other types are successfully in use in the operation of printing presses. A case recently came under my notice of a printing press operated by a direct-current motor, speed variation being obtained by a field rheostat in the ordinary way. It was found on examining the installation that the printer had marked the field rheostat with chalk in three or four places, indicating the positions at which it should be for different operations, thereby showing quite clearly that three or four was the maximum number of speeds required. I cannot entertain any doubt that both

three-phase commutator motors and mechanical variable-speed gears will ultimately be eliminated by multi-speed induction motors on every alternating-current circuit.

In reply to Mr. Ingleby, the nominal speeds of this motor are, of course, fairly definite when the resistances are short-circuited, but intermediate speeds can always be obtained by means of resistance control. If a heavy load is thrown on the motor a contactor gear operated by an excess-current relay can be arranged so that the speed of the motor falls to the next lower nominal speed. A considerable amount of information as to the weights and costs of these machines compared with an ordinary slip-ring induction motor is to be found in Mr. Hunt's 1914 paper.* The percentage of overload capacity is about the same at every speed, and is, of course, under the control of the designer of the machine. The flux density falls off gradually in most cases as the speed rises. Full-load or twice full-load starting torque is obtained by the use of an appropriate resistance connected to the stator tappings with the rotor winding completely open-circuited.

Coming now to Mr. Hunt's remarks, I quite agree that there is a vast amount of work yet to be done in perfecting the multi-speed and, in particular, the cascade induction motor. I am very interested in the results of the test referred to by Mr. Hunt in which with normal voltage the core losses were 20 per cent less at cascade speed than those at top or 14-pole speed. Mr. Hunt's designs are usually of the open-slot type, and it is well known that in machines of this character a pulsation loss occurs in both the rotor and stator iron, due to the rapid changes in the flux in the rotor teeth owing to the passage of the open slots over them. This loss is, of course, proportional to the speed, and in this connection I may refer to a paper † by I. E. Hanssen. I would suggest that the phenomenon that Mr. Hunt noticed is due to this pulsation loss, which is lower at cascade speed than at 14-pole speed, and since in this machine probably a large proportion of the total iron loss is pulsation loss, this loss will be greater at a higher speed. Mr. Hunt states from a new point of view the methods which I have developed for obtaining cascade machines having any desired ratio. This re-statement gives a graphic and clear explanation of the matter, and I am in perfect agreement with it. We have gone through the subject of leakage carefully together and, while undoubtedly any machine must have a certain amount of leakage, it has been proved that these cascade windings, including both the new and old types, have materially less leakage than the normal three-phase winding, since they have a greater number of phases per pole and therefore a better and more nearly sinusoidal distribution of ampere-conductors.

I note that Mr. Stubbings has come to the conclusion that the only argument which holds with the cascade winding is a detailed mathematical investigation based on the most fundamental principles. It is precisely such a fundamental investigation that is lacking in his remarks, and I think if he will take the trouble to read the paper through once more he will agree that the conclusions are fully demonstrated. It is quite possible to analyse the ampere-conductor diagram of a 5-section winding carrying five-phase currents into two components corresponding to 6- and 4-pole fields, and this has repeatedly been done by myself and Mr. Hunt in conjunction. Of course, the two component distributions of ampere-conductors into which such a resultant diagram is analysed do not necessarily correspond to distributions which could be produced by an actual winding capable of being assembled on a core by a winder. But this is not at all necessary. If we can resolve a resultant curve into two component sets of ampere-conductors and from these deduce the two component fields, this is all that is required. Mr. Stubbings will find the rule given on pages 519 and 520 for determining the number of maxima required in the curve of ampere-conductors quite exact, if he will take the trouble to read it again, as reference is fully made to cases where two numbers of pairs of poles have a common factor, and if he bears this in mind I think he will find his difficulties entirely removed. As regards the stator windings, the first (shown in Fig. 38) has the same number of terminals as the well-known three-speed motor as built under Mr. Hunt's patents, and these could conveniently be brought to a controller fixed on the side of the motor in exactly the same way as at present. As regards the second (Fig. 33) the control required is identical with that at present used for the 12-pole type of single-speed machine, except that three-phase instead of single-phase water resistances are used. Mr. Hunt and I, as mentioned in his remarks, have worked out designs for these 8-pole single-speed motors, and these show extremely promising results. Mr. Stubbings is, I believe, particularly interested in the flux distribution in these machines. I therefore call his attention to a remark on page 525 of the paper in which it is pointed out that the side of the polygon between the two angles marked with two neighbouring slot numbers represents in magnitude and phase the magnetomotive force acting in the tooth between the two given slots. By projecting these sides on any fixed axis we may find the numerical values of the magnetomotive force for every tooth round the circumference at any given instant, and this is of considerable assistance towards obtaining the resultant flux. A curious point may be here remarked on, i.e. that when the primary and secondary flux densities are equal there is a limited number of fixed points in the rotor which at every instant carry no flux. Such a cascade rotor could, therefore, be built with salient poles.

* *Journal I.E.E.*, 1914, vol. 52, p. 406.

† "Calculation of Iron Losses in Dynamo Electric Machinery," *Transactions of the American Institute of Electrical Engineers*, 1909, vol. 28, p. 993.

ELECTRIC DRIVING IN THE PAPER MILL, ON HEAT-ECONOMY LINES.

By A. B. Mallinson, Associate Member.

(*Paper first received* 10 *January, and in final form* 7 *February*, 1921; *read before the* North-Western Centre 8 *March, and before the* Liverpool Sub-Centre 21 *March*, 1921.)

Summary.

The various uses for which steam is required in paper manufacture are first discussed.

Then follow particulars of the various classes of machinery to be driven, and comments on the selection of suitable electrical gear. The heating requirements for heat economy, and the various types of power plant by which this can be attained are discussed in detail, together with recommendations on how each type of plant will be used for the different types of paper mills.

The utilization of existing prime movers on "heat-economy" lines is specially shown.

The factors governing the choice of electric supply are then dealt with.

Finally, the driving of the variable-speed end of the paper machine proper is discussed in detail. The requirements for a successful drive are first enumerated, then various means of attaining that drive, not only with a single motor, but alternatively with several motors, both electrically and mechanically interlocked, are investigated.

A Table at the conclusion of the paper gives particulars of machines which have been converted for electrical working in the United Kingdom, and the total number of machines in the country.

Introduction.

As compared with most other manufacturing processes, the electrification of paper mills has, until the last year or so, been very backward. The paper maker is decidedly conservative, and this, coupled doubtless with the unpleasant results of a few experimental installations, has made him very loath to abandon the present steam-driven machine lay-out, which has done its work so reliably.

There were undoubted signs, however, even in 1914, that paper makers were beginning to realize that, whilst their system of individual steam drive might be ideal as regards reliability, there were great possibilities of fuel economy to be attained by electrification of the paper machines in conjunction with a suitable heat-extraction power plant.

The post-war conditions of fuel supply have made the matter one of first importance, not only to the paper maker, but to the country in general. This, together with the possibilities for development in this country, are, the author considers, sufficient reasons for presenting a paper on the subject to this Institution.

The general requirements of paper-mill practice, and the possibilities of improvement in the economic utilization of heat and power, were dealt with from the paper makers' point of view very fully in a paper by Mr. W. Adamson, recently read before the Technical Section of the Paper Makers' Association of Great Britain and Ireland, in Manchester.

The author hopes to carry the matter a stage further, by the discussion of the various types of possible prime mover, and particularly the electrical requirements for driving the paper machine.

Manufacturing Requirements.

For paper manufacture in this country steam is an absolute necessity, being used:—

(1) As a means of heating in the chemical treatment of the raw material, such as:—

(*a*) Waste paper boiling and softening at 1 to 15 lb. gauge pressure.
(*b*) Chemical pulp softening at 1 to 10 lb. gauge pressure.
(*c*) Rag boiling at 20 to 50 lb. gauge pressure.
(*d*) Esparto digestion at 30 to 60 lb. gauge pressure.
(*e*) Straw digestion at 40 to 70 lb. gauge pressure.
(*f*) Soda recovery in esparto plants at 5 to 20 lb. gauge pressure.

(2) For the generation of power, to drive the beating, breaking, and refining plant, also the electric power and lighting plant. This is generally done by one of the orthodox types of compound condensing mill engines.

(3) For both power to drive and heat the drying cylinders of the paper machines, such heating steam being utilized at pressures of from 1 to 20 lb. (or even 30 lb.) per square inch.

(4) To warm the air in the buildings, particularly in the machine houses, and so obviate troubles from condensation, being used at 5 to 50 lb. pressure, depending on the type of heater.

(5) As a direct or indirect heating medium for sundry purposes in the mill, such as accelerating bleaching, size mixing, softening of pulp, freeing of paper stock as delivered to machine, etc.

(6) Steam injectors for hosing down at machine wet end.

(7) Condensation losses in the distribution of the steam throughout the mill.

(8) In the boiler-house, for the auxiliaries in steam raising.

The steam requirements under the various headings vary materially for different classes of paper. Table 1 may be taken as representing very good practice in most mills; for old mills which have not been brought up to date, the figures would be appreciably more.

The variations are dependent on the quality of paper being made, process used, and class of raw materials used for furnish.

TABLE I.

Pounds of Steam per Ton of Paper.

Class of Paper made	For Boiling or Digesting, etc.	For Main Power	Paper Machine Drive and Dry	Total
	lb.	lb.	lb.	lb.
News, 25% sulphite ..	—	7 500 to 8 500	7 500 to 8 500	15 000 to 17 000
Cheap unbleached printings	—	14 000 to 15 500	8 500	22 500 to 24 000
Bleached printings ..	—	21 000 to 25 000	8 500	29 500 to 33 500
Pure esparto paper ..	21 000 to 30 000	21 000 to 29 000	8 500 to 9 000	50 500 to 68 000
Esparto wood papers, 50 to 50%	8 000 to 15 000	12 500 to 26 500	8 500	29 000 to 50 000
Pure rag writings	6 000 to 10 000	48 500 to 65 500	8 500 to 9 500	63 000 to 85 000
Wood rag writings, 50 to 50%	3 500 to 5 000	22 000 to 28 500	8 500	34 000 to 42 000
Browns	900 to 8 000	7 600 to 34 000	8 500 to 9 000	17 000 to 51 000

This Table shows some interesting points:—

(*a*) That it is obvious that no electrification scheme from a public supply could entirely supersede the steam-raising plant, unless the power company happened to be so placed geographically that it could, in addition to supplying power, supply steam for heating and manufacturing purposes.

(*b*) That in every class of mill a practically constant quantity of steam per ton of paper is required for the paper-making machines. While on paper this is generally true, it is a factor liable to considerable variation. The general practice nowadays is to drive each paper machine by two independent high-speed engines; one, the "wet end" engine drives the agitators, screen, shake, and various pumps at a constant speed, and takes practically the same power for any speed of running of paper machine; the other, the "dry end" engine, is a variable-speed unit, the speed being varied according to the calliper of sheet to be made. These steam-engines are almost invariably high-pressure engines consuming about 35 lb. per hour per indicated horse-power supplied from boilers at about 100 to 120 lb. pressure, or, if from a high-pressure boiler, through a reducing valve.

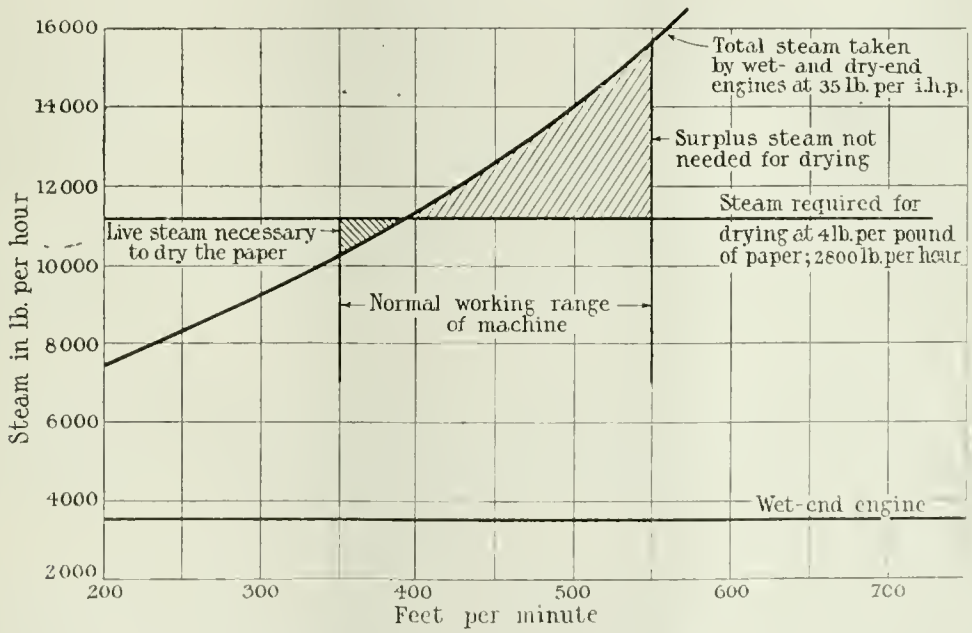


FIG. 1.—130-inch news machine, approximate steam consumption for heating and power.

The weight of paper turned out per hour is approxi-

mately constant, the speed being varied to suit the thickness of the sheet.

Fig. 1 shows how at one point only can the steam used for power correspond to the amount required for drying. If this point comes low down on the curve then there must be considerable surplus steam blown to waste when running at higher speeds.

(*c*) That in every class of mill (assuming, of course, there is little or no water power to augment the driving of preparation plant) enough heat passes away in the condensing water to supply all, or a considerable portion of, the requirements for heating purposes.

(*d*) That by the adoption of a suitable type of power unit, therefore, with such a steady demand for power and heating steam, the electrification of the paper machines, used for drying the steam from the power unit, is bound to lead to very material fuel economy.

Financial Consideration.

The whole object of any re-modelling on heat-economy lines is, of course, fuel saving; but capital outlay is, to the mill owner, with finance in its present state, of equal and probably, in many cases, greater importance.

Any scheme to justify consideration not only must show marked fuel saving, but be a paying proposition after providing interest and depreciation, etc., on the whole capital outlay incurred at present-day rates.

In this respect the possibilities of utilizing the existing prime movers on heat-economy lines with the capital outlay restricted to the provision of the extra power required merit the closest investigation in any existing mill.

Driving Requirements on Heat-economy Lines.

These can be split up into:—

(1) *Preparation.*—Comprising rag and grass dusters, choppers, conveyors, rotary digesters, pulpers, bleach plant, etc., the details of which drives present no difficulties. Table 2 gives typical examples of such drives and the power taken under stated conditions of tests.

(2) *Breaking, beating and refining.*—Where the stock is treated until ready for letting down into machine chests. This section, comprising the bulk of the drive in any mill, is in present-day practice almost universally a mechanical grouped drive, the prime mover being coupled direct or by ropes to a main shaft in the beater cellar from which the various drives in turn are taken. Advantages of this system are:

(*a*) All machines require to run continuously to facilitate charging and discharging, so there is no gain in being able to stop and start individually.

(*b*) All machines are such as are easily subject to heavy overloads, which if individually driven necessitates very liberally rated motors, whereas with mechanical drive the peaks average themselves out.

(*c*) It is cheapest, both in capital outlay and running cost.

Table 2.

Preparation Plant Electric Drives.

Drive	Size	Machine Speed	Drive	Motor Installed		Tested Load	
		r.p.m.		h.p.	r.p.m.	Duty	b.h.p.
Nuttall rag chopper ..	24 in.	100	Belt	20	400	Cutting ropes and bagging	$7\frac{1}{2}$ to 12
Coburn rag chopper ..	16 in.	100	Belt	14	500	Seconds rags	6 to 10
Rag duster	5 ft. × 12 ft.	24	Belt	$3\frac{1}{2}$	360	Seconds rags	$2\frac{1}{2}$ to $3\frac{1}{2}$
Esparto duster and fan ..	—	265	Belt	20	600	55 cwts. per hour	15
Rag boilers	Three 8 ft.	$1\frac{1}{2}$ to 2	Worm and belt	10	1 000	All on	$7\frac{1}{2}$
Rag boiler	12 ft. spherical	$\frac{1}{2}$	Worm and spur	10	600	Full	$5\frac{1}{2}$ to 6
Straw digester	17 ft. × 10 ft.	11 to 3	Worm and spur	14	1 000	3 tons	10 to 12
Masson & Scott wet paper pulper	Large	50	Spur and belt	30	500	10 to 12 cwts. per hour	20 to 25
Dry paper pulper and conveyor	Large	50	Spur and belt	50	500	18 to 20 cwts. per hour	45 to 60
Kolergang	9 ft.	10	Belt	16	500	Milling waste papers	12 to 15
Clay mixers	Two mixers and hoist	Shaft 140	Belt	10	700	Usual duty	3 to 8
Bleach mixers	Three 7 ft. and pump	Shaft 120	Belt	10	500	Two mixers on	2 to 6

Table 3 gives particulars of typical electrically-driven machines forming this group.

It must be remembered that the power taken in all these processes varies materially with different classes of raw material: wood pulps require least, and rags and ropes, most power.

Paper machines.—These would be electrically driven.

Calenders and cutters require speed variation, under conditions, however, which can generally be easily met by ordinary shunt control. The calender drive can be considered just as a machine drive, any one of the systems referred to being applicable to it.

Table 4 gives particulars of typical drives for finishing-department machinery.

TABLE 3.

Breaking, Beating and Refining—Electric Drives.

Drive	Size	Shaft or Roll	Drive	Motor Installed		Tested Load	
		r.p.m.		b.h.p.	r.p.m.	Duty	b.h.p.
News breaker	23 ft. × 11 ft. 6 in.	120	Belt	80	480	1¼ tons per hour	60 to 80
2 news breakers and chest and pump	23 ft. × 11 ft. 6 in.	130	Ropes and belt	175	480	1¼ tons each per hour	150 to 170
Rag breaker	19 ft. × 7 ft. 6 in.	120	Belt	50	500	12 cwts. ropes and bagging	41 to 48
Esparto potcher	30 ft. × 9 ft.	95	Belt	25	500	Washing	20 to 21
Sulphite breaker	13 ft. × 6 ft. 6 in.	110	Belt	12	460	4 bales furnish	10 to 12
"Milne" breaker, 2 rolls	32 ft. × 12 ft.	80	Gear	70	530	39 cwts. esparto	52 to 60
"B and J" beater ..	13 ft. × 6 ft. 9 in.	144	Belt	50	500	500 lb. ropes	45 to 50
"Bertram" beater ..	20 ft. × 8 ft.	150	Belt	40	600	500 lb. pulp	20 to 30
Beater	18 ft. 6 in. × 9 ft.	140	Belt	70	450	Rags	56 to 70
2 beaters	13 ft. 6 in. × 6 ft. 9 in.	160	Ropes	65	350	Rags	60 to 70
4 "M and S" tower beaters	4 ft. roll 4 ft. 3 in. tower	C.S. 180	Ropes	180	540	Esparto	160 to 180
"M and S" tower concentrator	4 ft. 3 in. diam.	184	Gear and belt	50	550	Esparto	35 to 50
News refiner	Walmsley	320	Belt	150	480	1¼ tons per hour. Usual news	110 to 150
Marshall refiner	"B & J"	250	Belt	100	500	Esparto and wood	60 to 90
Disc refiner	Bertram	200	Belt	60	500	600 lb. per hour rag wood furnish	40 to 45
Presspate	4 ft. 6 in.	184 (60 ft. per min.)	Gear	24	850	—	15
Presspate	5 ft. 6 in.	—	Gear	28	700	20 cwts. wet pulp per hour	16 to 20

The wet end represents no difficulty as it is simply a constant-speed drive. The various ways of driving the dry or variable-speed end form the main theme of this paper.

Finishing department.—Comprising the calendering, reeling, cutting and making-up of the finished paper. In most modern mills all these machines are already electrically driven and present no outstanding difficulties.

SELECTION OF MOTORS.

As the driving shafts of many of the machines used in the paper mill run comparatively slowly (60 r.p.m. for cutters, 100 to 150 r.p.m. for beaters, etc.), the use of high-speed motors is not to be recommended. In many otherwise excellent installations motors have been installed with ridiculously small pulleys, or the introduction of a more or less noisy backgear, so as to enable a high-speed motor to be put in. Doubtless

the competition to book the order has been the cause, but that can hardly be considered a sufficient excuse for setting up weak links in an expensive lay-out. Manufacturers would be well advised when submitting such schemes to give the purchaser alternative tenders, one the cheapest possible by using high-speed motors, and the other a recommended alternative with a low-speed motor and a sound millwrighting drive.

A typical example of this is the cutter drives quoted in Table 4, 4-h.p. motors running at 180 to 360 r.p.m. These, which were installed in 1903, have run perfectly,

Switchgear cannot be too good for the paper mill ; the conditions of handling, hours of service, humidity of atmosphere, bleach fumes, etc., will soon make trouble otherwise.

Wiring in tubing and on the lead-covered system is of no use. Cab tyre sheathing stands up well if carefully erected, using separate insulators with ample gripping surface (preferably not ridged), or cleats. For three-phase work armoured cable, with sealing boxes on standard lines, is worth the slight extra outlay. Bitumen-insulated cables should not be used in the machine

TABLE 4.

Finishing : Electric Drives.

Drive	Size	Speed	Drive	Motor Installed		Tested Load	
	in.	ft. per min.		b.h.p.	r.p.m.	Duty	b.h.p.
Single-drum reeler ..	72	—	Belt	6	800 to 1 000	200 ft.	4
2-drum winder	100	1 130	Belt	—	—	90 in. 16 oz.	13½
4-drum " M & W " winder	140	273 r.p.m. 1 000 ft. per min.	Belt	25	750	News 1 000 ft.	23
Damper Schurmann ..	112	600	Belt	6	1 200	—	5 to 6
Friction calender	72	70 to 210	Gear	35	300 to 900	Rag browns	30 to 35
Glazing calender, 5-roll ..	80	150 to 300	Chain	25	400 to 1 200	—	12 to 15
2-plate glazing calenders..	36	—	Belt	8	600	—	4 to 8
Coated paper calender, 7-roll	50	60 to 210	Gear	22	600 to 1 100	25 in. wide 180 ft.	9 to 12
Super-Eck, 10-bowl ..	96	50 to 400	Gear	50	400 to 800	240 ft.	30
Super-Schurman, 10-bowl	112	80 to 450	Jockey pulley	100	850	420	70
Ditto, 12-bowl	72	60 to 400	Belt	70	450 to 650	Wall-paper 375 ft.	63
Ditto, Walmsley, 10-bowl	130	30 to 600	Belt	A.C. 15 and 150	1 000 and 720	News 600 ft.	120
Single cutter and layer ..	84	70 to 200	Belt	2½ to 4	190 to 500	70 ft. to 180 ft.	1¼ to 2¾
Duplex cutter and layer..	120	80 to 160	Belt	4½	180 to 360	135 ft. duplex	3¾
Duplex cutter	72	100 to 200	Belt	4	180 to 360	25 in. boards 200 ft.	3½
2 coating machines with fans and reeler	48	C.S. 150 r.p.m.	Belt	15	550	About 120 ft.	12 to 14
Guillotine	48	C.S. 250 r.p.m.	Belt	4	1 000	4 in. cut	3½*

* Momentary.

there being no noise, no fast-running belts, practically no wear and tear, but just a slow-running belt from a motor on a wall bracket to the driving pulley on the cutter.

The pipe-ventilated motor with a properly-run air-inlet pipe is undoubtedly the best for paper mill conditions, as in all parts of the process there is either an abundance of dust, nap or water. There is no need for air-outlet pipes ; the warm discharged air helps to heat the room.

SWITCHGEAR AND WIRING.

These two essential factors, which can so easily make or mar the installation, will only be dealt with briefly.

houses, where the roof temperature is sometimes 130 or 150° F.

Generally speaking, for both switchgear and wiring, there will not be much trouble if good mining practice is followed.

RELIABILITY.

All equipment, whether power-plant, individual motors, or machine drives, must be absolutely reliable. Paper mills run night and day, and obtain their maximum output by keeping the paper machines running with the minimum of " shut." A stoppage in main power plant, with the present system of engine-driven machines, of even half an hour does not affect the

weekly output since the paper machine carries on, having sufficient reserve of prepared stock in the chest in front of it; on the other hand, on an electrically-driven machine the tripping of a circuit breaker, causing machine motors to shut down for just a few seconds, is fatal, as it entails washing down the wire, with attendant loss of time, output, and stock. This, to the paper maker, is one of the most vital factors in consideration of any scheme of power reorganization.

Generally speaking, the simpler the equipment is the better; electrical interlocks and foolproof safeguards can be easily overdone in this respect.

Heating Requirements.

Before any choice of power plant can be made, the heating requirements need thorough consideration.

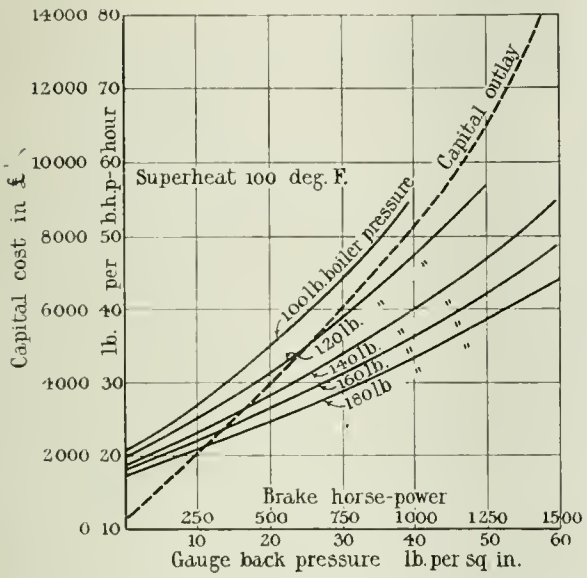


Fig. 2.—Back-pressure high-speed steam engines up to 1 500 b.h.p.

Curves showing effect of boiler pressure and back pressure on steam consumption, also capital cost at 160/180-lb. boiler pressure and 15 lb. back pressure.

For drying the web on the paper machine, where there is a continuous draw of steam for drying purposes, the conditions are fairly simple. Actual tests have proved that 3½ to 4 lb. of steam are required for every pound of paper delivered on the reel, with the present system of cast-iron drying cylinders; roof heating in machine houses requires 0·2 to 0·5 lb. per pound of paper.

The digestion or boiling of rags, where the usual 8-ft. digesters are installed, averages out to a fairly steady draw for the boiling-house; esparto or straw digesters with their great capacity cause very considerable fluctuations in the demand for heating steam, which may easily upset all calculation unless the power unit is suitably installed.

The actual measurement of the steam used by an approved steam meter or isolation of a steam boiler taken over an extended period of normal working, together with a thorough knowledge of the process and its requirements, is the only safe guide.

The pressure at which the heating steam is required also merits scrutiny; cases will often be found where by slight rearrangement of pipes, or the proper use of covering medium, the pressure can be appreciably reduced without any detriment to the process.

Fig. 2 shows the steam consumption with various degrees of back pressure; the gain by keeping this back pressure down to a minimum, particularly where medium-pressure boilers are being used, is obvious.

Where heating requirements fluctuate (as in esparto mills) the introduction of a heat accumulator or receiver between power house and mill will be found beneficial.

To reap the full benefit from the installation the exhaust steam must be utilized for every duty where heat is needed in the mill, whether it be direct steam, indirect steam, hot air, or hot water.

Return of Heat to Boiler Feed.

Paper-mill practice lends itself well to this. No mill should, if properly laid out, even with the present-day system of condensing prime mover, have a hot-well temperature less than 150 to 160° F. With a heat-economy lay-out this should increase to 180 to 200° F., with resultant saving in smaller economizer lay-out needed. All condensed steam being pure, every possible drop should be returned to the hot-well, to save cost in treatment of feed water. With reciprocating engine prime movers there will be oil in the exhaust, which may be removed by an efficient oil separator.

Steam Boilers.

Where the maximum of power from the heating requirements is desired, steam at 180 to 200 lb. pressure, with superheat to 550 to 600° F., is advisable.

Many cases, however, will be found where lower pressures of 160 or even 120 lb. can be effectively used, thus enabling considerable saving to be made in the capital outlay. The pressure adopted depends on the requirements in quantity, and the pressure of heating-steam and power demands.

Superheat is absolutely essential to ensure dry steam for the heating requirements.

Types of Power Plant.

For the purposes under consideration these are:—

(1) Back-pressure high-speed compound steam engine.
(2) Back-pressure horizontal compound drop-valve engine.
(3) Back-pressure steam turbine.
(4) Heat-extraction horizontal compound condensing steam engine.
(5) Heat extraction or bleeding condensing steam turbine.
(6) Utilization of existing prime movers with mixed-pressure turbine, and other additional power units.

No particular system of prime mover can be said to supply the requirements of every type of paper mill.

A typical one-machine equipment may be instanced :—

Boiler : 30 ft. × 8 ft. 6 in., 180 lb., superheat to 550° F., hand-fired.

Engine : Horizontal drop-valve compound 400 h.p., 15 lb. back pressure, driving four beaters, revolving boilers, dynamo for machine, and usual auxiliaries.

Machine : Driven by 30-h.p. motor, speed range 8 to 125 feet per minute.

Output of paper : 70 tons weekly.

Over some years' operating, this mill averages about 16 000 lb. of steam to every ton of paper made, a figure fully 30 per cent less than on mills with the usual condensing main engine and high-pressure machine engines.

In general terms, having settled the power required, the quantity of steam for heating, and the required pressure, then the right type of prime mover is the one fulfilling these conditions with the minimum of capital outlay.

(1) *Back-pressure high-speed compound engine.*—This engine, coupled direct to the beater shaft by an efficient flexible coupling, or ropes, with the dynamo for machines and auxiliary drives direct or rope coupled, is materially the cheapest prime mover for heat-economy purposes, both in plant first cost, foundations, and housing. Its reliability has been abundantly proved. It is a type of engine which the paper-mill engineer thoroughly understands.

Fig. 2 gives the relative performance of this type of engine under different boiler and back pressures, together with an approximate idea of capital cost, including delivery and erection, for a direct-coupled drive to beater shafting.

Provided the horse-power required will not entail the production of too much surplus heating steam, and the demand for heating is practically steady, there can be no better system installed. It lends itself particularly to news, cheap printings, and certain classes of brown mills. If there are considerable variations possible in the heating demand, which do not require increase in horse-power, as in grass digestion, then it is not the system to adopt.

Where the conditions of power demand are such as to create too great a surplus of heating steam with this prime mover, or where the power demand fluctuates considerably, a combination with a similar unit working condensing, and electrically coupled together, or a heat-extraction condensing engine will probably fit the case better.

(2) *Back-pressure horizontal compound drop-valve engine.*—This is an adaptation of the modern low-speed

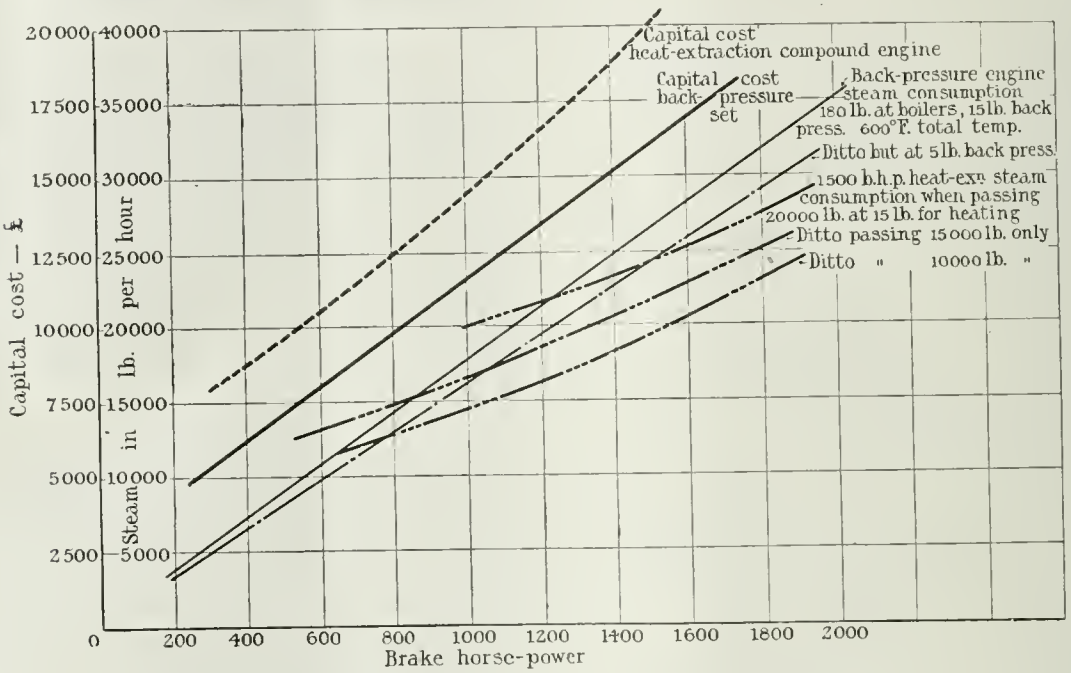


Fig. 3.—Steam consumption and capital cost of horizontal back-pressure and heat-extraction steam engines.

mill engine. As in type (1) it lends itself to either a direct-coupled or rope main drive, the dynamo being rope driven. The slower-running set entails more outlay in foundations and buildings, but this type of engine is just as reliable as any typical condensing mill engine, and preferred by some paper makers for that reason. For one or two machine mills requiring 400/1 000 h.p. this system has worked very well in actual practice. Fig. 3 is a curve of capital cost and steam consumption at 5 and 15 lb. gauge back pressure.

(3) *Back-pressure steam turbine.*—Any turbine is better than the reciprocating engine as regards freedom from oil in heating steam, but a back-pressure turbine otherwise has no advantages to justify its instalment, except perhaps in conjunction with another condensing heat-extraction prime mover.

The principal drawback to all these alternatives is

that if the proportion of power and heating requirements be not accurately forecasted, supplementary heating by live steam by-pass, or the blowing to waste of surplus back-pressure steam is necessary. While with steady draw requirements of paper drying and roof heating this should be attainable, on the other hand it is bound to fail in connection with intermittent draws of heating requirement, as in grass digestion.

(4) *Heat-extraction horizontal compound condensing steam engine.*—This comprises a drop-valve high-pressure cylinder of more or less orthodox type, a receiver from which the heating steam is tapped at any predetermined pressure, and a uniflow low-pressure cylinder to attain the utmost power from the condenser. It fears it has too great a rival in the bleeding turbine ever to have much success.

(5) *Heat-extraction or bleeding condensing turbines.*—For those mills where the demand for heating steam fluctuates, and particularly where it is required at different pressures, such as esparto mills, this system is undoubtedly the best.

The steam, containing no oil, is perfect as a direct or indirect heating medium.

The steam consumption depends, of course, on the quantity and pressure of draw-off. Fig. 4 gives typical curves for a 1 000-kW unit with different pressures and quantities of steam for heating.

The means of driving do, however, justify further

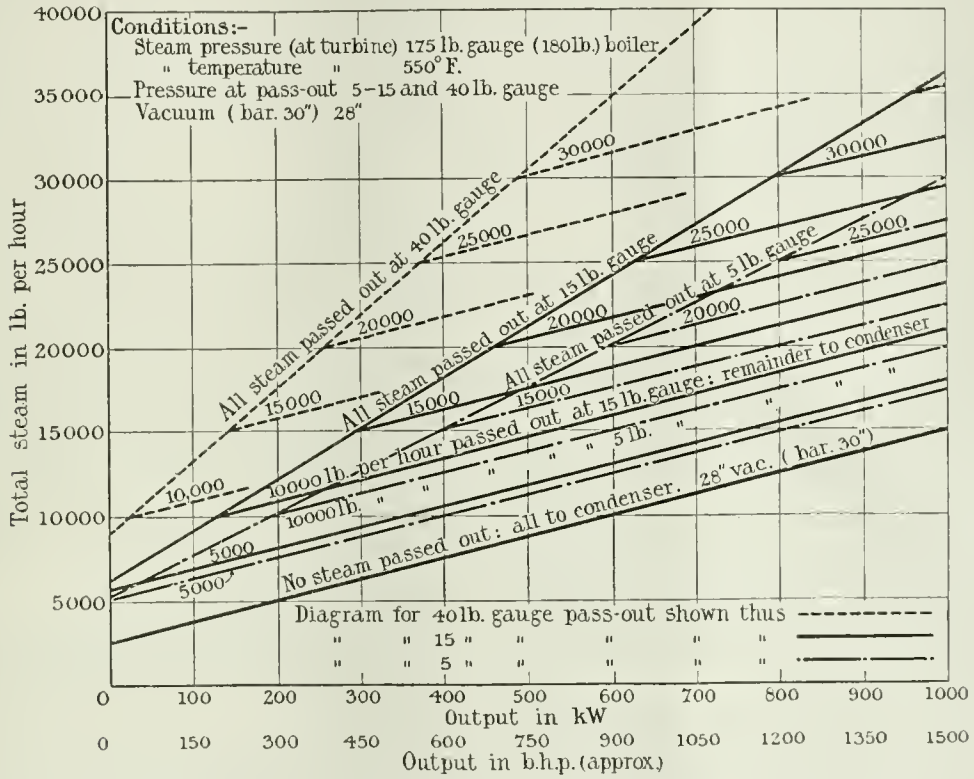

Conditions:-
Steam pressure (at turbine) 175 lb. gauge (180 lb.) boiler
" temperature " 550° F.
Pressure at pass-out 5–15 and 40 lb. gauge
Vacuum (bar. 30") 28"
All steam passed out at 40 lb. gauge
All steam passed out at 15 lb. gauge
All steam passed out at 5 lb. gauge
10000 lb. per hour passed out at 15 lb. gauge; remainder to condenser
" " " " 5 lb. " " " "
No steam passed out; all to condenser. 28" vac. (bar. 30")
10000 lb. " " " "
Diagram for 40 lb. gauge pass-out shown thus
" " 15 " " " " " "
" " 5 " " " " " "
Total steam in lb. per hour
Output in kW
Output in b.h.p. (approx.)

Fig. 4.—Steam-consumption diagram for 1 000-kW geared pass-out turbo-generator.

lends itself to direct drive as before, and provides a means of obtaining the extra power required for preparation and beating without an overplus of heating steam being provided. Such an engine, therefore, would lend itself to the requirements of a rag or fine mill. It does not, however, readily provide for bleeding at different pressures simultaneously, and has not as yet been installed to any great degree in this country. Its reliability would be just as good as that of types (1) or (2), and a direct shaft drive, without the cost of the electrical equipment, would help it.

Fig. 3 gives the approximate capital cost of such engines up to 1 500 b.h.p. and the steam consumption of a 1 500-b.h.p. engine at different "pass outs."

Whilst it is a most interesting proposition, the author consideration. If we are to get the best results from the steam, a turbine speed (depending on type) of 3 000 to 7 000 r.p.m. is essential.

The following are four possible drives:—

(*a*) Through single or double reducing gear, and ropes, or direct to shaft.
(*b*) Direct coupled to alternator.
(*c*) Through reducing gear to drive alternator.
(*d*) Through reducing gear to drive direct-current dynamo.

System (*a*) gives the cheapest capital outlay and, provided a straight main drive is available, more economical running due to cutting out electric losses. There is, however, the question of reliability. Is one

turbo set sufficiently reliable (without a stand-by) to run a paper mill for 132 hours every week, without any interruption of running time either due to turbine gear or drive ?

The author's view is that while no advantage is to be gained by the driving of beaters and refiner electrically, and while there is certainly a considerably greater first cost incurred, the better way to install turbines in paper mills is in conjunction with complete mill electrification, including beaters and refiners. Then, even if no stand-by be installed at the time, it can always be added at a later date without scrapping any of the lay-out ; further, it enables possible mill development to be better provided for.

As regards the three alternatives (*b*), (*c*) or (*d*), each has its known good points ; the direct coupled alternator cuts out the gear, at the expense of possibly lower turbine efficiency and possible electrical trouble on such a high-speed unit.

The perfection to which geared turbines have now been developed, the undoubted advantages obtained by running the turbine at the most economical steam speed, and the use of standard designed medium-speed alternators or dynamos are, the author considers, more than sufficient to justify the extra expense incurred in such a combination.

(6) *Utilization of existing prime movers on heat-economy lines.*—The question of capital outlay, if nothing else, makes the consideration of any scheme whereby the plant standing at a low value in the books can be utilized on these lines, of first importance.

There are, broadly speaking, two ways :—

(*a*) The generally accepted principle of putting the present condensing engines on to a back pressure, letting them drive what they will of the main drive, and utilizing all surplus steam other than that taken for heating purposes for driving a low- or mixed-pressure turbine. This turbine would provide electric power for the driving of the paper machines, and all other electrically driven plant, and such drives as would have to be taken off the main engine due to its running under back pressure.

(*b*) The other alternative would be to let the main engine run entirely on back pressure, utilizing all its steam for heating purposes and supplying the extra power required by means of direct-coupled high-speed or other set working condensing.

Propositions on these lines show up very well in capital outlay, but the condition of the existing plant must be most carefully gone into before making a decision, particularly if it is a question of adding the turbine to work in conjunction with the existing plant, which may be approaching the end of its useful life.

System of Supply.

In practically all early mill installations direct current was installed so as to obtain the speed range necessary for many of the drives. As paper mills are fairly compact lay-outs transmission difficulties do not arise, 220 volts being mainly used.

The modern direct-current motor being such a reliable article, the author considers that, if beating and refining plant be not electrically driven, direct current should still be chosen. Where, however, the whole mill is to be electrified, alternating current should be selected, particularly if the commutator motor substantiates its present promise.

In deciding an alternating-current system the question of frequency immediately arises. Generally speaking, 50 periods has now been standardized throughout the country for three-phase power equipments and, without material reasons for departure from such a standard, it would appear unwise to adopt in the paper mill any other frequency, particularly if there is any prospect of being able to get a supplementary or stand-by supply from an outside source.

The periodicity of 25, however, does exceedingly well for paper-mill drive. In the matter of millwrighting it enables direct-coupled motors to be installed to suit the low-speed drives common to the paper mill, without the first cost being abnormal. It also has advantages in keeping up both efficiency and power factor, thereby saving both in capital outlay for the generating plant and the wiring system.

Electric Driving of the Paper Machine.

The reasons for converting the paper machine from individual engine drive to electrical have already been stated.

The paper machine is really a group of machines interlinked, forming a continuous process in the manufacture of the paper sheet. A part, known as the "wet end," runs at constant speed and practically constant load for all callipers of paper made by the machine. The other, called the "dry end," has to vary with the calliper of sheet made.

The shafts to be driven, taking the Fourdrinier machine, are :—

One couch or wire.
One to three presses.
Two or three sets of drying cylinders.
Possibly a set of intermediate or smoothing rolls.
One to four stacks of calenders.
Reeling frame.

In other words, from 5 to 13 shafts, depending on the class and output of paper to be made. These various shafts are coupled together with belts or ropes.

As the web shrinks in the process of drying, it is necessary to have very fine means of regulation between the various sections. In early machines the regulation was effected by adding pieces of felt, called "bulking," to the pulley rims to increase the diameter. Modern practice has been to use either expanding rope pulleys or, which is more usual, taper belt pulleys with fine-threaded belt shifters, to run the belt in any desired position.

Horse-power Required.

Wet ends vary from 10 h.p. on narrow low-speed machines up to 350 to 400 h.p. on the latest "news" machines with a refiner on the back shaft.

Dry ends vary from 20 h.p. up to 350 to 500 h.p. on fast "news" machines.

Requirements for Successful Drive.

(1) To give the speed-range essential for the paper maker to manufacture the various callipers and qualities of sheet. The variable high-speed steam engine has a working range of 9 to 1, which has satisfied all requirements without the need of any change-speed gears, except on machines where an exceptional range of "makes" is required.

In a considerable proportion of cases a range of 3 to 1 for actual paper-making is ample; where extreme ranges are called for, the paper maker can often so arrange his "makes" that a change-speed gear will give the effective range without an abnormal and correspondingly costly motor equipment. A 3 to 1 shunt control, say, can give 30 to 90 ft., and 80 to 240 ft. per min., a total range (allowing a working overlap) of 8 to 1 with a simple shunt-wound motor. Such a change gear is effectively provided by a belt drive, the pulley ratios being so calculated that the one belt can be used for either fast or slow drive.

(2) To run the machine continuously at low speeds for warming up, washing felts, dryer scraping, trueing up of presses, and possibly buffing of rolls. Generally speaking, it will be found that the conditions are more severe in this respect in the one- or two-machined old-fashioned mills. In the larger and more modern lay-outs, the universal practice nowadays is to do all roll buffing out of the paper machine on a special buffing machine.

(3) To provide a satisfactory start-up from cold.

(4) To give the minimum increase of speed at any point to point on the control. The paper maker attaches the greatest importance to this factor; a jump from, say, 200 ft. to 220 ft. might necessitate making that particular order at 200 ft., as 220 ft. per min. would be too fast, whereas at 210 ft. the same sheet could have been turned out and 5 per cent more output obtained.

(5) The advances in speed from point to point should be as equal as possible over the whole range.

(6) The speed must remain constant. A slowing down or speeding up of machines after the proper speed has been obtained causes overweight or underweight of finished paper.

(7) The method of control must be as simple as possible; the nearer an approach is made to the old variable cone pulley on the variable-speed machine engine, where the machinist has simply one hand-wheel to turn, the better.

(8) Absolute reliability of action, which obviously includes not only motors, but every detail of the equipment from supply to machine driving shaft.

(9) If by the adoption of the multi-motor drive for the dry end, a reduction on the amount of "broke" can be guaranteed, it is a very appealing argument to the paper makers.

(10) The point of control to be preferably where the machinist has a full view of the running machine.

Methods of Drive.

The driving of the "dry end" can be either :—

Single motor: Taking the place of the engine, and retaining the present taper belt pulleys. It may be one of the following alternatives :—

(*a*) Shunt and series control, d.c. motor, from mill supply.
(*b*) Shunt control, d.c. motor, from mill supply.
(*c*) Potentiometer control, d.c. motor, from a separate dynamo.
(*d*) Potentiometer and shunt control from a separate dynamo.
(*e*) Ward-Leonard control from mill supply.
(*f*) Booster control from mill supply.
(*g*) Alternating-current commutator motor from mill supply.
(*h*) Direct- or alternating-current constant-speed motor with variable oil gear.

Multi-motor: A motor being connected to each of the shafts from couch to reel, thus dispensing with the taper pulleys and clutches.

(*a*) Direct-current motors from mill supply with either a mechanical or an electrical interlock between each section of the complete drives.
(*b*) Alternating-current commutator motors, with or without an interlock between each draw.
(*c*) Constant-speed a.c. or d.c. motors with variable oil transmission.

Consideration of Alternative Systems.

(*a*) *Shunt and series control.*—The earliest equipments used were on these lines; the variations in speed on series control were the cause of endless complaints, and undoubtedly much of the prejudice existing in the older school of paper makers to-day to the electric drive arises from bitter experiences with these early equipments.

(*b*) *Shunt control.*—This simple system, provided that a range of not more than 3 or $3\frac{1}{2}$ to 1 is required, is excellent. The usual drum-type control gear is used with series resistances amply rated to allow for slow running when warming up; on the barrel, say, 44 shunt points are provided for the 3 to 1 range; the motor is protected by the usual control pillar with circuit breakers interlocked with the controller barrel. By providing an auxiliary 15-point shunt regulator on the control panel proportioned so that its total resistance is equal to that between any set of points on the controller barrel, 660 shunt-control running speeds are possible. The author has used this device for some years in paper-mill work, with excellent results.

(*c*) *Potentiometer control from a separate dynamo.*—By this control a stable speed-range of 6 to 1 or 7 to 1 is attainable. The supply is from a separate dynamo driven preferably by ropes from the main power unit, thereby cutting out the motor-generator losses of the Ward-Leonard system. The whole main control is by one hand-wheel, giving 50 speeds, with the addition of the auxiliary fine-regulator controller of, say, 15 points, a total number of steps of, say, 750. This system has proved very reliable in practice.

(*d*) *Potentiometer and shunt control* (*see Fig.* 5).—A combination of the systems (*b*) and (*c*), which will give a speed range of 10 or 12 to 1. It will be noted that the one hand-wheel provides for the first part of its range 36 points on potentiometer control, and at

the higher speeds 15 points by shunt control. The 15-point fine-regulator device is again used, giving a total of 765 steps in speed. The simplicity of this lay-out,

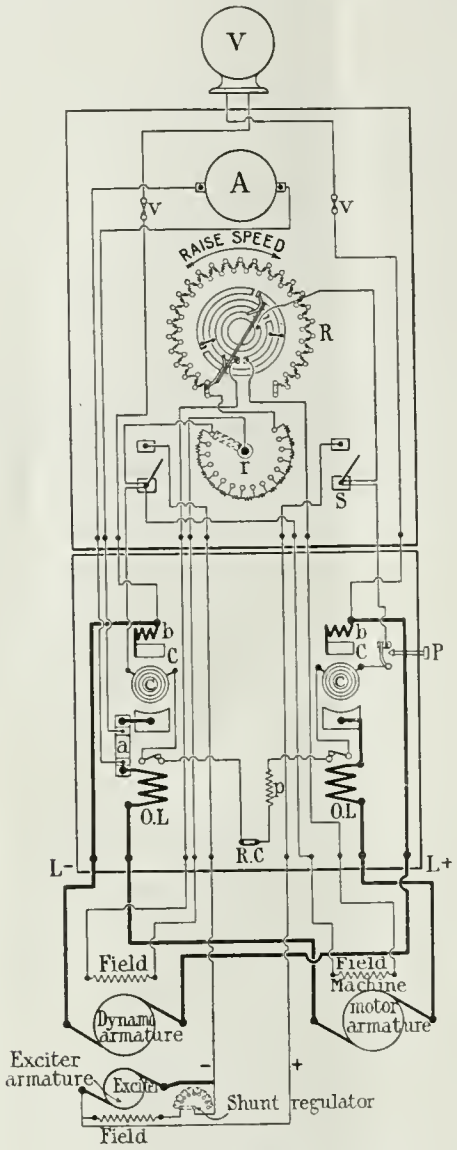


FIG. 5.—Combined potentiometer and shunt control switchgear for paper machine.

A Ammeter.
a Ammeter shunt.
b Blow-out coils.
C Hand-operated circuit breaker, mechanically interlocked with R so that circuit breaker can only be closed when R is in the "minimum voltage" position.
c Circuit-breaker coils.
OL Overload release.
P Push-button.
p Permanent resistance for C.
R Combined potentiometer and shunt regulator.
r Auxiliary potentiometer regulator.
S Field break switch.
V Voltmeter.
v Voltmeter fuses.
RC Link for remote control press-button if required.

together with the large range of speed possible by it, are particularly noticeable.

(*e*) *Ward-Leonard system.*—The driving of the separ-

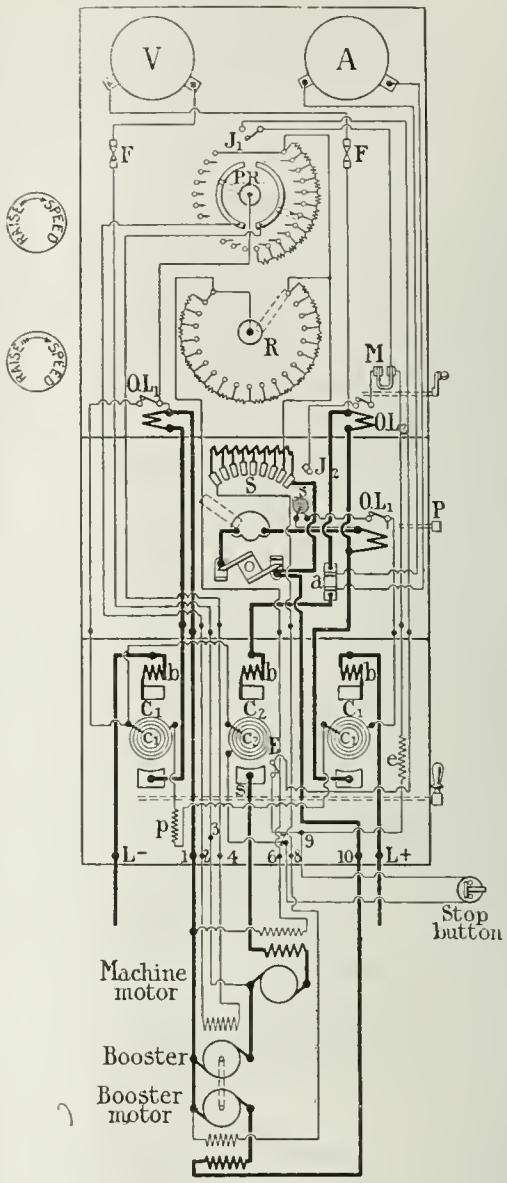


FIG. 6.—Booster-controlled paper-machine switchgear.

A Ammeter.
a Ammeter shunt.
b Blow-out coils.
C1 Hand-operated circuit breaker mechanically interlocked with S and electrically interlocked with C2.
C2 Electrically-operated circuit breaker mechanically interlocked with S, PR and C1.
c1 Circuit-breaker coil.
c2 Circuit-breaker coil.
E Economy switch, closed when C2 is open.
e Economy resistance.
F Fuse.
J1 Adjustable interlocking switch closed when PR is in position corresponding to negative boost equal to line voltage.
J2 Interlocking contact (makes circuit of C2 when S is full on).
M Master switch with re-setting device.
OL1 Overload release (plain).
OL2 Overload release with reset.
P Push-button release for C1.
p Permanent resistance.
PR Reversing potentiometer regulator for booster field.
R Shunt regulator for machine motor, mechanically interlocked with PR.
S Starter mechanically interlocked with C1 and electrically interlocked with C2.
s Starter "no-volt" coil.
V Voltmeter.

ate generator and exciter for each machine by a motor from the main supply, instead of mechanically by belt or ropes from the engine, converts system (*d*) into what is termed the Ward-Leonard system. Such a system can obviously work on either alternating or direct-current supplies. Its drawbacks are the extra capital outlay incurred and the running losses due to the motor-generator.

(*f*) *Booster control* (*see Fig.* 6).—This system has been developed to overcome the drawbacks of the Ward-Leonard control. Provided the main supply is direct current and the voltage of the main supply does not exceed, say, 250 volts, it is satisfactory. The main motor is wound for double the mill supply voltage, and the speed-range then obtained by the variation of boost either up or down on the booster, which is only half the size of the Ward-Leonard booster, thereby saving in both capital and running costs. The range of speed may be up to 12 to 1. The control, it will

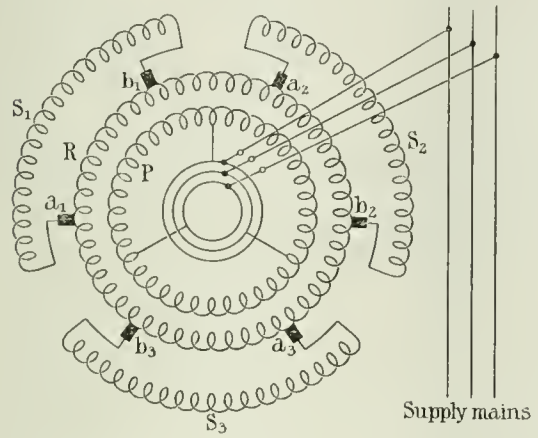


FIG. 7.—Diagram of connections of an a.c. commutator motor.

S_1, S_2, S_3 = Windings in stator.
R = D.C. winding in rotor.
P = A.C. winding at supply voltage in rotor.
a_1, a_2, a_3 = Brushes on one arm.
b_1, b_2, b_3 = Brushes on other arm.

be noted, is by two hand-wheels, one for the booster field, and the other for the machine motor field; the use of two 25-point regulators will give 625 running speeds.

(*g*) *Alternating-current commutator motors.*—Variable-speed a.c. commutator motors have for several years been on the market with series characteristics, but only during the past few years has a practical machine with a shunt characteristic been developed by one or two makers, such as the British Thomson-Houston Company in this country, and the Allmanna Swenska Elektriska Aktiebolaget, Sweden.

These commutator motors provide a speed range of 3 to 1 with efficiency and power factor little less, if any, than those of the ordinary induction motors. As this speed-range can be increased by series control to 6 or 10 to 1 for starting purposes, it gives the motor all the advantages attained by the d.c. shunt-control motor, with one great improvement, i.e. the speed-range being attained by moving the position of the brushes on the commutator the variations in speed are gradual; it is simply a question of having a slow-moving gear attached to the brush arm to obtain the most perfect degree of fine regulation.

The principle can be readily followed from Fig. 7. The rotor is double wound, one winding P being

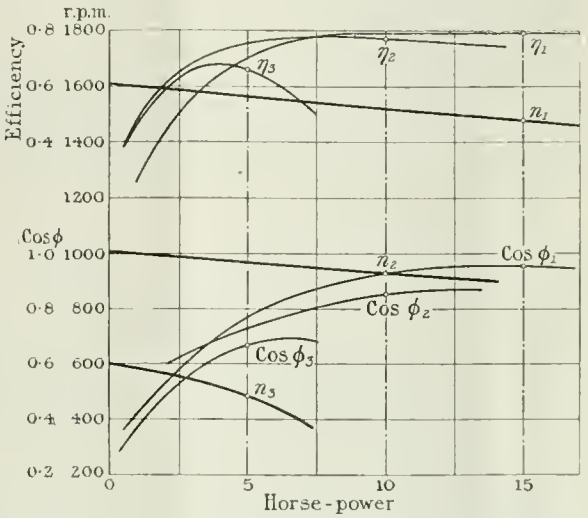


FIG. 8.—Load characteristics for three brush-positions on a three-phase commutator motor.

connected through the slip-rings on to the supply mains; the other winding R is an ordinary d.c. winding with a commutator. The windings on the stator, S_1, S_2, S_3 are coupled to the brushes a_1 b_1, a_2 b_2 and a_3 b_3. The regulation of speed by moving the brushes can be performed at the motor; either mechanically

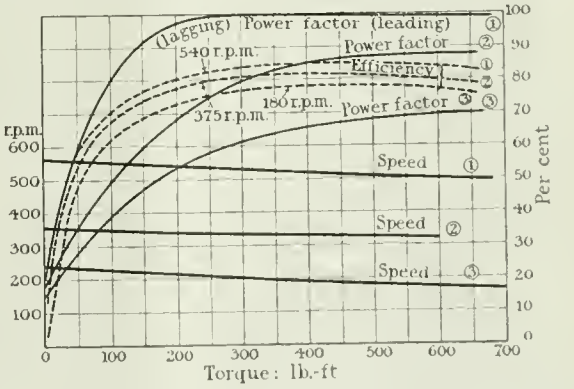


FIG. 9.—Power factor, speed and efficiency curves of a three-phase B.T.H. a.c. commutator motor at 400 volts, 50 ∼, 180 to 540 r.p.m.

from a distance by means of rods or chain, or electrically through a small motor and gear. The brushes a_1, a_2 and a_3 are all moved together, similarly b_1, b_2 and b_3.

If the brushes a_1, a_2, a_3 and b_1, b_2 and b_3 are on the same bar, the motor runs as an ordinary induction motor.

The load characteristics of a 15 to 5 h.p. (Swedish

Electric) motor are given in Fig. 8. Considering the relatively small size of the machine referred to, the results are very good.

Similar curves for a larger machine (B.T.H.) of 50 h.p. are plotted in Fig. 9. The high efficiency and power factor, the latter especially at the above synchronous speeds, are worthy of note. Readings are given for three speeds, but any number of speeds could be obtained by altering the brush position.

There are, undoubtedly, great possibilities before this type of motor in the paper mill, not only for the machine drive, but also for all other duties requiring variable speed, such as calenders, cutters, etc. It has already been successfully applied to machine drives in Scandinavia and this country; and several others

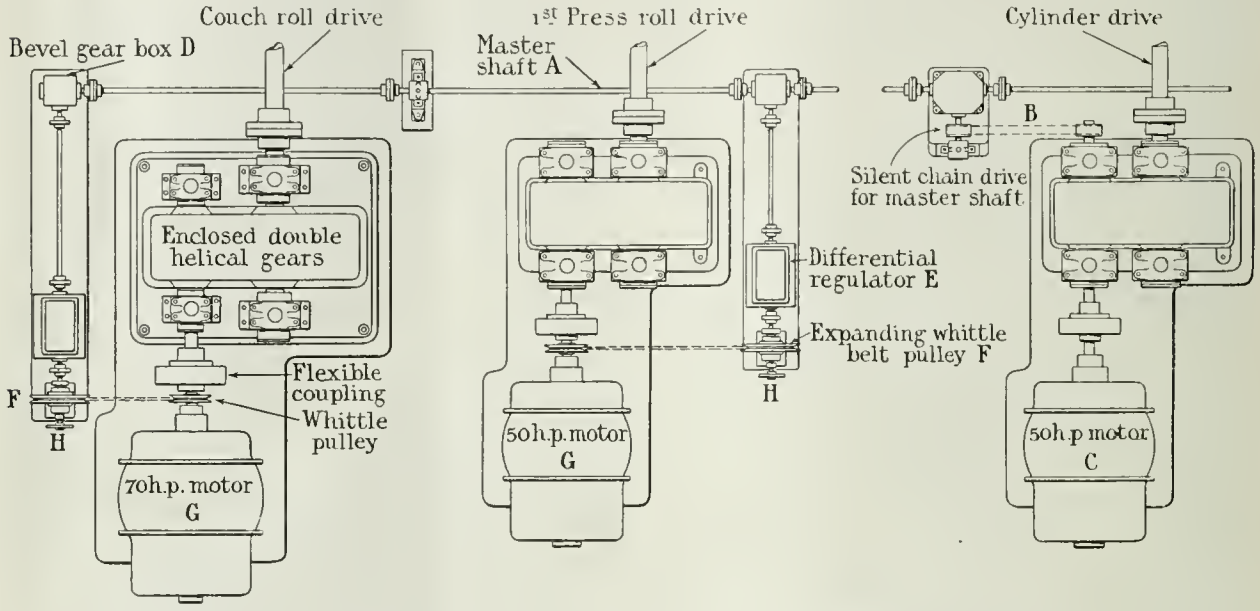


FIG. 10.—Diagram of the Harland patent speed interlock for 140-in. paper machine, at 300 to 700 ft. per min.

are on order, both for single- and multi-motor drives.

The possible objections to the system are :—

(*a*) The double-wound rotor is a complication which may eventually result in failures, due to continuous running and consequent lack of time for systematic cleaning.

(*b*) The commutator necessarily becomes rather large in diameter, and at high speeds may not prove as good in commutation as the present-day d.c. machine.

(*c*) The first cost of the motor will be higher than in the d.c. shunt system, though probably little, if any, dearer than the complete Ward-Leonard system.

(*d*) It has not yet had the extended trials on which opinions of d.c. systems are based.

Its other advantages, however, make this type of motor worthy of very careful consideration for a machine drive where a three-phase supply is installed, and particularly those where a maximum running speed-range of 3 to 1 is all that is desired.

(*h*) *Variable oil gear.*—Whilst not an electrical device, the possibilities of a variable-speed drive, such as the Williams-Janney oil gear provides, in conjunction with a constant-speed motor, or driven off the main shaft, must be mentioned.

This oil gear gives a gradual rise in speed and, provided proper cooling of the oil is arranged, the speed keeps absolutely steady. The sole control is by one small hand-wheel. It has not yet been used for a machine drive in this country so far as the author has been able to ascertain, but there is no reason why it should not prove quite effective. The possible objections are the losses in efficiency in the oil pump and oil motor, amounting to about 20 per cent, and the possibility of trouble arising in time due to wear of the working parts when running constantly night and day.

MULTI-MOTOR SYSTEMS.

(1) *Direct-current motors, with shunt control.*—This system was first tried some 20 years ago, but considerable difficulties were found in keeping the various sections of the machine in proper relation to each other at different speeds, motor temperatures, etc. Any multi-motor system, to be satisfactory, must provide absolutely constant speeds between the various "draws," otherwise excessive "broke" will result. Mr. R. Harper, a leading Scottish paper-mill engineer, has spent many years in investigating and experimenting on this subject.

(2) *Direct-current systems with interlocks.*—There are several systems now on the market, such as :—

Harland patent speed interlock.
General Electric Co. (U.S.A.).
American Westinghouse.
Metropolitan-Vickers.
British Thomson-Houston

The Harland system is a combination of mechanical differential and electrical interlocks. Fig. 10 gives details of three units of such a drive. The lay or master shaft A runs the full length of the machine, being driven by means of the chain B from the cylinder-drive motor C, its speed therefore varying with the speed of the drying cylinders.

For every other section of the machine to be driven in step with the dryers there are separate drives, Fig. 10 just showing the couch or wire drive, and the first press drive. Each of these drives comprises a set of enclosed bevel gears D driving a small shaft terminating in one half of the differential regulator E. The other side of the differential is driven by the expanding Whittle belt pulley F from a small pulley on the motor shaft by a standard Whittle belt, which operates only the shunt regulator.

The differential operates a shunt regulator, the blade being connected to the housing of the differential. This regulator controls the adjoining section motor G. If the lay shaft A and the motor G are not exactly in step the differential will revolve clockwise or anti-clockwise, and will operate on the shunt field of the motor G, raising or lowering its speed until the synchronous speed is attained.

The expanding Whittle belt pulleys F enable variation to be made by the machinist in the "draw" between individual sections. This can be done either at the back of the machine, by means of hand-wheel H as shown, which is the equivalent of present-day practice with taper pulleys and belt shifters or, if desired, operating shafts could be carried through to the front of the machine by simple gearing.

The speed of the machine as a whole is regulated from a control board placed in a central position, which affects all the motors simultaneously.

The simplicity by which the whole machine or any individual section can be varied in speed will be obvious from this brief description.

An experimental Harland equipment has been in operation in this country for over five years at speeds up to 250 ft. per min., and in January last at Laurentide, Canada, a large fast-running machine was started, which is running at over 800 ft. per min. very satisfactorily. The actual running results prove that this system can give all that is required by the paper maker for the drive. There is a slight tendency to hunt on the experimental equipment, due doubtless to backlash in the interlock, but it does not appear to be detrimental to the manufacture of the sheet.

The General Electric Co. (U.S.A.) system has been fitted to a fast-running news machine at the Crown Willamette Paper Co., West Lynne, Oregon, which started up about November 1920, and is running at a speed of 600 to 700 ft. per min. It is purely an electrical system; a d.c. motor and a synchronous a.c. motor driven by taper cone pulleys are applied to each section shaft, the function of the synchronous motors being to run in step with the master motor and keep the sections relatively in step, the section draws being attained by the taper pulleys. All the means for keeping the various sections in correct step are electrical.

At the Mead Paper and Pulp Co., Chillicothe, two 154-inch machines are now being installed with this system, the particulars being:—

Speed range, 80 to 500 ft. per minute.
Estimated output per 24 hours, 80 000 lb.

	D.C. Motor	A.C. Synchronous Compensator
	h.p.	h.p.
Wire	50	10
3 presses each	35	10
2 section dryers each ..	35	10
2 " Stacks " calenders each..	50	10
Reel and rewinder	20	—

A total of 345 h.p. in nine d.c. motors, and 80 h.p. in eight a.c. motors per machine (see *World's Paper Trade Review* for 21 January, 1921.)

The American Westinghouse have a machine running on their system at Gould Paper Mills, Lyon Falls, N.Y., which was started in November 1919; this also is a purely electrical system, being a combination of d.c. and a.c. interlocks.

The Metropolitan-Vickers Co. have recently developed a system which, in place of the longitudinal lay shaft and mechanical differential of the Harland system, has an electrical differential. It is claimed for this system that hunting can be entirely eliminated. There are not yet any systems in operation.

The British Thomson-Houston also has perfected a control which appears to have good possibilities, but which has not yet been actually tried on a paper machine. A particular advantage of the system is that it is applicable to either d.c. or a.c. commutator motors.

(3) *Alternating-current commutator motor.*—This type, providing as it does for speed-changing without jumps, will suit very well for a multi-motor drive. The adjustment between draws when once the motors have warmed up should be fairly simple. So far, the author cannot find any trace of machines so driven, although some are now under construction.

(4) *Variable oil gear.*—The driving of each shaft of the machine by a constant-speed (say, squirrel-cage) motor through an oil gear is also perfectly feasible. When the oil is at a constant temperature there will be no change of speed, provided the motor speed remains constant. The adjusting of the draws between the drives would be as simple as with the present taper belt pulleys. By fitting the same gears throughout, one spare unit would provide against gear trouble.

Choice and Scope for Machine Drives.

Table 5, which has been prepared with the help of the Paper Makers' Association of Great Britain and Ireland, gives particulars of practically all machines now electrically driven in this country. It will be noticed that the total is only 21 machines. When one considers that there are over 500 paper machines in the British Isles alone, the possibilities for development

TABLE 5.

List of Electrically Driven Paper Machines in the British Isles.

Machine Width	Date Electrified	System	Wet End	Dry-end Motors		Machine Speed Range	Change Belts, etc.	Source of Supply
in.			h.p.	h.p.	r.p.m.	ft. per min.		
72	1920	D.C. shunt and series	15	25	400 to 1 200	66·6 to 200	No	Back-pressure high-speed engine
156	1912 (2)	D.C. shunt and series	—	350	250 to 660	250 to 660	No	Back-pressure high-speed engine
126	1905 (2)	Ditto	—	160	250 to 500	250 to 500	No	Ditto
80	1900	D.C.potentio-meter	—	35	730 max.	0 to 200	No	Condensing engine
80	1900	Ditto	—	2 9 to 12 1 78	730 max,	0 to 350	No	Ditto
130	1900	Ditto	—	4 18 1 156	730 max.	0 to 350	No	Condensing engine
80	1901	D.C. shunt and series	—	40	210 to 840	40 to 160	Yes	Bleeding turbine
70	1913	Ditto	—	40	210 to 840	40 to 160	Yes	Ditto
54	1919	D.C. shunt and series	10	30	350 to 700	100 to 200	No	Condensing engine
96	1915	D.C.potentio-meter	Mec'l	30	1 000 max.	8 to 125	One	Back-pressure and low-speed engine
72	1920	D.C.potentio-meter and shunt	Mec'l	30	1 000 on potentio-meter 1 500 on shunt	11 to 96	One if more range required	Ditto
63	1908	D.C. shunt and series	—	45	250 to 1 000	20 to 50 to 200	No	Condensing engine
66	1910	Ditto	—	45	250 to 1 000	20 to 50 to 200	No	Ditto
95	1910	Ditto	—	45	250 to 1 000	20 to 50 to 200	No	Ditto
72	1915	D.C. shunt and series	14	25	450 to 1 200	17 to 180	One	Condensing engine
74	1919	Ditto	14	33	450 to 1 200	17 to 180	One	Ditto
90	1905	D.C. shunt and series	24	65	560 to 1 000	40 to 200	One	Back-pressure high-speed engine
96	1912	Multi-motor potentio-meter	—	20 10 40 8	Couch, wet press, cylinders, reel	100 to 200	Proved unsatis-factory, since discarded	Separately-excited generator; 49 kW
96	1913	A.C. commutator	—	24	1 170 mean 3 to 1 range	20 to 60	Yes	Condensing engine

Note.—The horizontal lines divide the respective mills in which the machines are.

can be realized. The d.c. single-motor systems, of course, predominate in this table, as the a.c. motor has only recently been made feasible.

It is noteworthy that half the equipments now running obtain their power from a condensing prime mover, apparently using live steam for heating the dryers. This method obviously can be improved upon.

The author's opinion is that electrification of existing machines will generally be on the single-motor system, as unless the gear at the back of the machine is worn out the expense of the multi-motor system cannot be justified.

For new machines, however, the multi-motor system has the following good points :—

(1) The cost of machine and drive is little, if any, more costly, no gearing being required.
(2) With the extra-high-speed news machines now being developed, the gearing is getting almost beyond practical limits.
(3) By cutting out the gearing, a narrower machine-house will suffice, thus making a marked saving in building and foundation costs.
(4) When considering the installation of a new machine to replace an existing one, the multi-motor system will enable a wider machine (with consequent increased production) to be placed in the same machine house, due to the reduced space required for the drive.

Discussion before the North-Western Centre, at Manchester, 8 March, 1921.

Mr. F. C. Anderson : I was somewhat disappointed with the paper in so far as it deals with the driving of paper machines. It seems to show that the author is not very dissatisfied with the old drives with their clutches, cone pulleys, etc. No doubt belt drives are fairly satisfactory for many classes of work and for moderate-speed machines, but they leave much to be desired when used for larger machines at high speeds. As the author says on page 550 under "Multi-motor Systems," an essential condition of any drive is a constant "draw" between sections. I maintain that we do not get it with the old drives, or at any rate only approximately. I should like to quote an American authority who deals with the subject of belt slip which is very pertinent to this matter. Professor Sawdon claims that the tests which were carried out at Sibley College, Cornell University, are probably the most thorough tests ever taken on belt slip and creep. They cover a period of 11 months, during which 9 000 observations were taken on a machine especially constructed to measure slip and creep, etc. He says : "By taking continuous observations of slip over long intervals of time, with the machine running under constant conditions of tension, brake load and speed, data were collected and curves plotted between per cent of slip and time." The slip varied over a period of 30 minutes between 0·5 and 14 per cent on the top curve, and on the bottom curve between 1 and over 10 per cent. This authority says the curves are perfectly typical. "From these it will be seen that the slip is continually varying. By comparing a large number of such curves it was found that this variation takes place through no clearly defined cycle." Had the belt speed during test been high one might have suggested air pockets, but the conditions of test prevent any such thought ; the belt speed over all tests was low, i.e. 2 200 r.p.m. A particular test was made on a load increase of 20 per cent. The belt was supposed to be normally loaded after the increase, and the tension ratio was 3·57 per cent. The slip had changed from 2·26 to 3·29 per cent, i.e. more than 1 per cent. If this were applied to two sections of a paper machine between which the "draw" is set at 1 per cent or possibly $\frac{1}{2}$ per cent, a 20 per cent change of load takes place, caused by additional weights or increased friction ; we find that the slip has changed on the section by 1 per cent, but as the "draw" was only 1 per cent this figure has doubled, or in the case of the "draw" set at $\frac{1}{2}$ per cent the change would be 200 per cent. Cone pulleys are extensively employed on paper machines. The velocity of the large diameter is not the same as that of the small diameter. With which velocity does the belt agree ? Friction losses must be greater, whilst the points above referred to are common to any flat-pulley drive and are clearly greatly aggravated in the case of a cone pulley. A paper maker stated some time ago that his machine was driven by a steam engine ; subsequently, he changed it to a single-motor drive, retaining the drive otherwise as before, and as a result he calculated that he could quote 15 per cent less on a specification. The paper machine can now be a positive instrument in the hands of the operator, as with the Harland interlock constant "draw" can be maintained absolute. This should result in better paper or more production. The author mentions "hunting." There was a little hunting on the experimental machine, due to play in the bearings and slackness and wear in the pinion teeth. The play was such that it was extraordinary that satisfactory paper could be made under these conditions, and it is a point in favour of the regulator that it succeeded in maintaining a control sufficiently accurate to make paper ; but under these conditions it is not surprising that it was difficult to make up for the play above referred to. There is no difficulty whatever in arranging a direct-current control which will be free from objectionable jumps when the speed of the machine is varied. The early efforts with individual motor-drives showed more courage and enthusiasm than knowledge, and I have little hesitation in saying that by applying a multi-motor drive without any control to a paper machine, the engineer was making the speed regulation worse than with a belt drive. Interlock control reflects any fluctuations, not in revolutions but in degrees of angular displacement, and it ensures that the drive will make the same number of revolutions per second, and the synchronism between the master and any one section is absolute. Sections can be started up one at a time and interlocked. The Harland interlock control is available for either direct-current or alternating-current drives. The Laurentide machine running

at 600 feet per minute takes 1·26 b.h.p. per inch width. From data collected from a considerable number of single-motor drives on news-print, the average horse-power of these machines with mechanical transmission between sections was 1·97 per inch width. The difference of 36 per cent in favour of the Laurentide machine seems very creditable, and is the result of good workmanship and design on the paper machine, combined with the sectional drive. Some portions of the paper trade already recognize that very large high-speed machines must be driven sectionally, and it is fully appreciated by the Laurentide engineers that their mammoth machines could never have been worked by the old methods at the speeds at which they aim. The bank of drying cylinders alone weighs 650 tons, and takes the equivalent of 1 000-h.p. torque to start from rest, although the running load falls far below this figure. The facilities and ease of control by push buttons to "inch" drying cylinders and presses when washing felts, are such that they need to be experienced to be appreciated. I maintain that the economic advantages of control, production, and the ability to increase working speeds, are sufficient to justify and cause many paper-mill owners to convert existing drives to sectional drives. The ability to increase speeds inch by inch until trouble is experienced will mean increased output, and when it is realized that these advantages are within their reach without loss of thermal efficiency, paper-mill owners will adopt the method in steadily increasing numbers. Such developments as these do much to assist the electrical trade at a time like the present.

Mr. W. Adamson: The paper machine has now been developed to such an extent both in width and speed that the usual mechanical drive has become opposed by a multi-motor electric drive. The introduction of the multi-motor drive, however, has been so recent that I do not think anybody can say that it will take the place of all mechanical drives on paper machines. It must not be forgotten that the largest machines in the world (built by my own company) are driven by the mechanical drive, and only to-day we heard that one of these mammoth machines which has been working since 1915 making paper with a trim width of 189 inches at a speed of over 700 feet per minute and taking under 350 b.h.p. to produce over 80 short tons of news-print paper per 24 hours has an average break of only one per day. The multi-motor electric drive will have difficulty in improving on this, as one break per day with the above output is a very fine achievement for any drive, especially when one considers the poor quality of the news-print paper which consists almost entirely of mechanical wood. A sheet of the average newspaper will split with the slightest pull. If this paper were wetted it would not be necessary to pull it—it would come away without pulling—and yet the mechanical drive can meet the conditions with only one break per day. In some cases the multi-motor electrical drive may be the only proposition owing to the high power required by some of the fast-running machines—say those running at 1 000 feet per minute and over. Some of these were designed and built by American engineers during the war, and in one particular case a machine about 150 inches wide is taking almost 1 000 h.p. We anticipate, however, that machines of British manufacture at the same speed will take considerably less power—probably 25 per cent less, this being gained partly through design and partly through more accurate workmanship. Such speeds are getting almost beyond mechanical production in the usual way and it is this field that the multi-motor drive will exploit with the greatest success.

Mr. A. L. Boyle: I have only been connected with two paper mills. One was a new mill, and in the other we are simply converting old methods to new. We have installed a pass-out turbine from which we obtain wonderful results. In fact, we have reduced the coal consumption 50 per cent. We are satisfied that there is something to be said for the individual drive, and we are installing it on one of our machines at the present time.

Mr. C. R. Kemp: The author refers to the paper read by Mr. Adamson recently on "The Economic Utilization of Heat and Power in Paper Mills." Some of the first part of the present paper is covered by Mr. Adamson in greater detail, and the latter goes a stage further in that he gives comparative consumptions for a mechanically driven and an electrically driven mill, whereas the author contents himself with giving steam consumptions of engines and turbines. The consumption figures which Mr. Adamson gives for the electrically driven mill (with single-motor drives) are too high; as a matter of fact, they compare very favourably with those for the mechanically driven mill. With sectionalized electric driving the figures for the electrical case can be further materially improved. I quite agree with the author that paper machines should be electrically driven, but I should like to see the argument developed a little more. It is not only for the sake of heat economy that paper machines should be electrically driven, but also because the electrical drive can be made infinitely better than the mechanical. In Fig. 1 a curve of steam consumption for a simple-expansion paper-machine engine is given, and the author states that such engines are invariably used and work with a consumption of 35 lb. of steam per i.h.p.-hour. Therein lies one of the main reasons why paper mills have excessive coal bills. The paper maker's argument is: "I use my engine as a reducing valve and get my power for nothing." Certainly the back-pressure principle is adopted, but only in a very unscientific manner. How much better it would be to generate power by a central back-pressure unit, i.e. a compound engine or steam turbine instead of by individual simple-expansion units. There would then be no surplus steam and there would be surplus power which could be used to drive other machinery in the mill, all of which would be obtained for practically nothing on the back-pressure principle. Mr. Adamson in his paper mentioned the use of compound engines in place of simple-expansion engines for driving paper machines, and with such engines the curve in Fig. 1 would lie completely under the horizontal line. It is only fair to the steam engine to mention this point, although individual variable-speed compound engines cannot often be used, on account of low boiler-pressure. The author mentions that pass-out turbines

can be used for bleeding at different pressures. This is a fallacy, as the double tap-off has no advantage over the single tap-off plus direct heating, for when all the steam is tapped off from a single tap-off turbine all the available heat is used and there is no object in having another tap higher up. A tap-off scheme should be arranged so that the tap-off pressure is as low as possible and all the heating at the higher pressure is done by live steam through a reducing valve. Pass-out turbines have the great advantage that they accommodate themselves well to independent fluctuations of heat and power requirements, and such fluctuations must be taken into consideration in every mill. I do not think the author has laid sufficient emphasis on this point in the paper. There are two classes of electric driving for use with paper machines, i.e. the single-motor drive and the multi-motor drive, and I think the author could with advantage have discussed the relative merits and demerits of the two systems. He confines himself to stating that, generally speaking, the single-motor drive is the one to use for existing machines and that the multi-motor drive has certain good points for new machines. He gives a short description of various kinds of drives, single-motor and multi-motor, but does not mention the obvious drawbacks of the mechanical drive between the single motor and the various sections of the machine. The mechanical drive occasions great loss of power and takes up a large amount of space for the accommodation of line shafts, pulleys, belts, reduction gear units, friction clutches, etc. It carries with it the danger of fire and risk to life. It abstracts light, is dirty and clumsy, and requires constant skilled attention and maintenance. Last, but not least, it is not positive, for once the belt is in position the relative speeds of the two taper pulleys should remain constant, otherwise there is a great risk of breakage of the paper. The author himself states that if, by the adoption of the multi-motor drive, a reduction in the amount of broke can be guaranteed, it is an argument which appeals to the paper maker. This remark hints at the great objections there are to the mechanical drive, and the guarantee the author mentions can be given with the Harland interlock; in fact, with this type of multi-motor drive the entire absence of broke arising from the drive can be guaranteed. With regard to the power absorbed by the mechanical drive, it is interesting to compare the data given in Fig. 1 with information based on the Harland interlock drive mentioned by the author. From Fig. 1 we see that when the machine is running at 550 feet per minute the dry-end engine alone consumes about 12 200 lb. of steam per hour, which at 35 lb. per i.h.p.-hour means that 350 i.h.p. has to be developed by the engine. Figures taken from Interlake indicate that individual motors driving the dry end of the machine instanced by the author would need to develop about 152 b.h.p. This is sufficient to show where much of the power goes which is required to drive a paper machine having a mechanical drive. There are now two other Harland drives running in Canada, in addition to Interlake, and there will shortly be some running in Great Britain. Actual results show conclusively that everything which is claimed for the drive is well borne out. The author gives a short description of the drive in his paper, but much more can be written about it, as many interesting features have been brought to light by experience. The experimental drive to which the author refers is an old machine in which there is plenty of play in bearings and gears, and the interlock does its best to compensate for the resulting unevenness in the drive. This is probably what has induced the author to say that there is a slight tendency to hunt. There is no hunting in the ordinary sense on the Laurentide or the other Canadian machines, and, if one gives a little thought to the action of a differential gear, one will realize at once that there can be no hunting when the casing to which the regulator arm is attached is stationary. As a matter of fact, there is no movement of the arm which can be detected by the naked eye. There must be a slight "breathing" between contacts otherwise the gear would not function, but one must remember that for every degree of angular displacement of the one side of the differential relative to the other, there is half a degree of movement on the regulator arm.

Mr. F. P. Spicer: Such facts as the author has put before us must prove of assistance both to the paper maker and to the engineer, by focusing attention upon the weak spots of steam driving in the paper mill and showing where the electrical engineer may help and where he must stay his hand. Some years ago I installed electric light and power in a gas-works, and the manager one day when being reproached about "introducing the enemy" replied: "If for the particular purpose for which I am using it, electricity best meets my needs and enables me to produce better gas more cheaply, I should be acting against the best interests of the gas industry if I failed to adopt it." I consider that to be the spirit in which we as electrical engineers must approach the subject, and it is in the same spirit that we ask the paper makers to meet us. From Fig. 1 I gather the following facts. When using steam engines to drive a 130-inch news machine, the following powers are required between the limits of 200 and 550 ft. per minute:—

	lb. of steam per hour	i.h.p.	b.h.p.
Wet end ..	3 500	100	(say) 80
Dry end ..	4 000 to 12 300	114 to 350	90 to 280

Table 5 gives details of a 126-inch machine electrically driven to work between practically the same limits. The maximum power on this is 160 h.p. for the dry end – or little more than half the power with the steam drive. I have before me the tests taken in 1906 on a very similar machine, which I think may be of interest. The wet end was fitted with a 30-b.h.p. motor running at 620 r.p.m., and the dry end with a 160-b.h.p. motor running at from 250 to 400 r.p.m. The average load in the case of the wet end was 27·5 b.h.p., and the maximum 33 b.h.p. When the machine was making

paper for a daily publication it required 70 b.h.p. for the dry end when running at 250 feet per minute, and 88 b.h.p. when running at 290 feet per minute, the width of paper being 124 inches. It may not be fair to compare a concrete example with the author's more general curve, but the apparent great difference seemed to warrant the inquiry whether a further claim for electrically driving the paper machine could not be put forward.

Mr. S. J. Watson: The possibility of paper manufacturers taking their supply from a public source is not mentioned in the paper, although there are in this country to-day a number of paper mills the whole of the power requirements of which are met from a public source and, so far as I am aware, the paper manufacturers are well satisfied. Although the power may be obtained from an outside source, boilers must still be installed in order to obtain the steam required for the various heating processes which the author mentions. In more than one instance when the public supply has been utilized, the number of boilers previously used for the purpose of supplying steam for heating and power has been reduced to about one-third and found sufficient for heating purposes at a lower pressure. There is one point which I should like to make in connection with public supply, namely, that the public supply will invariably be in the form of alternating current. In some instances it may necessitate the conversion of alternating current to direct current, if a very long range of speed regulation is required. But many of the power requirements in a paper mill can be met by means of a constant-speed motor, and this will enable far closer speed regulations to be obtained than would be possible by means of a steam drive or by a direct-current motor. In one case a synchronous speed is obtained which is absolutely constant, irrespective of load, whereas with an engine drive the speed varies with the load, and with a direct-current motor the speed varies with both the load and the temperature. With regard to the variable speed for such purposes as driving the dry end of a paper machine, a very considerable speed variation is required, particularly on a machine which may be used for making different types of paper, and also for the calenders which are used subsequently. Some form of motor with a large range of speed variation is essential. A range of 3 to 1 has been mentioned, but many paper makers require more than that in order to have something in hand for special conditions. I am particularly interested in the author's remarks on the alternating-current commutator motor. Not only in connection with paper-making machinery, but also with other trades allied to the textile trades, such a motor with a good range of speed variation would help us to meet the difficulties which at present exist. The question of variable-speed alternating-current motors with rocking brush-arms came prominently forward some few years ago, but recently I have heard very little about it. Any information with regard to modern developments would, I am sure, be welcome. I should also like to know what will be its efficiency in comparison with, say, a direct-current motor of the same size and range of speed, and what will be the relative cost. These are points of considerable importance. Mention is made in the paper of multi-motor drives for the dry end of paper machines, and I am wondering how many motors it is usual to adopt under this scheme. I take it that each individual motor would still drive more than one cylinder. If the author will give a little more information on that point it will be of value. We have heard of the high speeds at which paper can now be made, and of the width of the machines in use to-day. It is of interest to know that some of these very large machines require as much as 700 h.p. each. It is a big problem to deal with individual machines requiring so much power, and the question of obtaining motors to give the best possible results becomes of greater importance. My experience leads me to think that the potentiometer method of obtaining speed regulation is one of the best, because very small speed-changes can be obtained, thus giving better results. We hear occasionally of the application of variable-speed oil-gear drives, and I understand that the efficiency is extremely good at full load, but at lower loads is very bad. If the author can give us some actual figures on this point they will be of interest.

Mr. E. E. Tasker (*communicated*): On page 540 in item (*c*) the author refers to the quantity of heat passing away with the condenser cooling water, but in some mills advantage is taken of the heat therein contained, the cooling water being pumped through the condenser and thence into a tank at about the level of the mill roof. From this tank the warm water is drawn when required for beaters, etc., being supplemented on occasions with steam when hot water is required. This question of the quantity of heat carried away by the condenser water is governed to some extent by the type of prime mover installed. For example, in the case of a steam turbine this would have a comparatively low thermodynamic efficiency in the high-pressure stages covering the range between stop-valve pressure and that of the heating system, and so far as this part of the machine is concerned the steam turbine does not operate as efficiently as the corresponding portion of a compound reciprocating engine, the high-pressure cylinder of which may have a thermodynamic efficiency as high as 80 per cent compared with 40 to 50 per cent for the turbine. It therefore follows that where the steam demand is within certain limits more of the heat contained in the live steam is utilized in the high-pressure cylinder of the compound engine, and in consequence the low-pressure cylinder will not develop so much power, and fewer heat units will be absorbed by the condenser water. Whilst upon the subject of the condenser I should like to direct attention to the usual practice of pumping or allowing to flow through the condenser the full volume of water, irrespective of the load on the steam turbine or engine. The most economical procedure is to vary the flow of water necessary to produce a good vacuum; by that means economy in pumping is effected and, assuming the warmed water is utilized for other purposes as already indicated, the most effective heating of that water will be obtained without sacrificing the economy of the prime mover. A most ingenious and thoroughly effective regulator is now available, which is automatically

controlled by the vacuum produced in the condenser and which is capable of varying the water flow corresponding to the load fluctuations. I do not think the author has emphasized sufficiently the necessity of eliminating, where possible, losses in transmission. Usually in a paper mill of the old type a considerable percentage of the mill-engine output is absorbed in friction and other losses which, although unavoidable, can be frequently minimized by proper mill-wrighting and the use of roller or ball bearings. Some engineers object to these types of bearings, and although they are not suitable for every class of work they could be more extensively utilized and applied under expert advice and supervision. A number of troubles in the past have been due to the improper selection of the style and size of bearing appropriate for the work. I have used roller bearings on main beater shafts with great success; they have run for many years without the slightest trouble and with resultant economy. In one particular mill it was found that normally the main engine developed at full load about 550 h.p., whilst careful tests conclusively demonstrated that the friction load alone of line shafting, belts, and rope transmission without any machines running totalled 180 h.p., that is to say, about one-third of the total engine output was absorbed in friction and other losses. These were afterwards largely eliminated by modernizing the mill drives. The author touches only lightly on the finishing department, but this is really a most important section of a paper mill, and here a considerable wastage may take place owing to rejects on account of irregular cutting. Steady and uniform speed is essential for this part of the work, and in one mill the percentage of rejects due to irregularities was reduced from 14·5 to a negligible quantity where low-speed motors were installed on the lines indicated by the author on page 542. In regard to switchgear and wiring, whilst agreeing generally that tubing or casing in the conductors is to be condemned in the strongest possible terms, I cannot agree that lead-covered cables are unsuitable, as in all cases where I have installed them in paper mills there has not been the least trouble. The only other class of wiring in my opinion is open wiring, cab-tyre-sheathed cables being supported on insulators where all is open and available for inspection at any time. On page 543 the author gives most interesting figures for the actual steam consumed for drying purposes, and it is refreshing to secure sound evidence on this point. Some years ago I sought information of this nature from various sources, which included users as well as manufacturers of paper machines, and the figures given me varied between the limits of 7·5 and 18 lb. of steam per pound of paper. Whilst upon the subject, I should like to direct attention to the probability of using electric heating for drying purposes. It is not so easy as it sounds, but engineers have already overcome much more formidable obstacles in the application of electricity to industrial needs. Broadly speaking, the paper maker objects to the introduction of complicated electrical equipment and switchgear, and, therefore, the simpler the plant can be made the more will it appeal not only to the actual paper maker but also to the engineer responsible for its installation and upkeep. In this connection I welcome the development of the three-phase commutator motor with shunt characteristic and fine gradation of speed produced by shifting the brushes. Such machines give fine speed regulation without definite steps such as would be produced by the shunt regulator on a direct-current motor, and in this connection appear to embody the essential features for such fine regulation as that required on the individual

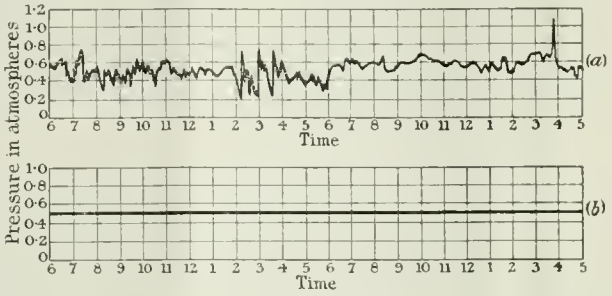


FIG. A.

(a) = Hand regulation.
(b) = Automatic regulation.

sections of a paper machine where the "draw" varies on each section. During the past year I have carried out trials on three-phase commutator motors supplied by the Swedish General Electric Company, and apart from the general excellence of the machines I was much impressed by the ease and facility with which the speed could be controlled by merely rocking the brushes. The three-phase commutator motor has exceedingly valuable features and, although necessarily

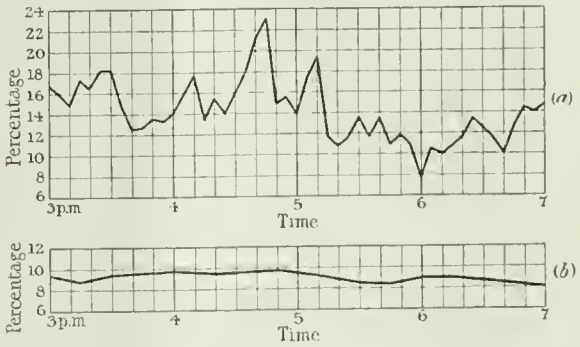


FIG. B.

(a) = Hand regulation.
(b) = Automatic regulation.

more expensive than an induction motor, can be utilized with great advantage in numerous ways industrially, and with its possibilities I welcome its introduction in paper mills. The author refers to pipe ventilation of motors in paper mills, and this is desirable in nearly all cases in the machine room, as where the machines are not liable to be washed down with a hose there is always a quantity of fluff and dust which is bound to settle on the motor. Looking last week at a photograph of a recent plant installed in America where the direct-current machines are of the open type, I wondered how long they would run without the inevitable trouble

due to insufficient protection. Where space permits I would advocate a separate motor room parallel to the paper machine. This would enable all motors to be of the open type, thus rendering them easier to inspect and clean, a procedure regarded as essential in cement mills. A special feature of the paper is that of heat economy, and I think the author will agree that in the majority of paper mills in this country this is almost an unknown term. The waste of heat units in the ordinary paper mill is perfectly appalling, but with the increasing price of coal drastic changes will have to be made. In many cases automatic regulation of the steam to drying cylinders can be adopted with advantage, and the upper curve of Fig. A is an actual chart of steam pressure when hand controlled. The lower curve shows the result of automatic regulation under precisely similar working conditions. Automatic regulation of stock to ensure a more uniform product can also be effected, and the curves shown in Fig. B indicate the difference between hand and automatic control. Devices of this nature which eliminate the human element are bound to result in greater efficiency in many directions, and secure better results with less expenditure in heat units and money, which is, after all, of importance to the paper maker.

[The author's reply to this discussion will be found on page 560.]

Liverpool Sub-centre, at Liverpool, 21 March, 1921.

Mr. T. D. Clothier: On reading that only 20 out of 548 paper machines are electrically driven in this country (as shown in Table 5) and that the aggregate horse-power of these motors is about 1 600, the impression obtained is that this trade is lamentably out of date; a closer examination, however, shows that the paper machines form only a small part of the power used in a paper mill, and that power otherwise used, some of which is scheduled in Tables 2, 3 and 4, is not included. I must admit some difficulty in reconciling the figures set out in Table 1, Fig. 1, and elsewhere in the paper, where steam is referred to by weight without reference to the range of pressure, or as to whether the steam is returned to the system or condensed; more especially as it is clear from Fig. 1 that the same steam is indicated for two purposes in succession, once in the engine and again in the dryer. Generally, it is made evident that a nice balance is called for between the steam required in the process of making paper and the use of the same weight of steam, above or below the process pressures necessary to provide power for driving the mill. I want to suggest, however, that the better method to-day would be to determine first the steam requirements for the process steam and to produce this in the most economical manner; that electricity should be used for all power purposes; and that any surplus heat should be utilized by the public authority to produce electricity, the excess energy, if any, being thus available for use elsewhere, or the balance, as the case may be, being obtained from the public supply. On page 538 there is a reference to the use of steam for bleaching. In the laundry practice (where steam is also required) electrolytic bleaching processes have been introduced, apparently with successful results. I should like to know whether the same process could be utilized in paper making. The author expresses a preference for low-speed motors, mill-wright drives, and generally the practice of the past; but the regard for the efficient use of metal and of the minimum expenditure of capital to produce a desired result has led to the production of the high-speed electric motor. Although I have only a slight knowledge of the paper-making industry, I should like to suggest that the speeds at which paper is made, as set out in Table 5, are too low. The manufacturer of paper machinery and the electrical manufacturer should co-operate and speed up the paper-making tools to be more suitable for electric driving, by cutting out some of the belting, gearing and shafting which form part of the paper-mill equipment at present. The author sets out four possible drives from a turbine, three of which are through reducing gear. I do not think a reduction gear is warranted here in any case unless the turbine sets are very small; a direct drive to an alternator is the best possible; even a direct drive to a direct-current generator is better than the introduction of a reduction gear, but, if direct current is required, a direct-coupled alternator and the use of rotary converters for the conversion of direct current is a good plan. The use of a medium-speed alternator and a reduction gear could not warrant the extra cost of such a combination. For the dry-end drive the ingenious "Harland" arrangement shown in Fig. 10 seems to be on the best lines for using an electric drive, especially if the double reduction gears could be dispensed with. The lay shaft, however, must be somewhat troublesome to carry to all the machines. The Metropolitan-Vickers Company are developing an electrical replacement of this shaft, and Messrs. Robinson manufacture an apparatus which I think would lend itself readily to the same purpose. There seems to be an unnecessary waste of power in belting, gearing, shafting and low-speed machinery in the paper-making trade, and much of the older machinery is due rather for replacement than for reconstruction to modern systems of driving. In order to make the best use of electricity for driving machinery, the latter also requires adapting to enable better use to be made of the more even turning movement, higher speeds and increased output possible per unit of factory space.

Mr. George Rattenbury: I wish to raise a few points from the switchgear maker's point of view. The author recommends pipe ventilation for the motors. When it is not possible to install motors and switchgear in a separate motor room, I should think that it would be a good plan to have pipe ventilation for switchgear as well as for motors. That applies more particularly to large switchgear, where there are big shunt regulators in constant use. The author also recommends the use of an auxiliary shunt regulator to subdivide the steps

of the main regulator. This is a very convenient arrangement from the switchgear maker's point of view, because it reduces the total number of steps necessary to obtain a large number of shunt regulator speeds, but some users seem to object to this system because in changing from step to step of the main regulator there is an unavoidable change of speed equal to the larger steps. That is to say, after regulation has been obtained to the limit of the auxiliary regulator, in order to get up to the next step of the main regulator it is necessary first of all to lower the speed by the auxiliary and then to raise it on the main regulator. It would be interesting to know what is the maximum change of speed permissible without damage to the paper, and whether this varies with the calliper and kind of paper. The author states that advances in speed from point to point should be as equal as possible over the whole range of speed, i.e. the speeds should be in arithmetical progression. I suggest that it might be better from the paper maker's point of view if the percentage increase of speed from point to point were the same throughout the range. In that case the speeds would be in geometrical progression, and this arrangement would be an advantage from the switchgear maker's point of view because it would reduce the arcing on the shunt-regulator contacts where it is usually most severe, viz. at the heavy end of the regulator. This system would not be so suitable where an auxiliary regulator is in use, because the resistance of the latter would then have to be of the same value as that of the maximum step on the main regulator. Another way of obtaining the large number of steps required would be to provide the main regulator with a duplicating brush, by means of which two or three times as many speeds would be obtained without increasing the number of resistance sections and without the disadvantage of the sudden jumps which are obtained when auxiliary regulators are used. With regard to the method of driving the dry-end, I should like to ask the author whether it is ever practicable to use a 3 : 1 variable-speed motor for the working range, with a small auxiliary motor suitably geared for the low speeds necessary for washing down and starting up. This auxiliary motor would be provided with a free-wheel arrangement, so that the main motor in starting up would overrun it. This arrangement is the one used on calenders. There is another method for obtaining the wide variations of speed necessary, i.e. to use two motors of equal size coupled together by means of a differential gear, the casing of the differential gear driving the machine. For the working range the two motors would assist each other and the differential gear would be practically locked. For low speeds one motor would be reversed and would act as a generator, and the speed of the machine would then depend upon the difference in speed of the two motors. By using motors with a 3 : 1 speed variation, it is possible to obtain a complete range of speed up to one-third of the maximum with the motors acting differentially, and then with the ordinary regulation to obtain the working range from one-third up to the maximum. This system would save the cost of a motor-generator set but, of course, it would add to the cost of the switchgear, and the cost of the differential gear would be extra. It might, however, be practicable in a small machine. For multi-motor drives the 3-wire system presents possibilities. The motors could be connected across the inners for low speeds and across the outers for the higher speeds. In that way the cost of the booster would be reduced. If the electrical engineer knew how the old steam-driven paper machines were governed, he might be able to form some idea of what he had to compete with in regard to speed control. I should like some further information in regard to the starting-up of paper machines, which is mentioned on page 547. For instance, is it necessary to provide very gradual speed variation from standstill to the maximum working speed? When a booster set is used, sometimes a negative booster is employed for starting purposes only, the normal running speed being direct from the supply. In that case is it practicable to cut out the booster set when the machine reaches its working speed?

Mr. A. E. Malpas: I should like to know how the roof heating to prevent sweating was carried out. During the war I had occasion to erect a plant for making high explosives, and one part of the chemical process consisted in concentrating a liquor. The steam, amounting to about 150 tons per week, collected under the roof, and caused a great deal of trouble and repeated shocks, as well as breakdown of the wiring. Apparently the conditions in a paper mill are similar. I notice that most of the plants the details of which are given on page 542, are direct-current machines. I take it that, at all events up to quite recently, the difficulties in connection with speed regulation have prevented the employment of alternating current. The alternating-current commutator motor is now quoted as being used, but I should like to know whether their cost is not so high as almost to rule out the extended use of alternating current. The merits of the two systems have been very much debated and alternating current has, to a large extent, displaced direct current in most works. In the case mentioned there were three 1 000-kW direct-current sets which were geared. The point mentioned at the bottom of page 540 with reference to group driving might easily be passed unnoticed unless one had some experience with high peaks. I remember a case where 6 or 12 furnaces had to be driven, and where the only effective way was by means of group driving, because each machine as it swung over had a very high peak. One shaft was used, for separate driving with individual motors would cause trouble. One likes to see a low-speed motor, but unfortunately in estimating for rather a large plant it is generally found that nobody can be persuaded to spend money on low-speed machines. I think the point made by Mr. Clothier was very just; that capital considerations are even more important to-day than they were, and must be considered. The sweating of buildings is a serious trouble, and I have had many such cases. In connection with the lead-covered systems mentioned at the top of page 542, it is, perhaps, not generally known that the lead covering is liable to attack by ammonia fumes, and in a paper mill the lead would be attacked by the chlorine fumes.

No one will disagree with the author when he says that reliability is one of the most important considerations, not only in paper plant but in any plant. I have made it an absolute rule that if a plant is laid down to run continuously for 168 hours per week, it must have a duplicate as stand-by. It is the only way to have a steady output, as in the event of a breakdown a sound machine can be switched in. I am inclined to agree with the author when he says on page 543 that there can be too many interlocks and safeguards, as very often as much trouble arises from the safeguards as from the breakdowns they were intended to prevent. I should like to know how to prevent the sweating in the roof; this problem is very important, particularly in chemical works. The return of heat to the boiler feed-water is a question that is often cropping up, and I am of the opinion that, if exhaust steam is used, too much heat cannot be returned. The best plan is to install economizers and reduce the fuel consumption of the boiler plant. The utilization of existing prime movers with mixed-pressure turbines seems to me a method of working that has been superseded. The exhaust turbine was coming into general use 10 or 15 years ago and there are many installations where they are used in conjunction with the reciprocating engine in order to take advantage of the higher vacua. I should think there are very few of these old installations still working. Has the author had much trouble with superheaters? With regard to Mr. Clothier's remarks on linking-up, it must be remembered that, where works require a large amount of steam, the difficulty is to maintain the exact balance between electricity and steam requirements. I suppose that in the case of a modern paper mill there is not sufficient spare power to sell to warrant the installation of the expensive cables necessary. I may cite a case which has come to my notice of a chemical plant where liquid has to be evaporated. Steam generating sets are run for 16 hours a day, and at night the whole of the load is thrown on to the main generating station. This appears to be quite an economical method.

Mr. A. S. Wilson: Paper-mill plant entails the combination of steam and electric drives, and I am glad that the author thinks there should be some check on the steam used. Many engineers ignore this question on the steam side of their plants, although a total leak of 1/100th of a square inch in a steam pipe at 150 lb. pressure is equivalent to a waste of at least 40 tons of coal per year. Speed variation over a very wide range and of a very even nature is apparently one of the difficulties met with in paper-mill driving, and I note the complicated nature of the control gear necessary when using direct-current motors. These difficulties could be avoided by the use of the alternating-current commutator motor referred to by the author. The cost is, of course, considerably greater than that of the direct-current motor, but I do not think that this would be prohibitive, as it must be remembered that to offset this increased cost practically the whole of the control gear required with the direct-current system can be dispensed with. All the switchgear necessary with the alternating-current commutator motor is a triple-pole starting switch, when a speed variation over a very wide range is obtained simply by moving the brushes. With the brushes in the minimum-speed position the motor can be thrown direct on the line, giving a good starting torque with a starting current not exceeding full-load current. Another saving in the cost of such equipments is obtained in the wiring, there being only three wires leading to the slip-rings. Perhaps the author will, at some future date, give us his further experience in paper-mill driving with alternating-current supply, utilizing this type of electrical equipment.

Mr. A. B. Mallinson (*in reply to the Manchester and Liverpool discussions*): The discussions have centred on the following points:—

(1) Choice of power plant.
(2) Relative advantages of single-motor, multi-motor, and engine drives to machines.
(3) General details, relative to the application of electric power, such as switchgear, wiring, alternating-current commutator motors, ventilation, etc.

(1) *Choice of power plant.*—The paper was primarily written having in view existing practice in British paper mills; conditions vary so much in each mill that it is impossible to lay down any hard-and-fast lines on which power plants should be installed. Each mill must therefore be considered on its own merits; generally speaking, however, one can assume that for "news" mills a back-pressure reciprocating-engine system will prove suitable, but for those mills where intermittent digestion of raw material is worked, such as "rag" and "esparto" mills, a power unit of the "pass-out" type with condenser will be essential.

The taking of power from an outside source, which Mr. Watson mentioned, cannot enable the boiler house to be dispensed with, as Table 1 shows; there have been several cases where mills have adopted this course, and very probably materially reduced their running costs by so doing, due to the out-of-date individual high-pressure engine drives previously in use, but I think any paper mill, if it is prepared to lay down the necessary capital for a modern heat-economy power plant, can show a substantial economy. Some mills have taken an outside supply on an agreement covering a term of years, solely on account of capital outlay, as they reckon to have saved enough at the end of that time to complete their scheme.

Mr. Malpas, speaking as one responsible for the power supply of continuously operating factories, such as paper mills, is, I am glad to note, quite in agreement with me on the question of duplicating turbo plants.

Mr. Boyle's experiences are typical of the possibilities that exist in paper mills for economies in power production; a saving of 50 per cent on a 132-hour-per-week load of 1 000 to 1 500 h.p. will obviously justify a very substantial capital outlay in the steam-raising and power plant sections.

I note Mr. Kemp considers the bleeding of turbines at different pressures to be a fallacy. This depends entirely on the particular requirements of the mill

concerned; if only a small quantity of steam is required, say at 40 lb. per sq. in. for heating, and the bulk is at 5 to 15 lb. per sq. in., obviously one pressure on the main heating system is the correct lay-out. But if in "esparto" mills there is, as shown in Table 1, two to three times the amount of steam for digesting as is required for drying, the relative pressures being 30 to 60 lb. per sq. in. for the former, and 5 to 15 lb. per sq. in. for the latter, surely the best lay-out is that which makes full use of the potential power available in both heating-steam demands. Mr. Kemp has rightly drawn attention to the fluctuations of the demand for heating steam for the intermittent processes of digestion which are mentioned in the last two paragraphs of the paper. One or two of the early attempts at heat-economy in paper mills, using back-pressure engines, were failures because the nature of these intermittent heating steam demands was not understood.

In reply to Mr. Tasker I have used the condenser water in two or three cases, but a considerable amount of the heat was lost by radiation by the time the water was used.

Mr. Clothier suggests that the paper-mill power plant should obtain its essential balance in heat and power demand by disposing of the surplus of either to the nearest public supply. Paper mills are primarily located by water conditions and, in consequence, many are not near any outside supply. Several mills have, however, on a small scale done something on these lines—supplying light to workers' houses, and power to small adjacent factories. Steam also has been supplied for heating the former.

(2) *The paper-machine drive.*—Mr. Anderson and Mr. Kemp speak from practical experience of the system with which they are connected, i.e. the Harland patent speed interlock, but I still do not consider that the single-motor drive has been superseded in view of the proved results from the Harland drives recently started in Canada. In considering the type of drive for the paper machine, the class of machine and its duty must be considered just as carefully as in the selection of power plant; both these speakers have put forward very forcible arguments for the multi-motor drive on the modern fast "news" machines, but not 10 per cent of the machines in this country are of this type, and probably not even 5 per cent are modern enough to be classed with the machines now being put into operation in Canada. The remaining 90 per cent or so are much smaller machines, making the better-class papers and browns, and taking an average of 30 to 40 h.p. to drive. Considerations of capital outlay alone will mean that these machines must stay in service for many years to come, quite apart from the decided differences of opinion among expert paper makers as to the possibilities of making as good a sheet (of fine paper) with wide machines and higher speeds. There is, therefore, not a great saving in power to be anticipated, even assuming the figures given by Mr. Anderson to hold good, on this type of machine. The friction of the back drive, whilst undoubtedly a very big item on the fast-running "news" machines of 350 h.p., is obviously very much reduced on the slower machines taking about 40 h.p. The capital outlay on the multi-motor drive with its 5 to 13 motors and speed-reduction gears, interlocks, master control, motor-generator, etc., must obviously be very substantial, and that alone, as I mentioned on page 553, will put it out of court in many cases where the change-over to the electric driving of existing machines comes to be considered by the paper maker.

The figures quoted by Mr. Anderson regarding belt slip are extremely interesting, but I do not think actual running experiences on paper machines justify the assumption that these variations in slip are constantly occurring. Mr. Adamson's statement in regard to the actual running results of the mammoth Walmsley machines in Canada, running with an average of only one "break" in 24 hours at 700 feet per minute, is proof of this. The figures given of the average power taken by "news" machines, e.g. 1·97 h.p. per inch width with single-motor drive, to 1·26 h.p. for the multi-motor Laurentide drive, show an undoubted advantage for the multi-drive on the fast-running "news" machine; there are, however, many factors which enter into the question, apart from the drive. For instance, Mr. Adamson quotes under 350 b.h.p. for the modern 189-inch machines at over 700 feet per minute, which is 1·85 h.p. per inch at 700 feet per minute against 1·26 h.p. at 600 feet per minute for the multi-motor drive. There will certainly be, in the next few years, great developments in the multi-driving of paper machines, and the results obtained by the Harland interlock and other systems will, I am sure, be very closely followed by those who are interested in the engineering requirements of paper mills. The multi-motor systems have already shown the paper maker these important factors:

(*a*) They have separated out the power required for each section of the machine.
(*b*) They have enabled the "draw" between sections to be accurately measured.
(*c*) They have located the great amount of power that is used in the single-drive fast-running machines to overcome the friction of the back drive.
(*d*) The Harland system at Laurentide has already run up to 976 feet per minute, a speed which I think it will be agreed would not have been thought possible as recently as 1914.

The reduction of "broke" which the advocates of the multi-motor drives claim will be carefully watched by paper makers.

With regard to the single-motor systems described in the paper, I would emphasize that these have all been in actual use for some years, and fulfil the requirements of the individual machines to which they have been fitted.

The use of compound engines for the machine drives, mentioned by one or two speakers, has certainly been tried, but not to any great extent in this country: the more usual practice has been to install a reducing valve between the paper-machine engine, and the boiler-house, to prevent variations of machine speed

when the boiler pressure falls whilst cleaning out fires, etc.

(3) *General matters.*—Mr. Rattenbury's suggestion in regard to the pipe-ventilation of the control gear is very good; however, as this gear will almost invariably be at some distance from the motor, it will be necessary to provide a small fan and motor with the switch pillar. As regards the auxiliary shunt-regulator, I have found that paper makers, where it has been installed, consider that the advantage of finer regulation which it gives much more than counteracts the objection to the momentary jump which occurs when changing from one range to another. The small motor for low speeds is not necessary on a paper machine, as the machine is threaded through at its running speed; the super-calender with its cotton or paper bowls is threaded through at 30 to 60 feet per minute, and as the load increases with each nip, a separate "crawling" motor is very advisable. The use of special combinations of two motors or three-wire systems is not advisable or necessary, as all the range required can easily be obtained by the more simple single-motor drive of the right type. In further reply to Mr. Rattenbury, the machine is run dead slow to warm up, wash down, etc., and is afterwards gradually brought up to the lowest running speed, but there is no need for this to be by very fine steps. I have found the ordinary drum controllers with about six series steps to be quite suitable for running up to the lowest paper speed.

Wiring.—The problem of satisfactory industrial wiring is a most important item in the success or failure of any electrification system; it is really almost incredible how installations are still being put in on lines that have been tried and discarded after endless trouble in dozens of cases.

For the information of those speakers who have inquired about the cost of alternating-current commutator motors, I give a curve (Fig. C.) showing the relative costs, under similar conditions, for single-motor drive for paper machines:

(*a*) By shunt control.
(*b*) By potentiometer control with rope-driven dynamo and exciter.
(*c*) By variable oil gear with squirrel-cage motor.
(*d*) By alternating-current commutator motor.

In each case the control gear and reducing gear, with flexible coupling and underbase to couple the motor up, is included, but not the generating plant, except in the case of the potentiometer control, which is, in consequence, the most expensive of all the drives. A curve is also given of the cost of direct-current generators, where there is no existing supply or sufficient margin on the existing supply. It will be noted from these curves that the cost of the complete alternating-current commutator motor-drive is about 17 per cent more than the 3 to 1 direct-current shunt-control equipment.

In reply to Mr. Malpas the condensation in the paper-machine houses has been a source of continual trouble to paper makers—quite apart from the effects on the structure and wiring, as the drops of water cause "breaks" on the machine, and faulty paper. On large machines making a ton and upwards of paper per hour, the generally accepted system is to heat the air in the roof by the "Plenum" system, with motor-driven fans drawing the outside air through a series of pipes, heated by back-pressure steam.

Mr. Clothier mentions electrolytic bleaching; this has not, to my knowledge, been adopted in any English paper mill, doubtless because in normal times the ordinary bleaching powder can so easily and cheaply be obtained. In the Colonies, where the powder has to be imported, with the inevitable increased cost and the loss of one-third to one-half its strength between manufacture and use, electrolytic bleaching has made considerable progress, and I believe is giving every satisfaction.

Mr. Spicer raises a query with regard to one of the machines given in Table 5; this particular machine, whilst nominally a "news" machine, is out of date compared with the modern machines generally re-

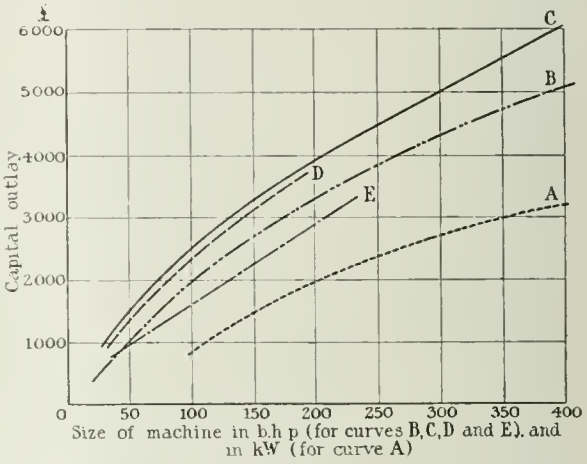


FIG. C.—Curves showing the cost of single-motor paper machine drives on different systems, January 1921.

A. Direct-current dynamos with three bearings and pulley for rope or belt drive. Speed 400 r.p.m., with switchboard.
B. Shunt-control motors with 3 to 1 range for machine drive with control gear and enclosed reducing-speed gear. 400 to 1 200 r.p.m. for smaller sizes up to 250 to 750 r.p.m. for larger sizes.
C. Potentiometer control, including motor, control switch, reducing gear, and belt- or rope-driven dynamo and exciter at 500 r.p.m.
D. Alternating-current commutator motors, including motor, reducing gear, and oil switch. 3 to 1 shunt characteristic speed range.
E. Williams-Janney oil transmission, including electric motor and starter, oil pump and oil motor, and speed-reducing gear.

ferred to in the paper, and in consequence the powers are not comparable. The figures which Mr. Spicer gives of the tests taken in 1906 are very interesting, as they show the speeds at which that class of paper was made then—the same paper nowadays being made at about 550 feet per minute.

Several speakers have referred to the friction losses in the shaft-driven mill. It is possible, in any ordinarily laid-out paper mill, with the one main shaft in the beater cellar, to drive all the beating and refining plant, which takes the bulk of the power, and if that shaft is properly millwrighted on roller, ball, or well-designed self-oiling bearings, I consider that a better system cannot be found.

Mr. Clothier suggests that it is out of date to keep to these low speeds, but it is necessary on account of the low speed of the beaters (100 to 150 r.p.m.). When extra countershafts are added, then undoubtedly the friction of the drive becomes much greater. My experience has been that the best course is to have one main drive, and to drive all the rest of the machines electrically.

Mr. Tasker mentions the possibility of raising the steam required for manufacturing purposes by means of electricity. This may be the case where there is abundant water power, but it is quite out of the question for English paper-mill practice; an article recently published gave the equivalent cost of electricity to compare with coal at 40s. per ton, and 7 lb. of water per 1 lb. of coal, as 0·085d. per unit. Mr. Tasker also draws attention to the possibility of automatic regulation of the steam to the drying cylinders; this in theory is sound, but my experience has been that the automatic regulators are soon put out of order, and wherever I have seen them tried they have been abandoned after an extended trial.

PROCEEDINGS OF THE INSTITUTION.

663RD ORDINARY MEETING, 17 MARCH, 1921.

(Held at the Institution of Civil Engineers.)

Mr. J. S. Highfield, Vice-President, took the chair at 6 p.m. owing to the unavoidable absence of the President on Institution business.

The minutes of the Ordinary Meeting of the 10th March, 1921, were taken as read, and were confirmed and signed.

Messrs. F. O. Barralet and G. H. Nash were appointed scrutineers of the ballot for the election and transfer of members and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

Members.

Brown, James.
Carr, William Saxon.
Erikson, Per Engelbert.

Associate Members.

Beanland, Henry.
Berthon, John Henry P.
Brocklesby, Charley.
Callis, Henry John.
Cave-Brown-Cave, Nigel Frederick, B.Sc.
Chambers, Geoffrey.
Drummond, David George F.
Ellis, James Rutherford.
Ferguson, John Davis, B.Sc. (Eng.).
Forbes, Lewis Jex-Blake, B.Sc. (Eng.).
Gilliland, Horace Gray, Capt., M.C.
Graham, John Shoolbred.
Hodgetts, William James.
Holmes, Joseph.
Jones, Arthur Ernest.
Jones, Ernest.
Jowitt, Leslie James.
Lane, Billings Joseph M., B.Sc.
Leedal, Reginald.
Martinez, Ginseppe.
Miller, Eugene Herbert.
Nunn, Robert John.
Ogden, Ernest.
Pearson, Ernest Alfred.
Pidgeon, Joseph Ernest, B.Sc. (Eng.).
Richardson, George.
Roscoe, Percy Harold.
Swaine, Thomas Charles L.
Taylor, Harry John, B.Sc.
Trench, Ralph Chenevix, Capt.

Graduates.

Bennett, Arthur Edgar.
George, Thomas Alfred.
Ghose, Nirendra Nath.
Haigh, Harold Edwin.
Howell, Arthur.
Knight, Frederick James.
Mills, William Seabrook.
Pearce, Frederick James.
Peat, Ralph Douglas.
Rouse, William Cameron.
Thakkur, Khimji Bhanji.
Tucker, Percy Walter.
Walter, William James H.

Students.

Abbey, Ernest.
Acheson, Frederick William.
Aitken, Ian Miller E.
Allen, Thomas Palmer.
Allsop, George.
Andrew, Percy.
Baker, James.
Balcomb, Leslie Charles E.
Bartlett, Herbert Edward.
Bennett, Cecil Kay.
Birrell, James Mackie.
Blazey, Norman Claude.
Booker, Standish Jerome G.
Braithwaite, Walter Nugent.
Bullen, Eric Harold S.
Burgum, Alfred Edgar.
Cann, Maurice George.
Carr, Frederick Albert.
Carr, Harold.
Carter, Ernest.
Clifford, Herbert Spear.
Cooper, Andrew Ramsden.
Costain, Edward.
Creighton, William Alexander L.
de Barsy, George Bernard M.
Dowson, Christopher Henry.
Edwards, Frank Stanley.
Emerson, Charles Robert.
England, William Lishman.
Farrant, Dovel Pearce.
Ferguson, Ian Foxton.
Forshaw, Henry Watson.
Francis, Sydney.
Furniss, William Stacey.
Gale, Bernard Robert.
Grafton, Reginald Hannaford.
Greeves, John Ronald H.
Griffiths, David William.
Hall, Eric Stephen.
Hancock, Raymond George
Hayes, William Ernest.
Henderson, Reginald.
Hibberd, William Alfred.
Hinxman, Hugh.

Students—continued.

Hunter, Edgar Ian.
Hutchinson, John Yates.
Hutton, William Stanley.
Imeary, Robert Keith.
Jago, Ronald Albert.
Kelly, Maurice Vincent.
Knight, Eric Richard.
Knox, George Clouston.
Lance, Thomas Constable M.
Lucas, Albert William J.
McMahon, Alfred Joseph.
Marshall, Leslie Benjamin.
Maskelyne, Noel.
Middleton, Edward.
Miller, Otto William.
Nixon, Walter Ivan.
Oliver, Alfred Marriott.
Oughton, Frank Cyril.
Outram, Frederick Colin.
Paterson, William Clyde.
Payne, Edwin Levi.
Perry, Fergus Roy.
Phillips, Charles George R.
Pritchard, Eric.
Reynolds, Walter Howard.
Ritson, Thomas.
Robinson, Charles Edwin.
Robinson, Collingwood.
Short, George Leonard.
Simmonds, Leslie Raymond.
Simmonds, Walter James.
Slater, Joseph Thomas L.
Smith, Leslie Middleton.
Sothers, Hugh Valentine.
Stephenson, Eric Fletcher.
Stoddart, John.
Taylor, Henry.
Taylor, Horace.
Thomas, David Evan F.
Thompson, Cyril Lingwood
Tildesley, Arthur Edward.
Tittle, John Moore, Capt.
Tuck, William Thomas E.
White, John Charles.
Wilde, Wilfrid.
Wilson, David MacArthur.
Wilson, William Ernest.
Young, Bruce.

Associate.

Shevlin, Joseph Thomas.

TRANSFERS.

Associate Member to Member.

Burford, James Wilfred
Cridge, Arthur John.
Dolton, William.
Grubb, Robert Walter.
Hines, John Gerald.
Kirkby, Henry McKenzie, M.C.
Maccall, William Tolme, M.Sc.
Turner, John William.
Warren, Arthur George.
Webber, George Edward F.
Wright, George Norris.

Graduate to Associate Member.

Davies, James Henry.
Mann, Herbert Clarence
Willoughby, Godfrey Pountney, M.Sc. (Tech.).

Student to Associate Member.

Riley, Harold, M.C., B.Sc.
Stoneham, Cecil Deacon.

Student to Graduate.

Bond, Fred.
Colburn, Fred.
Halford, William Charles J.
Heymann, Paul Gerhard.
Peasgood, Frank.
Semple, James Hamilton.

A paper by Sir William Noble, Member, entitled "The Long-distance Telephone System of the United Kingdom" (see page 389), was read and discussed, and the meeting terminated at 7.45 p.m.

664TH ORDINARY MEETING, 7 APRIL, 1921.

(Held at the Institution of Civil Engineers.)

Mr. LI. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 17th March, 1921, were taken as read and were confirmed and signed.

The following donations were announced as having been received and the thanks of the meeting were accorded to the donors:—

Building Fund.—W. M. Mordey and Sir John Snell.

Library.—The Association of Mining Electrical Engineers; The Association of Municipal Electrical Engineers (Union of South Africa); W. W. Beaumont; The Carnegie Institute of Washington; The Chief Electrical Engineer, Public Works Department, Wellington, New Zealand; Messrs. Constable & Co., Ltd.; The Department of the Interior, Canada; The Director-General of Posts and Telegraphs, India; The Electrical Contractors' Association; The Engineer-in-Chief, G.P.O., London; R. Grierson; J. H. Havelock; P. S. Helmick; T. E. Herbert; The Hydro-Electric Power Commission of Ontario; The Imperial Mineral Resources Bureau; B. Leggett; The Massachusetts Institute of Technology; H. Nukiyama; Messrs. S. Rentell & Co., Ltd.; Messrs. F. W. Skinner & Co.; Messrs. Smith, Kempe & Smith; The Surveyor-General of India; S. G. Starling; The Technical Publishing Co., Ltd.; and L. B. Turner.

Benevolent Fund.—(See *Institution Notes*, No. 25, page 18, March 1921.)

The President: Some two years ago, when an effort was being made to establish an Electrical Research Association, the Institution and the B.E.A.M.A. undertook a joint guarantee of £750 a year each for three years. We recently received a communication from the Research Association saying that it had now been successfully launched and that very considerable funds had been guaranteed, which will carry with them an equal contribution from the Department of Scientific and Industrial Research, and releasing the Institution from its guarantee. The Institution had guaranteed £750 for three years, but have only paid altogether £375, and we have achieved the object we had in view, namely, of getting a Research Association for our industry established on a firm footing. The Council decided to-day to make a donation of £200 from the Institution funds to the Association. All our members will benefit to a large extent by the information and researches which will be published, and therefore it is only right that the Institution as a body should make a reasonable contribution within its means.

A paper by Mr. K. Baumann, entitled "Some Recent Developments in Large Steam Turbine Practice" (see page 565), was read and discussed, and the meeting adjourned at 7.45 p.m.

SOME RECENT DEVELOPMENTS IN LARGE STEAM TURBINE PRACTICE.

By K. Baumann.

(*Paper first received* 8 *January, and in final form* 15 *March* ; *read before* The Institution 7 *April, before the* North-Western Centre 12 *April, and before the* North-Eastern Centre 9 *May*, 1921.)

PART I.

SUMMARY OF DEVELOPMENT.

Table of Contents.

(1) Introduction.
(2) General trend of development.
(3) Development of the reaction turbine.
(4) Development of the impulse turbine.
(5) Comparison of commercial development.
(6) Development of turbines for special purposes.

(1) Introduction.

In 1912 the author read a paper * before the Institution describing the development of the steam turbine up to that date. The object of this paper is to give a general outline of turbine practice during the interval, based on the published records and other information which has come to the author's notice, and also to review the circumstances which resulted in developments which can be best described as the "race for the maximum output at the highest possible speed."

In the earlier paper, the technical limitations affecting the increase in the maximum possible output at a given speed were fully described in anticipation of the progress which has since taken place. This paper describes how this progress has been made possible, and, in view of the importance of large efficient turbines required for the power stations of the future, the problems in connection with this particular type of turbine are dealt with in detail.

(2) General Trend of Development.

The general trend of development towards the maximum possible output at the highest possible speed is justified for several reasons, the most important of which relate to reliability, thermal economy, and first cost. All of these factors are fully dealt with in this paper. This development towards large capacities is conditional on a corresponding development of the driven machine with which the turbine is directly connected, or, alternatively, of the gear used for transmitting the power to the driven machine.

The only machine which comes under the first category is the turbo-alternator. From a purely electrical point of view, the development of the high-speed alternator is not justified. The high-speed alternator is less efficient than the low-speed alternator, and its first cost is practically the same. Nevertheless, the advantages of the high-speed turbine fully justify the special endeavours of the electrical designers to overcome the obstacles with which they may be faced in keeping pace with turbine development.

Table 1 gives a list of turbo-alternators built by various makers during the last 10 years. The largest alternator with a speed of 1 000 r.p.m. was built by the A.E.G. in 1918. This set is very expensive and future progress will be towards increasing the output of 1 500-r.p.m. machines, rather than developing 1 000-r.p.m. sets. The largest 1 500-r.p.m. machine at present in operation has a capacity of 35 000 kVA (maximum continuous rating), and most manufacturers are willing to build an alternator of this size. A 35 000-kVA alternator has been built for a speed of 1 800 r.p.m., and the capacity of this machine is

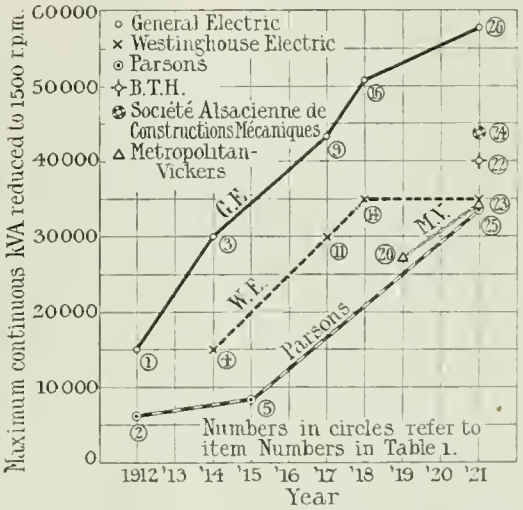


Fig. 1.—Growth of maximum continuous kVA rating of large alternators, reduced to 1 500 r.p.m.

equivalent to 50 800 kVA at 1 500 r.p.m., on the basis that the maximum possible capacity varies inversely as the square of the speed (r.p.m.). Fig. 1 shows the growth of the maximum continuous kVA rating reduced to 1 500 r.p.m. diagrammatically.

Table 2 gives a list of turbo-alternators running at high speeds (2 400 r.p.m. and above) which have been built during the last 10 years. The capacity has increased in this country from about 3 000 kVA in 1912 to 17 200 kVA maximum continuous rating in 1921. Fig. 2 shows the growth of the maximum continuous kVA rating reduced to 3 000 r.p.m. diagrammatically.

During the past few years, the direct-current generator driven directly from the turbine has disappeared from the market. Its place has been taken by the moderately low-speed generator driven by mechanical reduction

* *Journal I.E.E.*, 1912, vol. 48, p. 768.

TABLE 1.

List of Large Turbo-Alternator Sets.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
	Reference					Rating in kW				Comparison of Generators on Maximum Continuous Rating					Comparison of Turbines on Economical Rating					
Item No.			Manufacturer	Customer and Power Station	Year Installed	Max. Continuous	Economical	Vacuum at Economical Rating, in. abs. (Hg)	Speed, r.p.m.	No. of Generators	kW, Actual	Power Factor	kVA, Actual	kVA reduced to 1 500 r.p.m.	No. of Cylinders	No. of Shafts	No. of Exhausts	kW per Exhaust		
																		Actual	Reduced to 1 in. abs. (Hg)	For 1 in. abs., 1 500 r.p.m.
1	*Elec. World*	18/10/13	G.E.	Commonwealth, Fisk Street, Chicago	1912	20 000	15 000	1	1 200	1	20 000	0·85	23 500	15 100	1	1	1	15 000	15 000	9 600
2	*Engineering*	17/10/13	Parsons	Commonwealth, Fisk Street, Chicago	1912	25 000	20 000	1	750	1	25 000	1·0	25 000	6 250	2	1	2	10 000	10 000	2 500
3	*Elec. World*	18/10/13	G.E.	Commonwealth, North West, Chicago	1914	30 000	25 000	1	1 500	1	30 000	1·0	30 000	30 000	2	1	2	12 500	12 500	12 500
4	*Power* / *Elec. Journal* / *Power*	20/6/16 / June '15 / 19/8/13	Westinghouse	Interboro' Rapid T., 74th Street, New York	1914	30 000	25 000	1	1 500 / 750	2	15 000	1·0	15 000	15 000	2	2	2	12 500	12 500	12 500
5	*Engineering*	17/10/13 / 18/5/17	Parsons	Underground, Lots Road, London	1915	18 000	15 000	1	1 000	1	18 000	0·95	18 950	8 400	2	1	2	7 500	7 500	3 340
6	*Mech. Eng.*	19/6/14	G.E.	Philadelphia Elect. Co.	1915	35 000	25 000	1·5	1 200	1	35 000	1·0	35 000	22 400	2	1	1	25 000	20 400	13 050
7	*N.E.L.A.*	April '16	G.E.	Philadelphia Elect. Co.	1915	30 000	22 500	1·5	1 500	1	30 000	1·0	30 000	30 000	2	1	1	22 500	18 350	18 350
8	*Elec. Review*	7/4/16	G.E.	Boston Elevated Railway, Boston South	1917	35 000	25 000	1	1 500	1	35 000	1·0	35 000	35 000	1	1	1	25 000	25 000	25 000
9	*Power*	12/2/18	G.E.	Windsor Power Station	1917	30 000	22 000	1	1 800	1	30 000	1·0	30 000	43 250	1	1	1	22 000	22 000	31 700
10	*Elec. Journal*	26/2/18 / June '18	Westinghouse	Commonwealth, North West, Chicago	1917	30 000	25 000	1	1 200	1	30 000	0·85	35 300	22 600	2	1	2	12 500	12 500	8 000
11	*Power*	18/4/16 / 27/8/18	Westinghouse	Providence, Narragansett	1917	40 000	33 000	2	1 800 / 1 200	2	20 000	0·95	21 000	30 300	2	2	2	16 500	11 650	16 800
12	*Power*	19/2/18	G.E.	Detroit Edison, Connors Creek	1918	45 000	35 000	1	1 200	1	45 000	0·90	50 000	32 000	1	1	1	35 000	35 000	22 400
13	*Elec. Journal*	Feb. 1918 / May 1919	Westinghouse	Interboro' Rapid T., 74th Street, New York	1918	60 000	40 000	1	1 500	3	20 000	1·0	20 000	20 000	3	3	4	10 000	10 000	10 000
14	Author	—	Westinghouse	Commonwealth, Fisk Street, Chicago	1918	35 000	25 000	1	1 500	1	35 000	1·0	35 000	35 000	2	1	2	12 500	12 500	12 500
15	*Power*	12/3/18	G.E.	Commonwealth, North-West, Chicago	1918	35 000	25 000	1	1 500	1	35 000	1·0	35 000	35 000	1	1	1	25 000	25 000	25 000
16	Author	20/8/18 / —	G.E.	Commonwealth, Fisk Street, Chicago	1918	30 000	22 500	1	1 800	1	30 000	0·85	35 300	50 800	1	1	1	22 500	22 500	32 400
17	*Power*	3/9/18 / 17/9/18	G.E.	Cincinnati	1918	30 000	22 500	1	1 800	1	30 000	0·85	35 300	50 800	1	1	1	22 500	22 500	32 400
18	*Z.f.d.g.T.*	30/4/18	A.E.G.	Rheinisch Westfälische Kraftwerke, Cologne	1919	50 000	40 000	2	1 000	1	50 000	0·83	60 000	26 600	1	1	1	40 000	28 300	12 600
19	*Power*	25/3/19	Westinghouse	Muscle Shoals	1919	60 000	50 000	1	1 800 / 1 200	3	20 000	0·85	23 500	33 800	3	3	4	12 500	12 500	18 000
20	Author	—	R.W. Met.-Vickers	Manchester Corporation	1919	22 000	20 000	2	1 500	1	22 000	0·80	27 500	27 500	1	1	1	20 000	14 150	14 150
21	Author	—	Met.-Vickers	Glasgow Corporation	1920	18 750	15 000	0·9	1 500	1	18 750	1·0	18 750	18 750	1	1	1	15 000	15 800	15 800
22	*Z.f.d.g.T.*	30/4/20	B.T.H.	Rotherham	1921	30 000	25 000	1·5	1 500	1	30 000	0·75	40 000	40 000	1	1	1	25 000	20 400	20 400
23	Author	—	Met.-Vickers	Manchester Corporation	1921	27 500	25 000	0·9	1 500	1	27 500	0·80	34 400	34 400	1	1	1	25 000	26 400	26 400
24	Author	—	Zoelly Type	Paris (Gennevilliers)	1921	35 000	30 000	1·05	1 500	1	35 000	0·80	43 700	43 700	1	1	1	30 000	20 200	20 200
25	Author	—	Parsons & Met.-Vickers	Lots Road, London	1921	18 000	15 000	1	2 000	1	18 000	0·95	18 950	33 700	2	1	2	7 500	7 500	13 300
26	*Proc. A.I.E.E.*	Nov. 1919	G.E.	Proposed practice for 1921	—	34 000	—	—	1 800	1	34 000	0·85	40 000	57 500	—	1	—	—	—	—

TABLE 2.

Large High-speed Turbo-Alternator Sets.

1	2	3	4	5	6	7	7a	8	9	10	11	12	13	14	15	16	17	18	19	20	21
						Rating in kW					Comparison of Generators on Maximum Continuous Rating					Comparison of Turbines on Economical Rating					
																			kW per Exhaust		
Item No.	Reference		Manufacturer	Customer and Power Station	Year Installed	Max. Continuous	Overload for 2 Hours * ½ Hour †	Economical	Vacuum at Economical Rating, in. abs. (Hg)	Speed, r.p.m.	No. of Generators	kW, Actual	Power Factor	kVA, Actual	kVA reduced to 3 000 r.p.m.	No. of Cylinders	No. of Shafts	No. of Exhausts	Actual	Reduced to 1 in. abs. (Hg)	For 1 in. abs. (Hg) 3 000 r.p.m.
1	Author	—	B.B.C.	Schlettstadt	1911	3 000	3 750†	3 000	1·5	3 000	1	3 000	0·8	3 750	3 750	1	1	1	3 000	2 450	2 450
2	*Elec. Journal*	Feb. 1918	Westinghouse	Consumers' Power Co., St. Paul, Minn.	1912	4 500	4 500*	3 500	1·5	3 600	1	4 500	0·8	5 625	8 100	1	1	2	1 750	1 425	2 050
3	Author	—	Met.-Vickers	Singer Manufacturing Co., Glasgow	1913	4 400	5 000*	4 000	1·3	2 400	1	4 400	1·0	4 400	2 820	1	1	1	4 000	3 510	2 250
4	Author	—	Met.-Vickers	Vickers, Barrow	1914	3 300	3 750*	3 000	1·5	3 000	1	3 300	0·8	4 125	4 125	1	1	1	3 000	2 450	2 450
5	Author	—	Westinghouse	Evansville Public Service Co.	1914	5 000	6 000*	4 000	1	3 600	1	5 000	0·8	6 250	9 000	1	1	2	2 000	2 000	2 875
6	Author	—	B.B.C.	For Swiss Federal Exhibition at Berne	1914	5 000	6 250†	5 000	1·5	3 000	1	5 000	0·8	6 250	6 250	1	1	1	5 000	4 080	4 080
7	Author	—	Met.-Vickers	Manchester Corporation	1915	5 500	6 250*	5 000	2·45	3 000	1	5 500	0·85	6 475	6 475	1	1	1	5 000	3 190	3 190
8	Author	—	Met.-Vickers	Newcastle Electric Supply	1915	6 660	7 500*	6 000	1·25	2 400	1	6 600	0·75	8 800	5 620	1	1	1	6 000	5 370	3 430
9	Author	—	B.B.C.	Geertruidenberg	1916	6 000	7 500†	6 000	1·5	3 000	1	6 000	0·8	7 500	7 500	1	1	1	6 000	4 900	4 900
10	*Elec. Review*	9/7/20	Parsons	Newcastle Electric Supply, Carville	1916	11 000	—	10 000	1	2 400	1	11 000	0·75	14 680	9 400	2	1	2	5 000	5 000	3 200
11	Author	—	Met.-Vickers	South Metropolitan	1917	5 500	6 250*	5 000	1·25	3 000	1	5 500	1·0	5 500	5 500	1	1	1	5 000	4 475	4 475
12	Author	—	B.B.C.	Heerlen	1918	6 800	8 500†	6 800	1·5	3 000	1	6 800	0·8	8 500	8 500	1	1	1	6 800	5 550	5 550
13	Author	—	Westinghouse	Sand Springs, Ltd.	1919	6 250	6 250*	5 000	2	3 600	1	6 250	0·8	7 800	11 200	1	1	2	2 500	1 765	2 520
14	Author	—	B.B.C.	Heerlen	1919	10 000	12 500†	10 000	1·5	3 000	1	10 000	0·8	12 500	12 500	1	1	1	10 000	8 160	8 160
15	*Proc. A.I.E.E.*	Nov. 1919	G.E.	Regina	1920	5 000	5 000*	4 500	1·5	3 600	1	5 000	0·8	6 250	9 000	1	1	1	4 500	3 670	5 300
16	Author	—	B.B.C.	Amsterdam	1920	12 000	16 000*	12 000	1·2	3 000	1	12 000	0·8	15 000	15 000	1	1	1	12 000	10 950	10 950
17	Author	—	A.E.G.	Amsterdam	1920	13 500	16 000*	12 000	1·2	3 000	1	13 500	0·8	16 900	16 900	1	1	1	12 000	10 950	10 950
18	Author	—	Met.-Vickers	Liverpool Corporation	1920	13 750	15 625*	12 500	2·5	3 000	1	13 750	0·8	17 200	17 200	1	1	1	12 500	7 900	7 900
19	Author	—	Met.-Vickers	Practice in 1920	1920	12 800	—	10 250	0·75	3 000	2	6 400	0·75	8 550	8 550	1	1	1	10 250	11 800	11 800
20	Author	—	Parsons	Mersey Power	1921	12 500	—	10 000	1	3 000	1	12 500	0·8	15 620	15 620	1	1	2	5 000	5 000	5 000
21	Author	—	Met.-Vickers	Stepney	1921	11 000	12 500*	10 000	1·4	3 000	1	11 000	0·8	13 750	13 750	1	1	1	10 000	8 450	8 150
22	Author	—	Met.-Vickers	Sydney Corporation	1921	13 200	15 000*	12 000	2	3 000	1	13 200	0·8	16 500	16 500	1	1	1	12 000	8 475	8 475
23	Author	—	A.E.G.	Proposed practice for 1921	—	16 000	16 000	12 000	1·2	3 000	1	16 000	0·8	20 000	20 000	1	1	1	12 000	10 950	10 950
24	Author	—	B.B.C.	Proposed practice for 1921	—	15 000	15 000	12 000	1·2	3 000	1	15 000	0·8	18 750	18 750	1	1	1	12 000	10 950	10 950
25	Author	—	Met.-Vickers	Proposed practice for 1921	—	16 000	16 000	12 500	1·2	3 000	1	16 000	0·8	20 000	20 000	1	1	1	12 500	11 400	11 400
26	*Proc. A.I.E.E.*	Nov. 1910	G.E.	Proposed practice for 1921	—	7 500	—	—	—	3 600	—	7 500	0·8	9 375	13 500	—	—	—	—	—	—

gear, up to units of 4 000 kW, and for larger units by turbo-alternators in conjunction with rotary converters. The development of the reduction gear is also responsible for the disappearance of the direct-driven 25-period alternator running at 1 500 r.p.m. up to capacities of 4 000 kW.

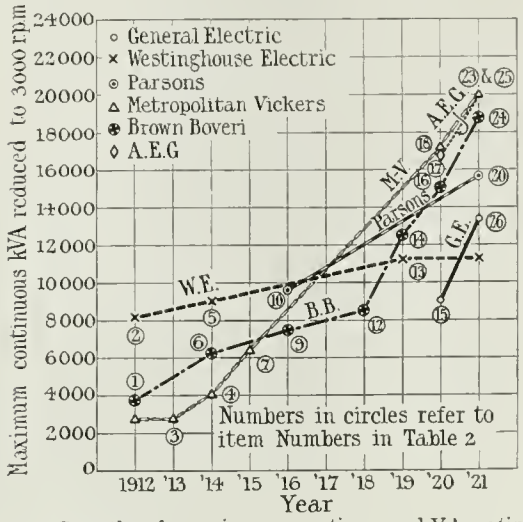


Fig. 2.—Growth of maximum continuous kVA rating of large high-speed alternators, reduced to 3 000 r.p.m.

The mechanical reduction gear has completely revolutionized turbine practice for small outputs, and the time is not far distant when even the 1 000-kW 3 000-r.p.m. turbo-alternator will be replaced by a turbine running at a high speed and driving the alternator through gears.

gear is fully justified by the higher efficiency of the high-speed turbine and its lower cost. Whilst the gear is lagging behind the turbine in the race for maximum power, the fact must not be lost sight of that the gear is in the early stage of its development.

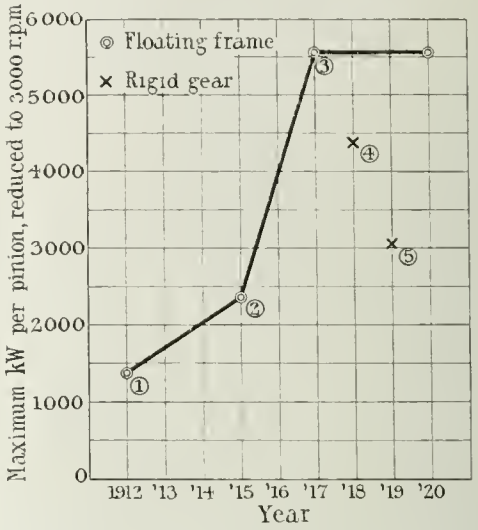


Fig. 3.—Maximum power transmitted by one pinion, reduced to 3,000 r.p.m., given in kW at generator terminals.
(Numbers in circles refer to item numbers in Table 3.)

Table 3 shows the development of the reduction gear in respect of the maximum power transmitted per pinion, reduced to a speed of 3 000 r.p.m., and Fig. 3 shows the development diagrammatically.

The most economical arrangement of the geared turbine for large capacities is the "two-pinion"

Table 3.

Maximum Power transmitted by One Pinion.

Reduced to 3 000 r.p.m. given in kW at Generator Terminals.

Item No.	Reference and Date		Manufacturer	Type	Customer	Year Installed	Maximum Continuous Rating		Speed r.p.m.	kW reduced to 3 000 r.p.m.
							B.H.P.	kW at Generator Terminals		
1	*Engineering*	26/5/16	Westinghouse Machine Co.	Floating Frame	Cleveland Electric Illuminating Co.	1912	5 500	3 850	1 800	1 380
2	Author	—	Westinghouse Machine Co.	Floating Frame	Metropolitan-Vickers, Trafford Park	1915	2 140	1 500	3 750	2 350
3	*Engineering*	26/5/16	Westinghouse Machine Co.	Floating Frame	Swedish Twin Screw Cruiser (H.P. Turbine)	1917	5 500	3 850	3 600	5 550
4	Tostevin before I.N.A.	26/3/20	—	Rigid Gear	Destroyer "Trinidad" (H.P. Turbine)	1918	6 250	4 380	3 000	4 380
	Engineering	9/4/20								
5	Tostevin before I.N.A.	26/3/20	John Brown & Co.	Rigid Gear	Battle Cruiser "Hood" (H.P. Turbine)	1919	17 500	12 250	1 497	3 060
	Engineering	9/4/20								

As in the case of the alternator, the development of the reduction gears towards a maximum possible output at a given speed is justified only from the turbine point of view. For a given speed in r.p.m. of the low-speed shaft, the cost of the gear increases with increasing pinion speed, but the more expensive arrangement, one pinion being driven by the high-pressure turbine, and the other by the low-pressure turbine. In such a case the output which can be transmitted through the pinions very nearly approaches the maximum output which can be obtained from the turbine when designed for a 29-inch vacuum.

The importance of the reduction gear for the future developments of turbine practice is fully realized, but the author does not propose to deal with this question any further in this paper.

In reviewing the general trend of turbine development during the last 10 years, reference must be made to the increase in size of units quite apart from the speed of the turbine. In view of the increase in the size of power stations, there has been a considerable demand for turbines of large capacities. In this country, progress in this direction has been retarded mainly on account of the war, and the development of large units has taken place mainly in the United States.

Table 1 gives full particulars of the turbine development during the past 10 years. From the designer's point of view the development of the economical rating for each exhaust is of particular interest. This is given in columns 19 to 21. The rating reduced to a basis of 1 inch abs. back pressure and a speed of 1 500 r.p.m. is shown diagrammatically in Fig. 4. For the reduction to this basis the following relationships are applied :

(*a*) The rating varies as the square root of the back pressure.

(*b*) The rating varies inversely as the square of the speed (r.p.m.).

The turbines with the largest capacities for a single exhaust are to-day as shown in Table 4.

Table 2 gives corresponding particulars of high-speed turbines (above 2 000 r.p.m.) of relatively large capacities during the same period, and Fig. 5 shows the development diagrammatically.

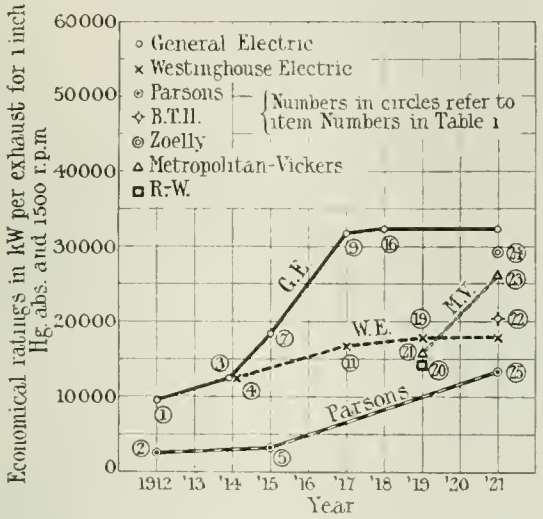


Fig. 4.—Growth of economical rating of large turbines in kW per exhaust, reduced to a vacuum of 1 inch abs. (Hg) and 1 500 r.p.m.

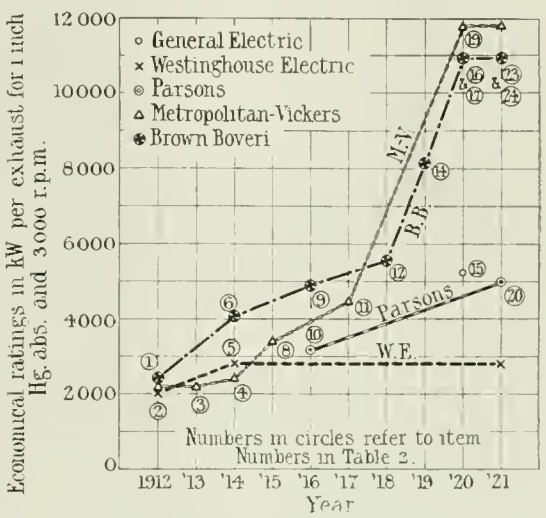


Fig. 5.—Growth of economical rating of large high-speed turbines in kW per exhaust, reduced to a vacuum of 1 inch abs. (Hg) and 3 000 r.p.m.

(3) Development of the Reaction Turbine.

The principal manufacturers of the reaction turbine are Parsons, Brown Boveri, and the Westinghouse Electric and Manufacturing Co., Ltd.

Table 4.

Reference to Table 1	Type	Installation	Year Installed	Economical Rating	Vacuum, Absolute	Speed	Reduced Rating
				kW	in.	r.p.m.	kW
16	G.E.	Chicago	1918	22 500	1	1 800	32 400
24	Zoelly	Paris	1921	30 000	1·05	1 500	29 200
23	Metropolitan-Vickers Multi-exhaust	Manchester	1921	25 000	0·9	1 500	26 400

In 1910 the combined Curtis-Parsons turbine was introduced by Brown Boveri, and this type has since become their standard design. It is built as a single-cylinder unit in every case.

Fig. 6 shows a 10 000-kW turbine manufactured by Brown Boveri and installed at Pedro Mendoza station (Buenos Ayres). The low-pressure end is built up of

discs, the peripheral speed of the rotor being too high for use on drums.

In this country, and in America, the development of two cylinders are arranged in tandem for driving a single generator. In order to cope with large volumes of exhaust steam, the low-pressure element is generally

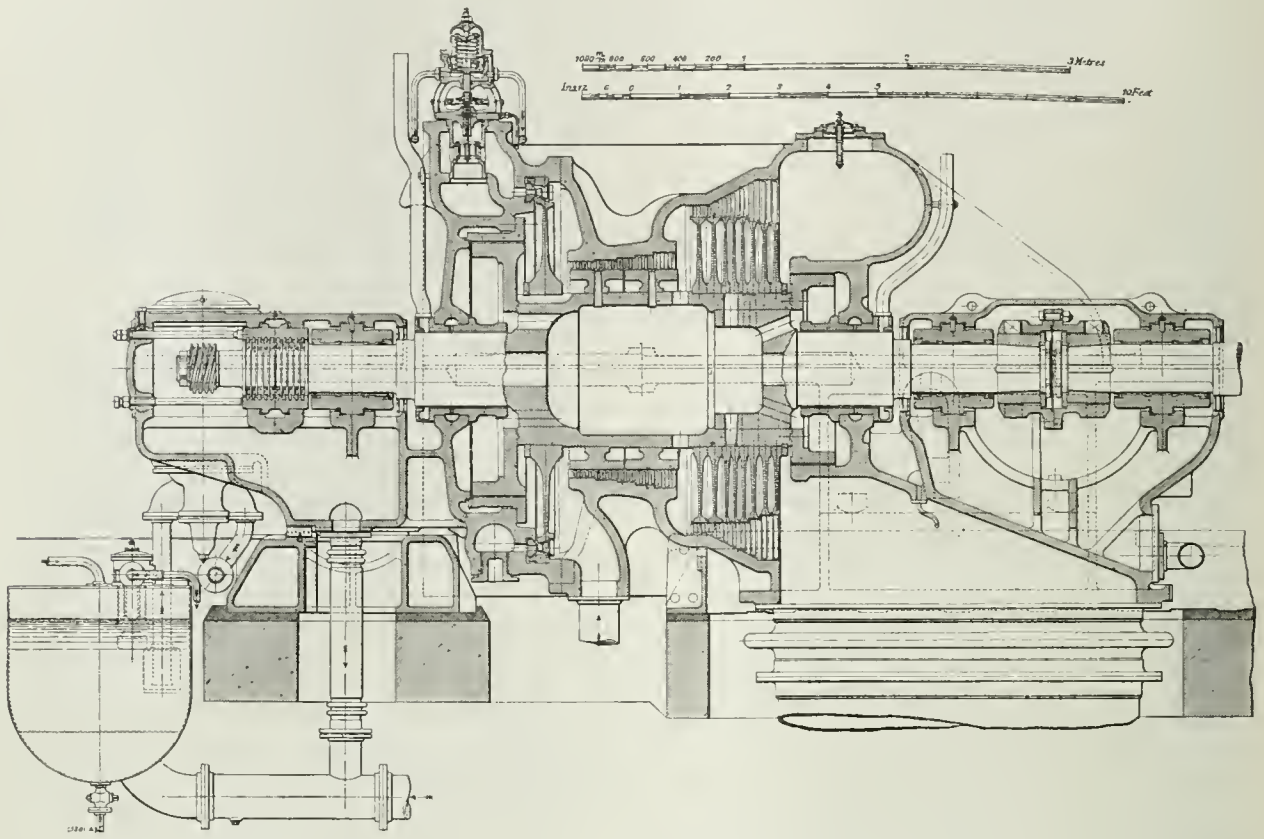


FIG. 6.—Brown Boveri turbine (1918), 10 000 kW maximum continuous rating at 1 500 r.p.m.

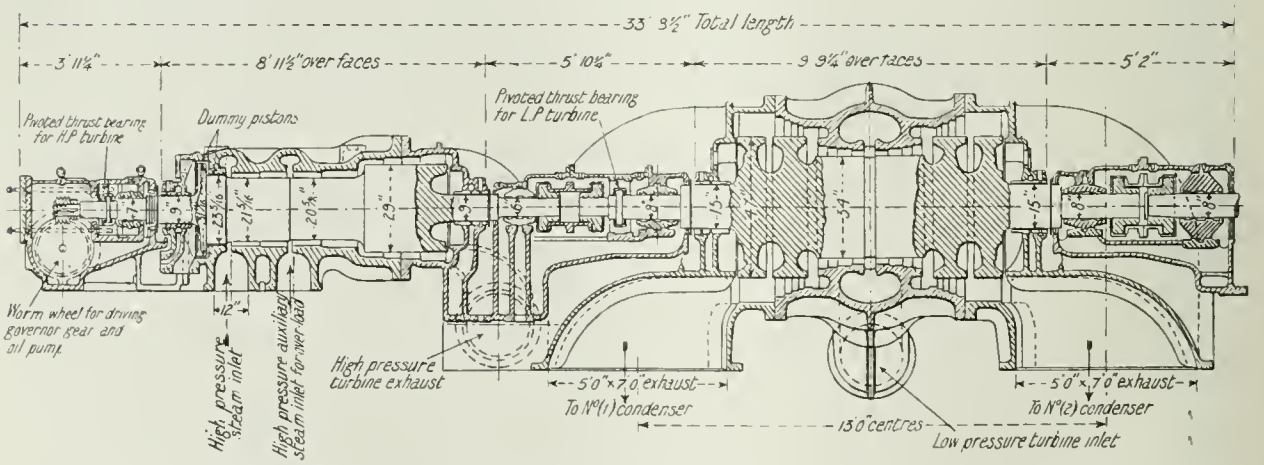

33' 3½" Total length
3' 11¼"
8' 11½" over faces
5' 10¼"
9' 9¼" over faces
5' 2"
Pivoted thrust bearing for H.P turbine
Dummy pistons
Pivoted thrust bearing for L.P turbine
Worm wheel for driving governor gear and oil pump
High pressure steam inlet
High pressure auxiliary steam inlet for over-load
High pressure turbine exhaust
5'0" x 7'0" exhaust
To N°(1) condenser
15'0" centres
5'0" x 7'0" exhaust
To N°(2) condenser
Low pressure turbine inlet

FIG. 7.—Parsons tandem reaction turbine (1916), 11 000 kW maximum continuous rating at 2 400 r.p.m. (Table 2, item 10).

the reaction turbine has proceeded on different lines. Parsons in this country standardized the two-cylinder design for large capacities, the high- and low-pressure elements being arranged in separate cylinders. The of the double-flow type. Fig. 7 shows a representative design of the large Parsons turbine.

The Westinghouse Electric and Manufacturing Co., Ltd., in America have further developed the idea of

sub-dividing the turbine into separate cylinders. In order to improve the efficiency of the turbine and to deal with the demand for increased capacities, some of their units were built for driving more than one generator, thus evolving the multi-shaft arrangement.

The development of the turbine practice of the Westinghouse Electric and Manufacturing Company has been fully described by Hodgkinson,* and full particulars with regard to the various arrangements used are given in Table 5. A turbine of the tandem compound type is shown in Fig. 8.

During the period under review, the Ljungström turbine has been put on the market. The first turbine of 1 000-kW capacity was installed in 1912 by the North Metropolitan Power Company, London, and since then considerable progress has been made with regard to the increase in capacity per unit. Table 6 gives a summary of this development.

A Ljungström turbine of 30 000-kW maximum con-

TABLE 5.

Development of Large Turbines made by the Westinghouse Electric and Manufacturing Co., Ltd.

1	2	3	4	5	6	7	8	9	10	11	12	13
Item No.	Reference to Table 1	Customer and Power Station	Year Installed	Rating in kW		Vacuum at Economical Rating, in. abs. (Hg)	Speed, r.p.m.	No. of Generators	No. of Cylinders	No. of Shafts	No. of Exhausts	Arrangement of Turbines
				Max. Continuous	Economical							
1	4	Interboro' Rapid T., 74th Street, New York	1914	30 000	25 000	1	1 500/750	2	2	2	2	Cross compound
2	10	Commonwealth, North-west Chicago	1917	30 000	25 000	1	1 200	1	2	1	2	Tandem compound
3	11	Providence, Narragansett..	1917	40 000	33 000	2	1 800/1 200	2	2	2	2	Cross compound
4	13	Interboro' Rapid T., 74th Street, New York	1918	60 000	40 000	1	1 500	3	3	3	4	Triple cylinder, cross compound
5	14	Commonwealth, Fisk Street, Chicago	1918	35 000	25 000	1	1 500	1	2	1	2	Tandem compound
6	19	Muscle Shoals	1919	60 000	50 000	1	1 800/1 200	3	3	3	4	Triple cylinder, cross compound

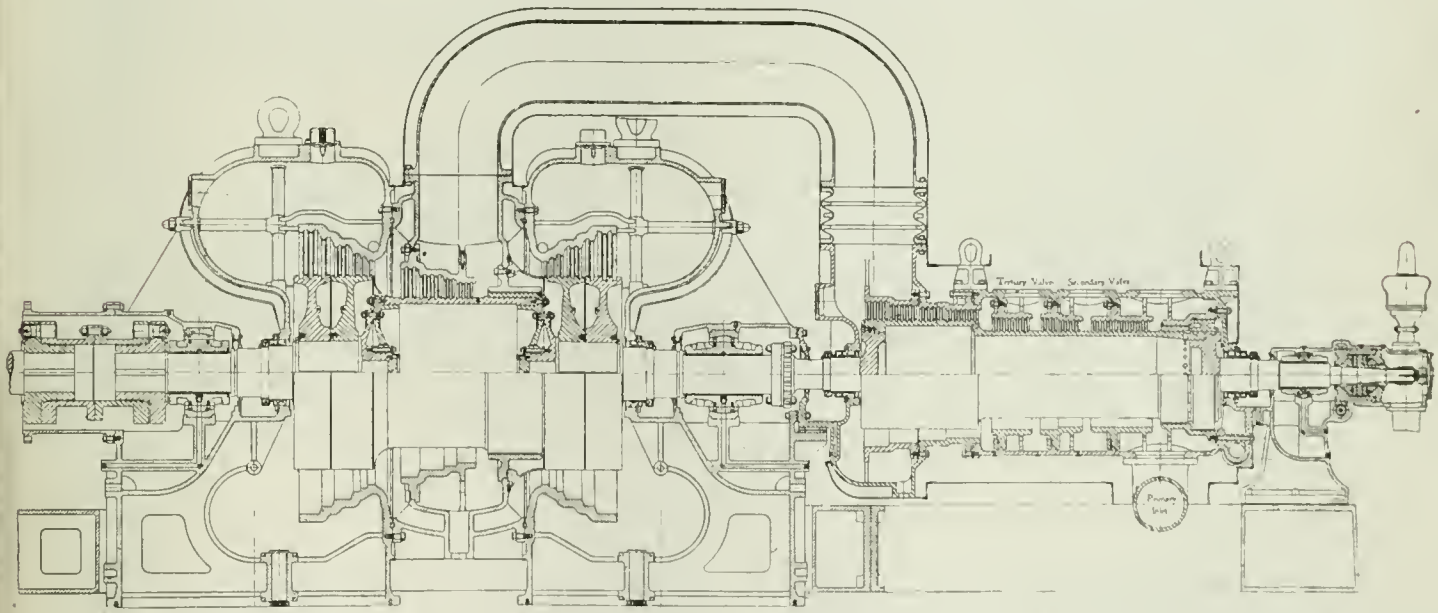


FIG. 8.—Westinghouse tandem turbine (1918), 35 000 kW maximum continuous rating at 1 500 r.p.m. (Table 1, item 14).

* *Electric Journal*, 1918, vol. 15, p. 57.

tinuous capacity is at present under construction in the works of the General Electric Company at Schenectady.

(4) Development of the Impulse Turbine.

A conspicuous feature of the development of the impulse turbine is the increase in the output per cylinder. The firm prominent in this connection is the General Electric Company of America, as is clearly shown in Table 1 and Fig. 1.

It is interesting to note that the first 30 000-kW turbine (Fig. 9), which was built in 1914, is of the two-cylinder type, and that very shortly after this machine was built a 35 000-kW maximum continuous rated turbine, single-cylinder type, was developed (see Fig. 10). The necessary leaving area in this single-cylinder unit has been obtained by increasing the diameters of the last stages and increasing the length of the blades. In comparing the two designs, which are shown to the same scale, one is immediately struck with the great difference in overall length, and the additional cost which is involved in the two-cylinder construction. Both factors are no doubt responsible for the early change-over from the two-cylinder to the single-cylinder construction. Experience has shown, however, that the single-cylinder construction shown in Fig. 10 possessed serious inherent disadvantages which are due to disc and blade vibrations, resulting from the large diameter adopted for the last disc.

Table 6.

Development of the Brush-Ljungström Turbine.

1	2	3	4	5	6	7	8	9
Item No.	Customer and Power Station	Year Installed	Rating in kW			Vacuum at Economic Rating, in. abs. (Hg)	Speed, r.p.m.	Power Factor
			Max. Continuous	Overload for Two Hours	Economical			
1	North Met. E.P.S. Co., Willesden ..	1912	1 000	1 250	1 000	2	3 000	0·8
2	Marshall & Sons, Gainsborough ..	1918	1 500	1 875	1 500	$1\frac{1}{4}$	3 000	0·8
3	Ipswich Corporation, Ipswich ..	1919	3 000	3 750	3 000	2	3 000	0·8
4	Luton Corporation, Luton	1921	5 000	6 250	5 000	2	3 000	0·8
5	Stockport Corporation, Stockport ..	1921	5 500	6 050	5 500	$1\frac{1}{2}$	3 000	0·8

Some interesting developments of the A.E.G. turbine are shown in Figs. 11 and 12. Fig. 11 shows a 3 000-r.p.m. turbine of large capacity in one cylinder. The last stage is arranged on the double-flow principle, the exhausts from the two halves facing one another. The steam passages from the last but one row to one half of the last stage are arranged around the last stage, and the exhaust steam is caused to flow around these passages before entering the exhaust proper. The flow of steam outside the last stage is very complicated, and will result in a considerable drop in pressure between the last row of blades and the condenser. There will also be a drop in pressure between the last but one stage and the extreme half of the last stage.

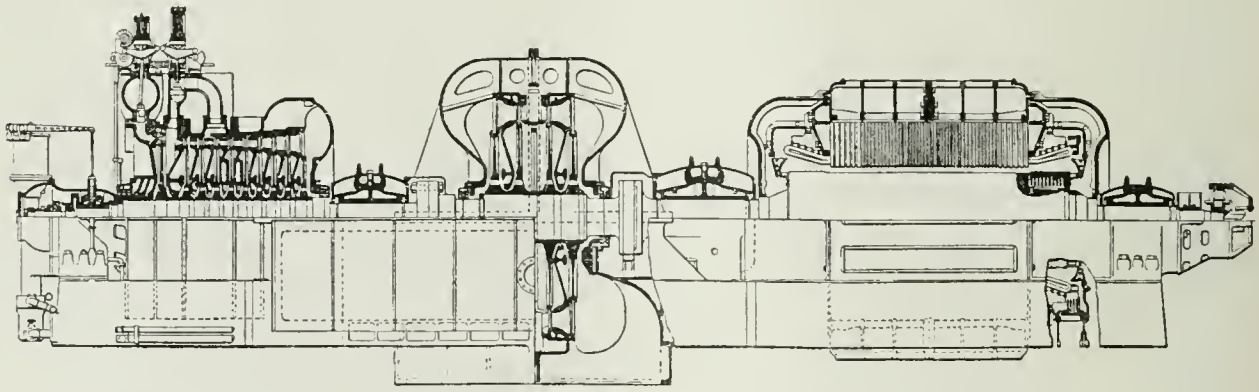


Fig. 9.—General Electric tandem turbine (1914), 30 000 kW maximum continuous rating at 1 500 r.p.m. (Table 1, item 3).

This turbine is of the four-bearing design, a flexible coupling being provided between the turbine and the generator or compressor shaft.

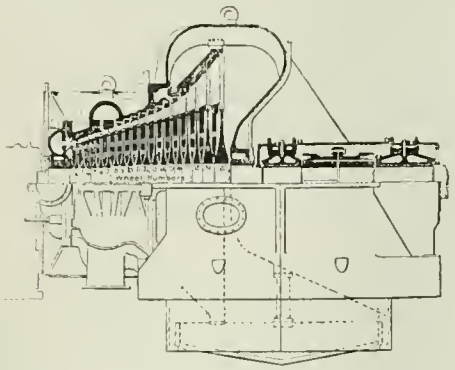


FIG. 10.—General Electric turbine (1917), 35 000 kW maximum continuous rating at 1 500 r.p.m. (Table 1, item 8).

Fig. 12 shows a 16 000-kW turbine designed to run at 1 500 r.p.m. A number of these sets have been built by the A.E.G. for large super-power stations such as Golpa (see *Zeitschrift des Vereines Deutscher Ingenieure*, 1919, vol. 65, p. 1145). This design is conspicuous for its small number of stages and for the large diameter of the last three stages. The length of the blades on the last wheel is very small, the ratio of blade length to mean diameter being only 1 : 9. The shape of the turbine exhaust is very unsatisfactory from the point of view of flow of steam through the exhaust, inasmuch as 50 per cent of the exhaust opening is covered by the turbine cylinder which is overhung into the turbine exhaust. This turbine is of the usual A.E.G. three-bearing construction.

During the period under review no designs of Zoelly turbines which differ materially from the construction described in his paper of 1912, have come to the author's notice. It has been reported that 10 000-kW turbines running at 3 000 r.p.m. are under construction, but no details of designs have been published.

Modern Rateau turbines as built by the Metropolitan-Vickers Electrical Co. are shown in Figs. 13, 14 and 15. Fig. 13 shows a 18 750-kW turbine of the single-flow type designed for a speed of 1 500 r.p.m., and for a vacuum of 29 inches. A turbine of this type and size is installed at the Yoker power station of the Clyde Valley Electrical Power Co.

The Metropolitan-Vickers multi-exhaust turbine is shown in Figs. 14 and 15, which also show other novel features.

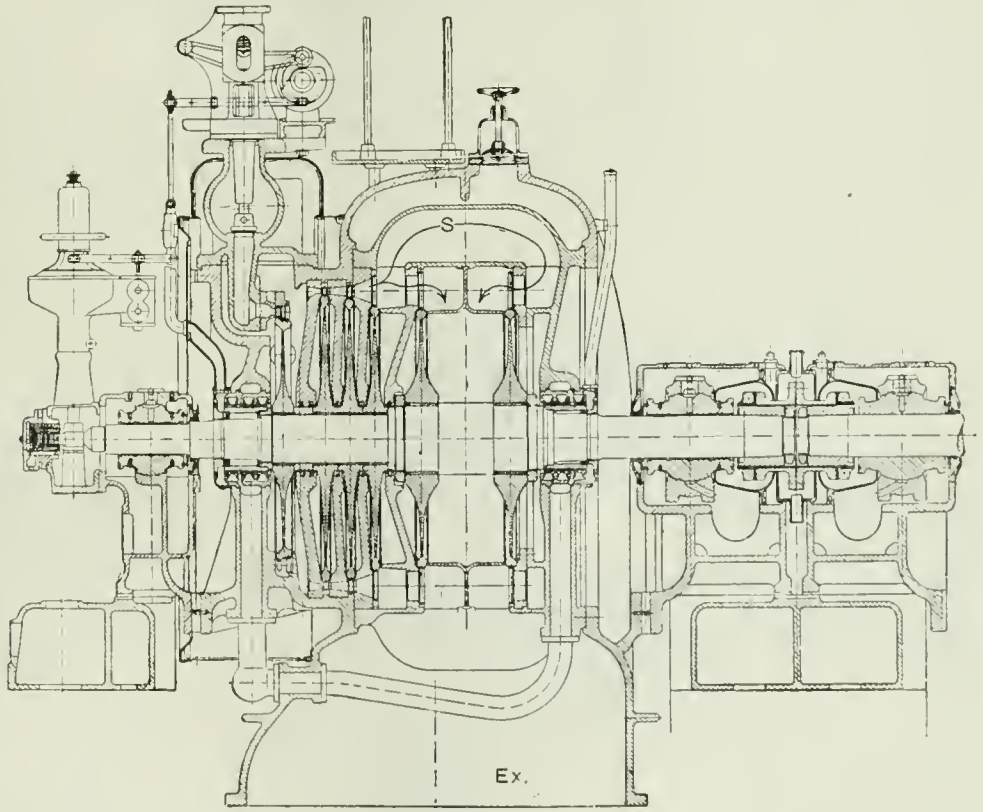


FIG. 11.—A.E.G. high-speed double-flow turbine.

(1) *Stream-line exhaust.*—This type of exhaust was introduced to secure a simple flow of the steam from the last row of blades to the condenser, thus reducing to a minimum the pressure-drop between the last row of turbine

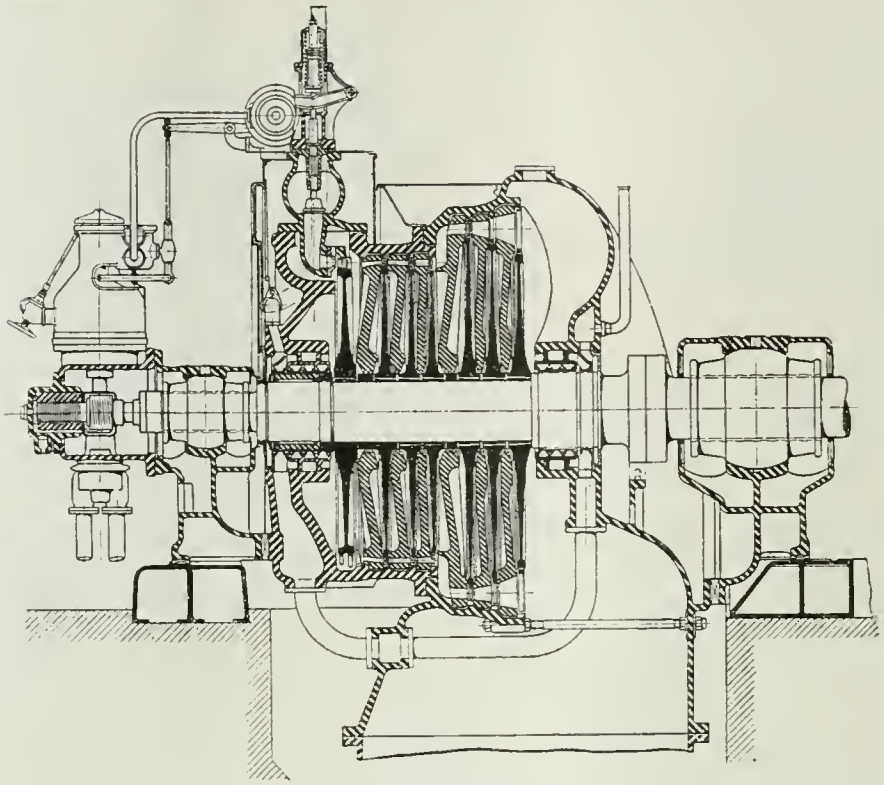


FIG. 12.—A.E.G. turbine (1919), 16 000 kW maximum continuous rating at 1 500 r.p.m., as supplied to Zschornewitz (Golpa).

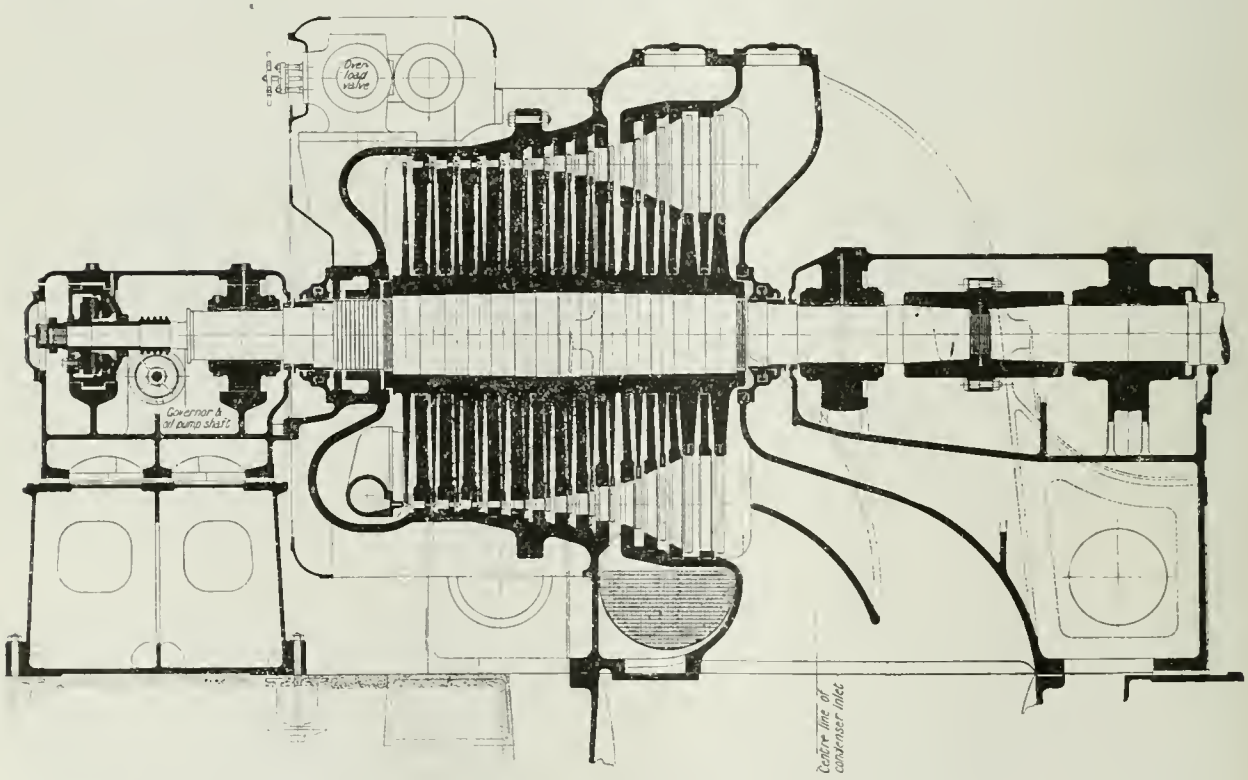


FIG. 13.—Metropolitan-Vickers turbine (1919), 18 750 kW maximum continuous rating at 1 500 r.p.m., as supplied to the Clyde Valley Electrical Power Company (Yoker and Clydes Mill).

blades and the condenser. The stream-line exhaust also ensures uniform distribution of steam over the whole condenser-tube surface, thereby effecting a reduction of pressure-drop in the condenser itself.

(2) *Internal feed-water heater.*—A surface-type feed-water heater is arranged within the turbine cylinder, the tubes in which the condensate is heated being exposed to steam which is tapped from the main turbine at a relatively low vacuum. The incorporation of the heater as an integral part of the turbine avoids the necessity of additional heater casings and pipe connections which would complicate the lay-out of the plant to a considerable extent.

(3) *Construction of exhaust casing.*—The exhaust casing is rigid in construction and is placed directly on the foundations, thereby dispensing with the use of heavy foundation girders. The condenser inlet flange is bolted directly to the turbine cylinder. The high-pressure end of the turbine is flexibly connected to the rigid portion of the casing, the extreme end of the high-pressure cylinder being supported from a pedestal. In the old-fashioned design heavy foundation girders are usually required to provide side supports for the turbine. Such girders not only increase the cost of foundations, but also interfere seriously with the accessibility of the joint between the turbine and the condenser. The design indicated in Figs. 14 and 15 eliminates these disadvantages.

(4) *Nozzle-box and steam-chest design.*—The pipe type of nozzle box and steam chest has been evolved and combined with nozzle control, the design being such that the highest steam pressures and temperatures can be used without fear of distortion of the turbine cylinder due to variations in load and steam temperature.

Further details of these designs will be fully described in another part of the paper.

TABLE 7.

	Number of Firms making Turbines	Number of Turbines made	Output	Average Size of Turbines
			kW	kW
Total	39	1 150	1 900 000	1 650
Impulse type	30	900	1 400 000	1 550
Impulse in percentage of total	77 %	78 %	75·7 %	—
Reaction type	9	250	500 000	2 000
Reaction in percentage of total	23 %	22 %	26·3 %	—

TABLE 8.

Turbine Orders in Great Britain for 1914, 1916 *and* 1919.

Turbines above 200-kW Capacity.

1	2	3	4	5	6	7	8	9	10	11
Item No.		Number of Turbines			Output in kW			Average Size of Turbine in kW		
		1914	1916	1919	1914	1916	1919	1914	1916	1919
1	Total, all types	143	167	189	237 500	344 000	523 975	1 660	2 060	2 770
2	Impulse	112	136	143	185 500	254 000	411 568	1 655	1 865	2 870
3	Impulse in percentage of total	78·4%	81·5%	75·7%	78·1%	73·9%	78·5%	—	—	—
4	Reaction	31	31	46	52 000	90 000	112 407	1 680	2 900	2 440
5	Reaction in percentage of total	21·6%	18·5%	24·3%	21·9%	26·1%	21·5%	—	—	—

(5) Comparison of Commercial Development.

It is interesting to compare the commercial development of the impulse turbine with that of the reaction turbine. The author estimates that prior to the war the position, as far as the European countries are concerned, was roughly as shown in Table 7.

Low-pressure turbines used in conjunction with existing engines have practically disappeared, the tendency being to replace engines by high-pressure turbines, which are more economical than combined engines and low-pressure turbines.

The demand for mixed-pressure turbines is still considerable, but there is a distinct falling off in enquiries in view of recent developments in the use of electricity for winders and rolling mills. Modern design practice for the mixed-pressure turbine is practically the same as in 1912, the only development being towards slightly larger sizes at 3 000 r.p.m., so that all capacities commercially required can be obtained with turbines running at 3 000 r.p.m.

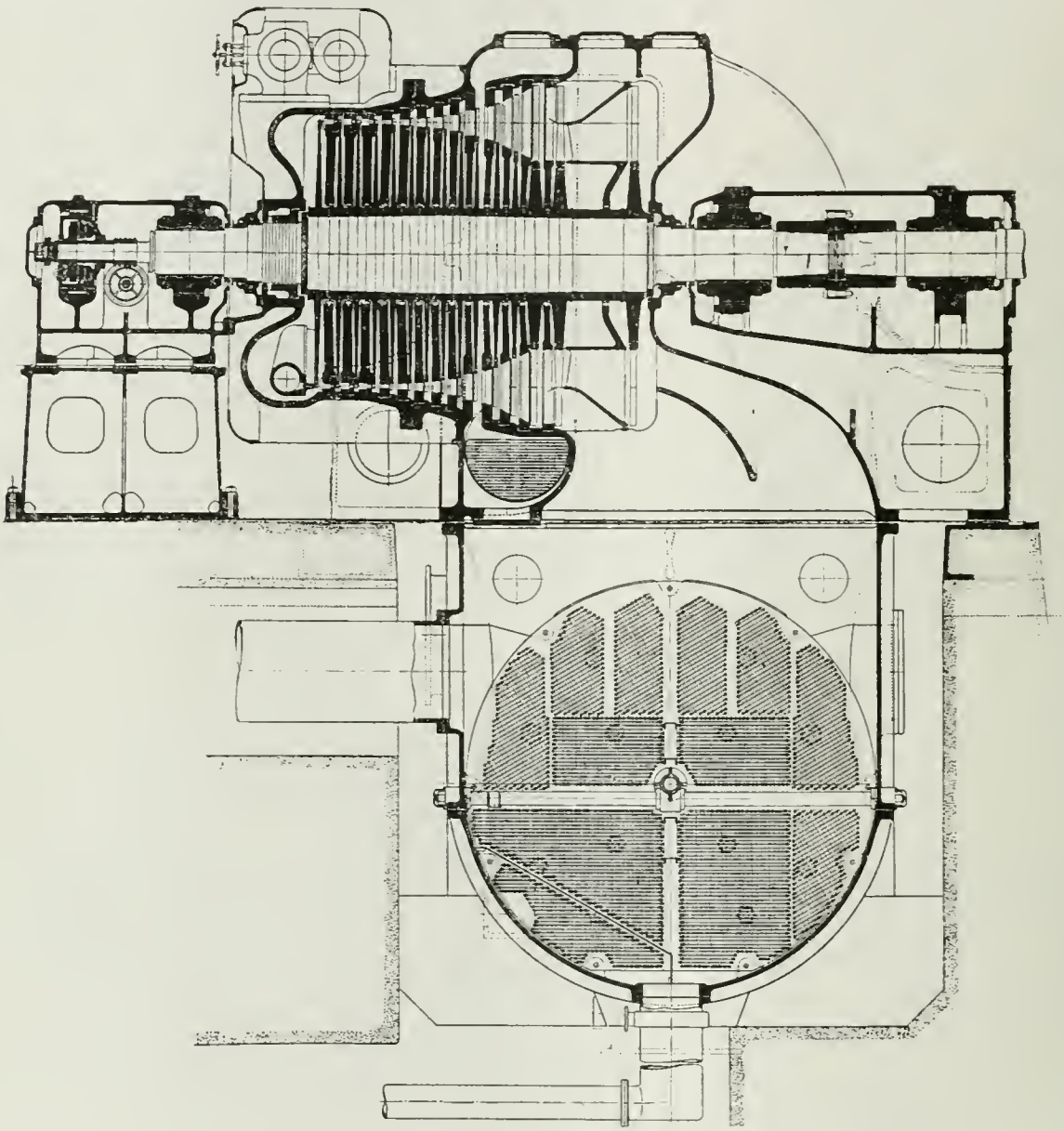


Fig. 14.—Metropolitan-Vickers turbine with heater and condenser (1919), 18 750 kW maximum continuous rating at 1 500 r.p.m, as supplied to Glasgow Corporation (Dalmarnock).

Table 8 gives similar approximate information as regards this country for 1914, 1916 and 1919.

(6) Development of Turbines for Special Purposes.

(*a*) *Low-pressure and mixed-pressure turbines.*—The demand for these turbines has considerably decreased.

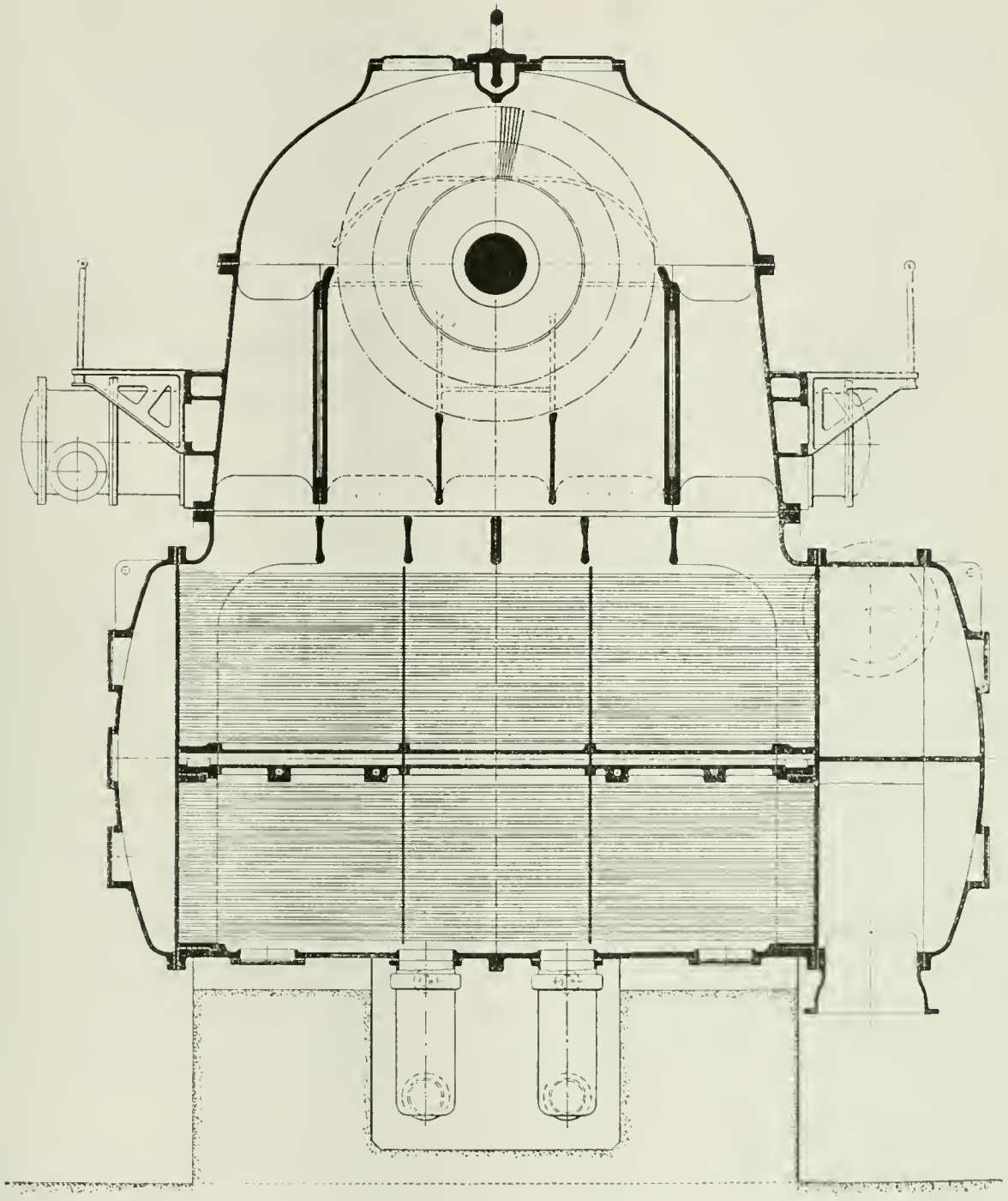


FIG. 15.—Metropolitan-Vickers turbine with heater and condenser (1919), 18 750 kW maximum continuous rating at 1 500 r.p.m., as supplied to Glasgow Corporation (Dalmarnock).

(*b*) *Back-pressure and reducing turbines.*—There has been an increased application of these turbines to mills and factories. The sizes of units required are moderate, and the practice has not changed appreciably during the period under review. The application of reducing turbines has been fully dealt with elsewhere.*

* H. L. GUY: "Steam Turbines for Land Purposes," paper before the Manchester Association of Engineers 27 January, 1917 (see *Engineer* 1917 vol. 123, p. 107).

PART II.

IMPORTANT FACTORS AFFECTING THE DEVELOPMENT OF LARGE TURBINES.

TABLE OF CONTENTS.

(1) Reliability.
- (*a*) Stresses, factor of safety.
- (*b*) Vibration.
- (*c*) Stresses due to uneven distribution of temperature.
- (*d*) Materials.
- (*e*) Care during manufacture and erection.

(2) Working conditions.
- (*a*) Progress made in steam conditions.
- (*b*) Basis of calculations for the improvement in heat consumption possible with improved steam conditions.
- (*c*) Improvement due to higher steam pressures.
- (*d*) Improvement due to higher total steam temperatures.
- (*e*) Improvement due to higher vacua.
- (*f*) Improvement due to reheating.
- (*g*) Improvement due to feed-water heating.

(3) Thermodynamic efficiency.

The principal factors which influence turbine design are summarized in their order of importance as follows:

(1) Reliability.
(2) Working conditions.
(3) Thermodynamic efficiency.
(4) Overall dimensions, weights and costs.

Whilst the factors under (4) are quite important, it is not proposed to deal with these in detail at this stage.

(1) RELIABILITY.

The effect of the failure of a large turbine on the running of a large power station may be so far-reaching that the engineer responsible for the purchase and the running of large power stations should be fully conversant with those points in the design of the turbine which affect the reliability of the turbine itself and consequently that of the station as a whole.

In view of the large number of turbine breakdowns which have occurred during recent years, and in view of the considerable financial losses which many of the failures must have entailed, the opinion expressed in many quarters (see, for instance, *Power*, 6 April, 1920, p. 557) that plant users should have a voice in the design of large turbines can be fully appreciated. There is no doubt that in many cases the losses which the manufacturer incurs due to an accident are much smaller than those which the user suffers.

It is the author's intention to discuss all the points in the design which affect its reliability, and to give such data as will enable the power-station engineer to study and appreciate the merits of any particular turbine design.

The maximum size of turbine which can be used for a given size of power station depends to a great extent on the reliability of the machine. The greater the reliability of the turbine available, the larger may be the capacity. If the larger capacity can be obtained only at the sacrifice of reliability, it would obviously be bad engineering to utilize such larger size. Even if the reliability of the different sizes available is the same, the power station itself will be less reliable with a small number of large units than with a larger number of smaller units. It is evident, therefore, that in order to maintain the reliability of the power station at a given standard, the larger machines should be more reliable individually than the smaller machines. The adoption or otherwise of large turbines should be settled only by the recognition of this principle. Any so-called progress in turbine capacities should be carefully examined from the point of view of reliability. A step in the wrong direction may cause failure, which is bound to retard further progress.

The reliability of a turbine depends on the following factors:

(*a*) The stress to which the material is subjected.
(*b*) The magnitude and type of vibration to which the different parts of the machine are subjected.
(*c*) The stresses due to uneven distribution of temperature.
(*d*) The reliability of the material itself.
(*e*) Care during manufacture and erection.

(*a*) *Stresses and factor of safety.*—In his earlier paper (*Journal I.E.E.*, 1912, vol. 48, p. 799) the author suggested that the maximum stresses should not exceed one-third of the elastic limit or about one-fifth of the tensile strength of the material. In other words—

The factor of safety referred to the elastic limit should not be less than 3; and—

The factor of safety referred to the tensile strength should not be less than 5.

In view of the fact that with modern material the elastic limit may be considerably more than 60 per cent of the tensile strength, and having in mind that the term "elastic limit" has been given a more specific interpretation which renders this elastic limit very difficult to ascertain in ordinary practice, the author suggests that the rule referred to above should be modified as follows:

The mean of the factors of safety referred to the yield point and to the tensile strength should not be less than 4 when running at normal speed—an emergency speed of 15 per cent above the normal being permissible.

This rule will enable the designer to take some advantage of the higher yield point obtaining with special alloy materials. It is not suggested that full advantage should be taken of this higher yield point, because materials having special properties cannot be controlled to the same extent as standard materials with a relatively lower yield point.

The author does not consider it desirable to depart appreciably from these rules in designing large turbines, in spite of the fact that machines of smaller capacities with higher stresses have been in operation satisfactorily for considerable periods. The additional margin of safety for large turbines which adherence to the above

rules will afford is also justified, for the following reason :—

Turbines of large dimensions require large forgings and castings, the homogeneity of which cannot be guaranteed in practice to the same extent as that of smaller pieces. A reasonable case could be made out for the employment of slightly higher stresses in turbines requiring small pieces, as, for instance, turbines designed for a speed of 3 000 r.p.m. ; but it would be highly undesirable to use higher stresses in large turbines running at 1 500 r.p.m. or below.

This contention is fully supported by Dr. W. H. Hatfield and H. M. Duncan, who report in connection with their investigation on turbine steels (North-East Coast Institution of Engineers and Shipbuilders, 19 March, 1920) that :

"The results generally indicate that commercial forgings are of moderately good quality steel, but that the qualities which can be induced in small masses are not obtained nor are they to be expected in large masses."

(*b*) *Vibration.*—The parts which are subject to vibration are principally those which rotate. The critical speed of the turbine shaft is fully dealt with in the author's paper of 1912. The opinion expressed in that paper, to the effect that the critical speed of the shaft should be appreciably above the running speed of the turbine, still holds good, and applies with particular force to large turbines. The risk involved in running a large turbine through the critical speed is too great. It is difficult to put this opinion into practice in the case of large turbo-alternator sets of the three-bearing type, and for this reason : the three-bearing design will have only limited possibilities in connection with large turbines. Further experience with smaller sets has shown that the three-bearing design is more sensitive as regards balance and vibration, as one would expect from purely theoretical considerations.

The vibration of turbine discs constitutes a problem to which the designer has had to devote considerable attention. Several failures of large turbines are attributed to the vibration of discs which, in order to save weight, have been made much too light from the point of view of rigidity. The problem presents no difficulties in the case of small rotors such as are used on 3 000-r.p.m. turbines. Discs having very large diameters such as those which have been used at the exhaust end of large 1 500-r.p.m. single-flow turbines must be very massive if excessive vibration is to be prevented.

Turbine blades are also subject to vibration, and breakages of blades have occurred where the vibration has been so excessive as to cause fatigue of the blading material.

(*c*) *Stresses due to uneven distribution of temperature.*—Some failures have been attributed to uneven distribution of temperature. Here again the difficulties increase with the size of the turbine discs.

Uneven distribution of temperature not only increases the stress but also lowers the natural frequency of vibration. It is quite possible for a disc, which runs without vibration under a steady load, to vibrate excessively during changes of load, the reason for this being that a change of load sets up momentarily uneven temperature distribution which may result in the formation of a natural frequency of vibration corresponding to the running speed of the turbine.

(*d*) *Materials.*—In the construction of turbines of large dimensions, it is highly desirable that material of standard properties should be utilized. For instance, a turbine design which requires disc material of very high tensile strength such as can be obtained only by the use of alloy steel is inferior to a design in which standard carbon steels can be used, provided always that manufacturing costs and steam economy are equal in both cases. Special steels require more careful attention during manufacture, and it is not always possible to make sure that the necessary attention has been given. Again, it is an established fact that whilst such special steels can be produced satisfactorily in small pieces, the successful forging and treating of larger pieces present considerable difficulties. Special alloy steels may be quite suitable for turbines of small dimensions such as the machines designed to run at a speed of 3 000 r.p.m., but they cannot be recommended for turbines of larger dimensions designed for a speed of, say, 1 500 r.p.m.

(*e*) *Care during manufacture.*—It may be claimed that it is possible to attain the degree of special attention during manufacture which is indispensable if special materials are to be successfully employed in the production of large turbines, but it cannot be denied that the turbine which does not require a great amount of skill and attention in the workshops is preferable to a machine the design and construction of which requires special checking at every stage.

The reliability of a turbine depends also to some extent on the manufacturing facilities available. From this point of view a turbine rotor which can be completely assembled in the workshop and shipped complete is to be preferred to a rotor which has to be shipped in parts and assembled on site.

In the latter case the necessary facilities for assembling are difficult to obtain and the work cannot be supervised with the same efficiency as in the workshop. The design of the turbine should be such that parts can be shipped without taking special precautions. Moreover, the parts should be of such a nature that they can be assembled on site by means of the facilities usually provided.

(2) Working Conditions.

Of the various methods by means of which the thermal efficiency of a power station can be improved, those with which we are concerned in this paper fall under two main categories :

(i) Operating conditions which are under the control of the power-station designer.
(ii) Conditions which are under the control of the turbine designer.

Improvement of operating conditions comes under the former category, and improved turbine thermodynamic efficiency is under the control of the turbine designer.

The purchaser fixes the conditions under which the turbine is required to work, and such conditions naturally affect the design of the turbine. At the same time, progress in working conditions is to some extent dependent upon the limiting features of turbine design.

(*a*) *Progress made in steam conditions.*—Table 9 shows the progress made in connection with steam conditions, i.e. pressure, temperature and vacuum. In 1912 the author adopted as standard steam conditions:

180 lb. per square inch (gauge), 150 degrees F. superheat, and 28-inch vacuum;

and the highest conditions which were in use at that period were:

200 lb. per square inch (gauge), 200 degrees F. superheat, and 29-inch vacuum.

At present, steam pressures as high as 350 lb. per square inch (gauge) and temperatures as high as 750° F. This means that the turbines must be made suitable for steam temperatures up to 750° F. to allow for variations from the normal temperature.

(*b*) *Basis of calculations for the improvement in heat consumption possible with improved steam conditions.*—The improvement which is possible with large steam turbines, of a given stage efficiency working with dry steam (i.e. hydraulic efficiency), cannot be found directly from a comparison of the adiabatic heat drop and the heat content per pound of steam, because the thermodynamic efficiency of the turbine depends to a great extent on the steam conditions, for the following reasons:

(A) The efficiency of a stage decreases with the amount of wetness in the steam.
(B) The reheat factor varies considerably with the pressure, superheat and the vacuum.
(C) With a given turbine the velocity ratio u/c_0 varies with the total heat drop (u being the mean peripheral speed and c_0 the theoretical steam velocity).
(D) Leakage and disc-friction losses increase with higher pressures, and leaving losses increase with lower back pressures.

TABLE 9.

Progress made in Steam Conditions.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
						Normal Steam Conditions								
						At Boiler	At Turbine							
Item No.	Reference to Table 1	Reference and Date		Customer and Power Station	Year Installed	Pressure per sq. in. (g)	Pressure per sq. in. (g)	Temperature	Superheat	Vacuum at Economical Rating	Cooling Water Temperature	Difference in Temperature between Steam at Exhaust and Cooling Water Inlet	Turbine Heat Drop per lb.	Exp. Pressure Ratio in Turbine
						lb.	lb.	° F.	° F.	in. abs. (Hg)	° F.	° F.	B.Th.U.	
1	—	*Elec. Review*	28/10/10	Newcastle Elec. Supply Co., Carville	1910	200	190	534	150	1·0	55	24	407·2	418
2	2	*Engineering*	17/10/13	Commonwealth, Fisk St., Chicago	1912	275	250	506	100	1·0	60	19	412·5	540
3	—	*Engineering*	21/1/17	B.T.H. Experimental Plant, Rugby	1915	375	350	700	264	1·5	—	—	459·9	496
4*	—	*Elec. Review*	23/3/17	New Buffalo	1916	300	275	689·4	275	1·0	60	19	465·4	591
5†	—	*Elec. Review*	9/7/20	Newcastle Elec. Supply Co., Carville	1916	270	250	650	244	1·0	55	24	450·2	540
6‡	—	*Power*	22/1/18	Jolliet (Public Service Co.)	1917	325	300	672	250	1·0	—	—	464·7	642
7	—	Author	—	Powell Duffryn	1920	375	350	750	314	2·0	75	26	459·4	372
8	21	Author	—	Glasgow Corporation	1920	275	250	650	244	0·9	55	21	455·2	600
9	—	Author	—	Birmingham Corporation	1921	325	300	700	278	2·0	75	26	437·9	322
10	23	Author	—	Manchester Corporation	1921	375	350	700	264	0·9	55	21	484·7	828
11	24	Author	—	Paris	1921	—	312 to 355	662 to 752	236 to 314	1·05	—	—	461·3 to 492·8	631 to 718

* Difficulties experienced with monel metal valve seats.
† Steam joints are made with Taylor rings.
‡ Pipe flanges welded. Sargent-Van-Stone joints. No difficulties experienced due to high pressure.

are being adopted for new power stations. The vacuum has been increased from 29 inches to 29·1 inches. The heat drop has been increased from 407 to 485 B.Th.U. per lb., i.e. by nearly 20 per cent, and the ratio of expansion has increased from 418 to 828, i.e. in the ratio of 1 : 2.

Higher steam pressures than 350 lb. per square inch are at present not justified except in power stations where economy is of extreme importance, where coal is very expensive and the load factor very high. A steam temperature of 700° F. is the present standard.

In the calculations of the improvements in heat consumption which follow, the heat content and the heat drop have been obtained directly from the formulæ of Professor Callendar, and the stage efficiency for dry steam of all stages has been assumed to be 80 per cent;

and the following rules have been applied with regard to the factors stated above.

(A) The *Thermodynamic Efficiency* is corrected by multiplying the stage efficiency of every stage working with saturated steam by the dryness factor.* This correction can easily be ascertained by comparing factors obtained by multiplying half the wetness at the turbine exhaust by the ratio of work done below the saturation line to total work.

The correction factors for wetness calculated in accordance with the above rule have been plotted in Fig. 16 for steam pressures of 200 lb. to 1 000 lb. per square inch for total temperatures of 688° F. and 788° F. and for back pressures of 0·9 inch (abs.) and 2 inches (abs.).

The correction for wetness depends not only on the superheat, but also on the steam pressure at the inlet of the turbine and the back pressure at the turbine exhaust, and, consequently, it affects the improvements obtainable with higher pressures and higher vacua.†

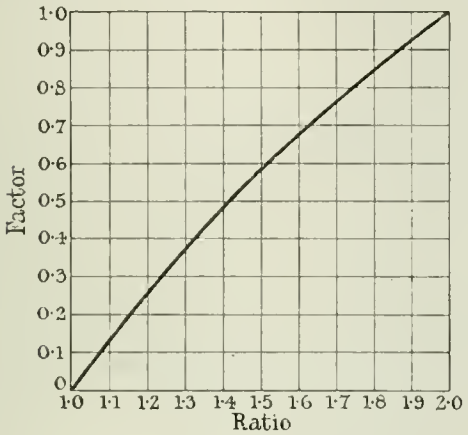


Fig. 15A

The wetness correction can accordingly be divided into three parts:

(*a*) The wetness correction resulting from varying initial pressure.

(*b*) The wetness correction resulting from varying superheat.

(*c*) The wetness correction resulting from varying back pressure.

It is convenient to express the difference in the corrections for any two pressures or superheats by that

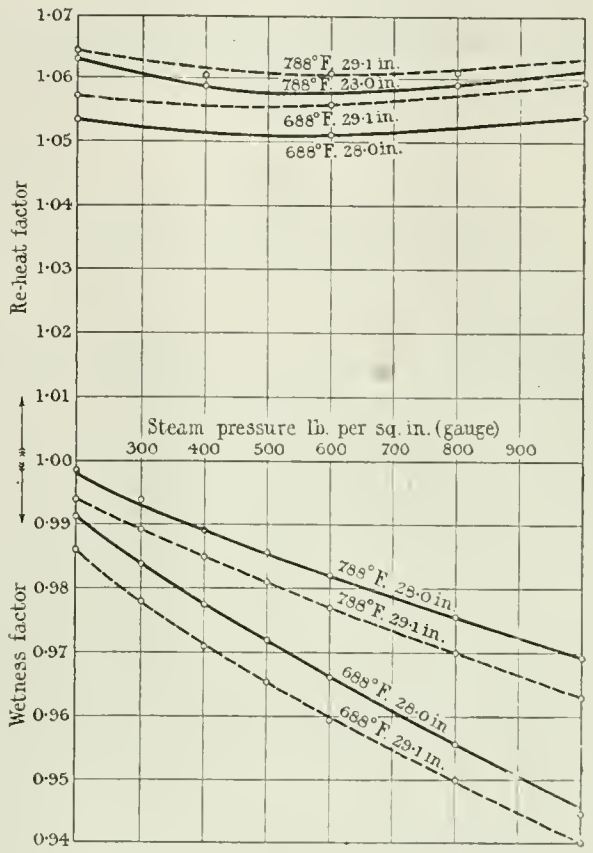


Fig. 16.—Reheat and wetness factors.

difference which would result if the ratio of the two pressures or superheats were increased to 2. The difference for any ratio can be obtained by multiplying the correction for a ratio of 2 by a factor found from the logarithmic curve in Fig. 15A.

On this basis the magnitude of these corrections is approximately as follows:

(*a*) For a ratio of initial pressures of 2 (superheat and back pressure remaining constant), the difference in the wetness correction varies roughly from 0·75 per cent at a pressure of 200 lb. to 0·5 per cent at pressures of 500 to 1 000 lb. per square inch, the thermodynamic efficiency of the whole turbine being lower at the higher pressure.

(*b*) For a superheat ratio of 2 (steam pressure and back pressure remaining constant), the difference in the wetness correction varies roughly from 3 per cent at a superheat of 100 degrees F.

* This correction s n accordance with the author's remarks in the earlier paper (*Journal I.E.E.*, 1912, vol. 48, p. 830), which are to the effect that the efficiency of a turbine will change 1 per cent for each 1 per cent variation in wetness, and that the steam consumption measured as condensed water will be 2 per cent higher for each 1 per cent increase in moisture. The new method of correction for the variation in superheat is more rational for conditions so vastly different from those contemplated in 1912 than the method then proposed by the author, in which the efficiency is corrected for any steam pressure or vacuum by means of a common superheat correction curve. The correction obtained by the new method, together with the corrections for the differences in the reheat factor, gives the correction for superheat, which for ordinary conditions corresponds very closely to the correction calculated from the author's curves given in the earlier paper, after due allowance has been made for the decrease in the ratio u/c_0 with increased superheat.

† Mr. H. M. Martin has very rightly drawn attention to these facts in "A New Theory of the Steam Turbine," published in *Engineering*, 1918, vol. 106, in which he explains the anomalies of the superheat corrections as being the result of undercooling of the steam below the saturation line. The calculations which he proposes deserve careful consideration by practical engineers. For the calculations given in this part of the paper, the author preferred to adhere to the rule outlined above, which was used by him prior to the publication of Mr. Martin's theory and which has been found to give results substantially in accordance with actual test results. As far as can be judged, the improvements calculated in this manner should correspond closely to those which would result from Mr. Martin's theory. The author's rule has the advantage of being simple and easy of application. It would be of considerable interest, however, if the results given here were carefully checked by means of the new theory.

to 2 per cent at a superheat of 300 degrees F., the efficiency being higher at the higher superheat.

(*c*) For a back-pressure ratio of 2 (steam pressure and superheat remaining constant), the difference in the wetness correction is about 0·5 per cent, the efficiency being lower at the lower back pressure.

(B) The *Reheat Factors* have been calculated from the formulæ published by Professor Callendar for both the superheat and the saturated portion of the expansion.* These reheat factors have been plotted in Fig. 16 for pressures of from 200 to 1 000 lb. per square inch (gauge) for total temperatures of 688° F. and 788° F. and for back pressures of 0·9 inch (abs.) and 2 inches (abs.). The reheat factor varies with the steam pressure, the superheat and the back pressure, and consequently the correction of the thermodynamic efficiency of a turbine resulting from variations of the reheat factor can be divided into three parts :

(*a*) The reheat correction resulting from varying pressures.

(*b*) The reheat correction resulting from varying superheats.

(*c*) The reheat correction resulting from varying back pressures.

* H. L. Callendar : "Properties of Steam and Thermodynamic Theory of Turbines" (London Edward Arnold, 1920) ; for superheated steam, formula 17 on page 303 ; for saturated steam, formula 15 on page 301.

The order of these corrections is roughly as follows :

(*a*) For a ratio of initial pressures of 2 (superheat and back pressure remaining constant) the difference in reheat correction varies roughly from 0·25 per cent at a pressure of 200 lb. per square inch (gauge) to 0·5 per cent, at pressures from 500 to 1 000 lb. per square inch (gauge), the thermodynamic efficiency being higher at the higher pressure.

(*b*) For a superheat ratio of 2, the difference in the reheat correction varies roughly from 0·5 per cent at 100 degrees F. to 1·5 per cent at 300 degrees F., the efficiency being higher at the higher superheat.

(*c*) For a back-pressure ratio of 2, the difference in the reheat correction varies from 0·25 per cent to 0·5 per cent, the efficiency being higher at the lower back pressure.

The wetness and reheat correction combined are approximately as follows :

(*a*) For a ratio of initial pressures of 2, the difference in the combined correction varies roughly from 0·5 per cent at a pressure of 200 lb. per square inch (gauge) to zero at pressures of 500 to 1 000 lb. per square inch (gauge), the efficiency being lower at the higher pressure.

(*b*) For a superheat ratio of 2, the difference in the combined correction is roughly 3·5 per cent,

Table 10.

Improvement in Heat Consumption with Increased Pressure without Feed Heating.

Initial temperature = 688° F. ; final temperature = 76° F., corresponding to a vacuum of 29·1 inches.

Steam conditions	1	—	Gauge pressure, lb. per sq. in.
	2	—	Absolute pressure, lb. per sq. in.
	3	—	Saturation temperature, ° F.
	4	—	Superheat, deg. F.
Heat drop	5	—	Total heat from 32° F., B.Th.U. per lb.
	6	—	Total heat from 76° F., B.Th.U. per lb.
	7	$\frac{(6) - (6)\ 200\ \text{lb.}}{(6)\ 200\ \text{lb.}}$	Decrease in total heat referred to 200 lb. per sq. in. (gauge) per cent
	8	—	Heat drop to 29·1 in. vacuum, B.Th.U. per lb.
	9	—	Theoretical increase in heat drop over 200 lb. per sq. in. (gauge) per cent
Theoretical improvement	10	(8)/(6)	Theoretical thermal efficiency, per cent
	11	$\frac{(10) - (10)\ 200\ \text{lb.}}{(10)\ 200\ \text{lb.}}$	Percentage increase in thermal efficiency referred to 200 lb. per sq. in. (gauge) per cent
	12	—	10 ÷ Carnot cycle efficiency = 53·33 per cent
	13	—	Correction for wetness, per cent
Actual improvement	14	—	Total actual improvement in thermodynamic efficiency referred to turbine at 200 lb. per sq. in., using steam at 688° F., per cent
	15	—	Decrease in heat consumption per kW, per cent
Decrease in size of condenser	16	0·80 [100 — (13)]	Turbine efficiency (assuming 80 % at 200 lb. per sq. in. (gauge) per cent)
	17	(8) × (16)	Heat utilized by turbine per lb. of steam, B.Th.U. per lb.
	18	—	Heat delivered up to condenser per lb. of steam, B.Th.U. per lb.
	19	—	Relative size of condenser per lb. of steam, per cent
	20	—	Relative size of condenser for same output, per cent
Improvement, etc., for 10 %, increase in absolute pressure	21	—	Increase in absolute pressure per step in terms of 10 %, per cent
	22	—	Increase in total heat, per cent
	23	—	Increase in heat drop, per cent
	24	—	Increase in theoretical thermal efficiency, per cent
	25	—	Wetness correction, per cent
	26	—	Total actual increase in thermal efficiency, per cent

the efficiency being higher at the higher superheat.

(*c*) For a back-pressure ratio of 2, the difference in the combined correction varies roughly from 0·25 per cent to zero, the efficiency being lower at the lower back pressure.

The combined corrections for steam pressures and for back pressures are relatively small compared with the more important corrections resulting from higher leakage and disc friction losses at higher pressures and the higher leaving losses at lower back pressures.

(C) With a given turbine, the *Velocity Ratio* varies inversely with the square root of the total heat drop, and the correction depends on the actual value of u/c_0 for given conditions. For the calculations given in this part of the paper the stage efficiency has been assumed to be independent of the steam conditions and back pressures. This means that in order to obtain the improvements calculated, a larger number of stages will be required to deal with larger heat drops.

(D) Likewise, no allowances have been made in the calculations given in this part of the paper for decreased efficiencies resulting from increased leakage and disc friction losses at higher pressures, and increased leaving losses at lower back pressures. This means that in order to obtain the improvements calculated, suitable provisions must be made to reduce these losses to a common standard.

(*c*) *Improvements due to higher steam pressures.*—In the discussion on Mr. Shaw's paper (*Journal I.E.E.*, 1919, vol. 57, p. 97) the author gave an approximate rule for the calculation of the improvement in heat consumption possible with higher pressures. This was to the effect that for every 10 per cent increase in pressure the overall economy is improved by 1 per cent.

Table 10 contains the result of accurate calculations on the basis outlined under (*b*) for steam pressures up to 1 000 lb. per square inch, the total temperature and the vacuum being constant, at 688° F. and 29·1 inches (Hg) respectively. It is assumed that there is no feed heating in connection with the main turbine.

The total variation of the reheat factor for a constant total temperature of 688° F. and a constant back pressure of 0·9 inch (abs.), as plotted in Fig. 16, is only 0·3 per cent, and this small correction has been neglected; due allowance, however, has been made for the variation in the wetness correction which is shown in Fig. 16.

The variation in heat consumptions with increase of pressure is plotted in Fig. 17. A curve has also been plotted in Fig. 17 to show the variation in the condensing surface required for the same total capacity in kilowatts.

Fig. 17A shows the improvement in thermal efficiency for 10 per cent increase in absolute pressures. Its value is given in the table on page 584, col. 1.

Actually with a given turbine, i.e. a given cost, the improvements calculated above will not be fully realized. It is estimated that on account of the lower value of the ratio u/c_0 and the increased leakage and disc friction losses at the higher pressures, the actual improvement will be reduced by about 1 per cent for

TABLE 10—*continued.*

Improvement in Heat Consumption with Increased Pressure without Feed Heating.

Initial temperature = 688° F., final temperature = 76° F., corresponding to a vacuum of 29·1 inches.

200	250	300	350	400	450	500	550	600	650	700	750	800	850	900	950	1 000
214·7	264·7	314·7	364·7	414·7	464·7	514·7	564·7	614·7	664·7	714·7	764·7	814·7	864·7	914·7	964·7	1 014·7
387·8	406·3	422·2	436·4	449·2	460·9	471·7	481·7	491·2	500·1	508·6	516·8	524·7	532·2	539·4	546·2	552·7
300·2	281·7	265·8	251·6	238·8	227·1	216·3	206·3	196·8	187·9	179·4	171·2	163·3	155·8	148·6	141·8	135·3
1 371·3	1 368·5	1 365·9	1 363·2	1 360·5	1 357·8	1 355·2	1 352·5	1 349·8	1 347·1	1 344·4	1 341·7	1 339·1	1 336·4	1 333·7	1 331·0	1 328·3
1 327·7	1 324·9	1 322·3	1 319·6	1 316·9	1 314·2	1 311·6	1 308·9	1 306·2	1 303·5	1 300·8	1 298·1	1 295·5	1 292·8	1 290·1	1 287·4	1 284·7
0	0·211	0·407	0·610	0·813	1·017	1·213	1·413	1·619	1·823	2·036	2·230	2·425	2·629	2·832	3·036	3·240
454·9	465·6	474·1	481·1	487·0	492·0	496·4	500·2	503·6	506·5	509·1	511·4	513·5	515·3	517·0	518·5	519·8
0	2·36	4·22	5·76	7·06	8·16	9·12	9·96	10·71	11·34	11·92	12·42	12·88	13·28	13·64	13·94	14·26
34·26	35·16	35·86	36·45	36·98	37·45	37·87	38·24	38·58	38·87	39 15	39·38	39·63	39·86	40·07	40·27	40·46
0	2·63	4·67	6·39	7·94	9·31	10·54	11·61	12·61	13·46	14·27	14·94	15·67	16·35	16·96	17·54	18·1
64·26	65·93	67·24	68·36	69·36	70·25	71·01	71·73	72·36	72·90	73·41	73·85	74·31	74·79	75·01	75·54	75·87
0	0·40	0·78	1·15	1·48	1·8	2·10	2·40	2·70	2·98	3·25	3·5	3·75	4·0	4·25	4·45	4·70
0	2·22	3·9	5·16	6·34	7·35	8·22	8·93	9·57	10·08	10·55	10·92	11·34	11·70	12·05	12·31	12·56
0	2·17	3·75	4·90	5·97	6·84	7·60	8·22	8·73	9·15	9·55	9·86	10·15	10·46	10·75	10·95	11·16
80	79·68	79·38	79·08	78·82	78·56	78·32	78·08	77·84	77·62	77·40	77·2	77·0	76·8	76·64	76·44	76·24
363·9	371·0	376·3	380·4	383·7	386·5	388·9	390·5	392·0	393·1	394·0	394·7	395·4	395·8	396·2	396·3	396·4
963·8	953·9	946·0	939·2	933·2	927·7	922·7	918·4	914·2	910·4	906·8	903·4	900·1	897·0	893·9	891·1	888·3
100	98·94	98·14	97·46	96·82	96·24	95·70	95·29	94·84	94·44	94·09	93·75	93·37	93·07	92·75	92·45	92·15
100	96·74	94·55	92·69	91·07	89·64	88·43	87·44	86·56	85·83	85·09	84·5	83·89	83·32	82·78	82·33	81·88
2·197	1·817	1·545	1·348	1·195	1·073	0·971	0·891	0·821	0·758	0·710	0·664	0·625	0·587	0·560	0·529	
0·092	0·112	0·132	0·151	0·171	0·191	0·211	0·231	0·251	0·271	0·292	0·313	0·333	0·353	0·374	0·394	
1·076	1·006	0·952	0·906	0·863	0·823	0·785	0·749	0·712	0·677	0·643	0·609	0·575	0·540	0·505	0·470	
1·168	1·118	1·084	1·057	1·034	1·014	0·996	0·980	0·963	0·948	0·935	0·922	0·908	0·893	0·879	0·864	
−0·018	−0·217	−0·241	−0·247	−0·271	−0·294	−0·315	−0·344	−0·350	−0·366	−0·378	−0·389	−0·415	−0·443	−0·465	−0·474	
0·986	0·901	0·843	0·810	0·763	0·720	0·681	0·636	0·613	0·582	0·560	0·533	0·493	0·450	0·414	0·390	

an increase in initial pressure of about 100 per cent (i.e. for a pressure ratio of 2), the total temperature remaining the same, or by about 0·14 per cent for an increase in pressure of 10 per cent.

For a constant superheat this additional correction should be larger, because the decrease in the value of the ratio is greater for the same increase in pressures of 2; and also because the leakage losses are slightly increased

For Pressures Between	Improvement in Thermal Efficiency in per cent for 10 per cent Increase in Pressure
200 and 250 lb. per sq. in. (gauge)	0·986
400 and 450 lb. per sq. in. (gauge)	0·763
600 and 650 lb. per sq. in. (gauge)	0·613
800 and 850 lb. per sq. in. (gauge)	0·493

as a result of the slightly larger clearances required for the higher temperatures. It is estimated that, in this case, the additional allowance would be 1·5 per cent for a pressure ratio of 2.

This allowance added to the combined thermodynamic efficiency correction for wetness and reheat factor gives, for a ratio of initial pressure of 2, a difference in the thermodynamic efficiency varying from 2 per

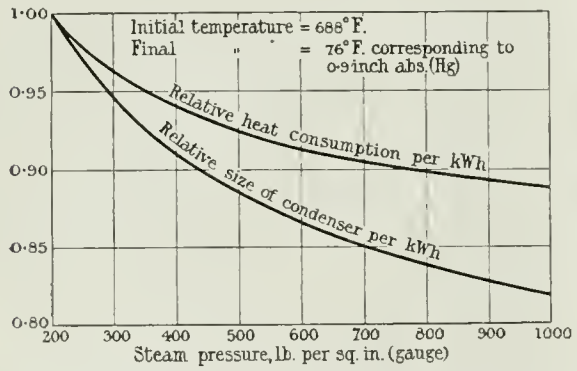


Fig. 17.—Relative heat consumption and size of condenser per kWh without feed heating, compared with 200 lb. per sq. in. (gauge), as a function of the gauge pressure.

cent at a pressure of 200 lb. per square inch (gauge) to 1·5 per cent at very high pressures. For all practical purposes an efficiency correction of 2 per cent for a ratio of initial pressures of 2 is sufficiently accurate as a guide for comparison of turbine efficiencies.

With modern methods of feed heating in which steam is taken from a low-pressure stage of the main turbine, the improvement possible with higher pressures will be larger than stated above. It is estimated that for pressures of 200 to 250 lb. per square inch (gauge) the improvement for 10 per cent increase in pressure will be approximately 10 per cent larger than stated, and for pressures of 800 lb. per square inch about 20 per cent larger; the resulting improvement after making this correction and the allowance for increased leakage and disc friction losses is accordingly:

For Pressures Between	Approximate Actual Improvement for 10 per cent Increase in Pressure	
	Without Feed Heating	With Modern Method of Feed Heating
	Per cent	Per cent
200 and 250 lb. per sq. in. (gauge)	0·85	0·95
400 and 450 lb. per sq. in. (gauge)	0·65	0·75
600 and 650 lb. per sq. in. (gauge)	0·50	0·60
800 and 850 lb. per sq. in. (gauge)	0·40	0·50

(*d*) *Improvement due to higher total steam temperatures.*—In the discussion on Mr. Shaw's paper referred to above, the author came to the conclusion that the heat consumption is decreased 3 per cent for an increase

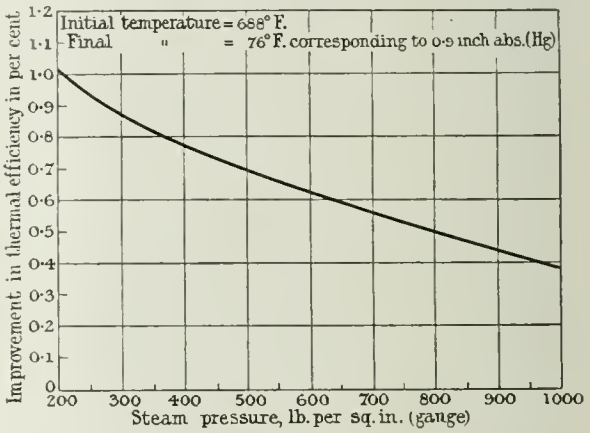


Fig. 17A.—Total improvement in thermal efficiency for 10 per cent increase in absolute pressure.

in temperature of 100 degrees F. for total temperatures between 600° and 750° F. and for steam pressures between 200 and 400 lb. per square inch (gauge). Table 11 contains the result of accurate calculations [due allowance being made for wetness and reheat factor on the basis explained in section (*b*)] for total temperatures of 688° F. and 788° F. The improvement in heat consumption obtained by raising the total temperature 100 degrees F. is as follows:

For Steam Pressures of	Improvement in Heat Consumption for 100 deg. F.
200 lb. per sq. in. (gauge)	3·47 per cent
400 lb. per sq. in. (gauge)	3·66 per cent
600 lb. per sq. in. (gauge)	3·88 per cent
800 lb. per sq. in. (gauge)	4·02 per cent

Actually, with a given turbine the improvement calculated above will not be fully realized. It is estimated that on account of the lower value of the ratio u/c_0 at the higher temperature, and the slightly increased leakage losses resulting from the larger clearances required with higher temperatures, the improvement will be reduced by about 1·5 per cent for a superheat ratio of 2, for superheats from 100 degrees F. to 400 degrees F.

This correction applied to the improvements given

These improvements are identical with that given in the discussion above referred to.

If the decrease in efficiency resulting from the lower value of the ratio u/c_0 at the higher temperature, and the slightly increased leakage losses estimated above, is deducted from the combined correction for wetness and reheat factor given in section (*b*), we arrive at a superheat correction for the thermodynamic efficiency of the whole turbine of 2 per cent for a superheat ratio of 2.

TABLE 11.

Improvement in Heat Consumption for 100 *degrees F. Extra Superheat for a Back Pressure of* 0·9 *inch abs.* (*Hg*).

Steam conditions	1	Temperature, ° F.	688	788	688	788	688	788	688	788
	2	Gauge pressure, lb. per sq. in. ..	200	200	400	400	600	600	800	800
	3	Absolute pressure, lb. per sq. in. ..	214·7	214·7	414·7	414·7	614·7	614·7	814·7	814·7
	4	Saturation temperature, ° F. ..	387·8	387·8	449·2	449·2	491·2	491·2	524·7	524·7
	5	Superheat, deg. F.	300·2	400·2	238·8	338·8	196·8	296·8	163·3	263·3
Heat drop	6	Total heat from 32° F., B.Th.U. per lb.	1 371·3	1 422·0	1 360·5	1 413·9	1 349·8	1 406·0	1 339·1	1 398·1
	7	Total heat from 76° F., B.Th.U. per lb.	1 327·7	1 378·4	1 316·9	1 370·3	1 306·2	1 362·4	1 295·5	1 354·5
	8	Increase in total heat for 100 deg. F. extra superheat	50·7		53·4		56·2		59·0	
	9	Heat drop to 76° F. (= 29·1 in. vacuum), B.Th.U. per lb. ..	454·9	482·8	487·0	516·4	503·6	534·6	513·5	546·0
	10	Increase in heat drop for 100 deg. F. extra superheat, B.Th.U. per lb.	27·9		29·4		31·0		32·5	
	11	Per cent increase in heat drop for 100 deg. F. extra superheat ..	6·14		6·04		6·16		6·33	
Theoretical improvement	12	Theoretical thermal efficiency = (9) ÷ (7) per cent	34·27	35·02	36·98	37·68	38·55	39·23	39·65	40·32
	13	Per cent increase in theoretical thermal efficiency	2·18		1·89		1·76		1·69	
	14	Theoretical efficiency ÷ (Carnot efficiency = 53·33 %)	64·3		69·2		72·3		74·2	
	15	Theoretical efficiency ÷ (Carnot efficiency = 57·09 %)	61·4		66·0		68·8		70·7	
Actual improvements Corrections	16	Correction for wetness, per cent ..	0·77		1·40		1·8		2·05	
	17	Correction for reheat, per cent ..	0·61		0·47		0·44		0·41	
	18	Total correction, per cent	1·38		1·87		2·24		2·46	
	19	Total actual improvement in thermal efficiency for 100 deg. F. extra superheat	3·59		3·80		4·04		4·19	
	20	Decrease in heat consumption per kWh	3·47		3·66		3·88		4·02	

above results in the following reductions and actual improvements with a given turbine:

For Pressures of	Reduction	Actual Improvement
200 lb. per sq. in. (gauge)	0·6 per cent	2·87 per cent
400 lb. per sq. in. (gauge)	0·75 per cent	2·91 per cent
600 lb. per sq. in. (gauge)	0·89 per cent	2·99 per cent
800 lb. per sq. in. (gauge)	1·02 per cent	3·00 per cent

(*e*) *Improvement due to higher vacua.*—It has been shown in section (*b*) that the combined correction for wetness and reheat factor for varying back pressures varies from 0·25 to zero per cent for a back-pressure ratio of 2. As this correction is small compared with the leaving loss, it follows that the improvement in steam consumption with lower back pressures can be calculated directly from the increase in heat drop with an accuracy which is sufficient for all practical purposes. To obtain this improvement it will be necessary, however, to increase the number of stages to deal with the larger heat drop, and to increase the exhaust capacity of the turbine to deal with the larger volume at lower back pressures. The effect of these factors is fully dealt with in Part III of this paper.

In calculating the improvement in heat consumption, it used to be the practice to allow for the increased heat per pound of steam resulting from the lower temperature of the feed water obtained at lower back pressures. With modern methods of heating the feed water now usually adopted, in which steam at pressures considerably below atmospheric is bled from the main turbine, this correction applies only to a very limited extent and for all practical purposes can be neglected.

The improvement in heat consumption is consequently identical with the improvement in the steam reheating the steam between stages. Owing to the great additional expenses involved in reheating the steam more than once, and the relatively small additional gain, multiple reheating is not a practicable proposition, and the following remarks deal with reheating the steam once only. Fig. 18 (curve B) shows the improvement possible with single reheating to the initial temperature at different stages, the initial pressure being 500 lb. per square inch, the total temperature 670° F. and the back pressure 0·75 inch. In calculating the improvement, allowances are made

TABLE 12.

Improvement in Steam Consumption with Back Pressures Lower than 2·0 inches abs. (Hg) Referred to the Consumption at 2·0 inches abs. (Hg) for Constant Stage Efficiency and Leaving Losses (Combined Correction for Wetness and Reheat Factor is Negligible).

Total temperature			688° F.					788° F.			
Gauge pressure, lb. per sq. in. (g) ..			200	300	400	500	600	200	300	400	500	600
Saturation temperature, ° F.			387·8	422·2	449·2	471·7	491·2	387·8	422·2	449·2	471·7	491·2
Superheat, deg. F.			300·2	265·8	238·8	216·3	196·8	400·2	365·8	338·8	316·3	296·8
		In. Abs. (Hg)	Exh. Temp., ° F.									
Section I	Heat drop at ..	2·0	101·11	414·2	434·4	448·5	458·8	466·3	441·2	462·3	477·1	487·6	496·3
	Additional heat drops for an absolute pressure of	1·5	91·67	15·0	14·4	14·2	14·0	13·8	15·5	15·0	14·7	14·5	14·2
		1·0	78·96	35·5	34·3	33·7	33·1	32·6	36·4	35·5	34·8	34·2	33·6
		0·9	75·77	40·7	39·7	38·5	37·6	37·3	41·6	40·1	39·3	39·0	38·3
		0·75	70·33	50·3	48·5	47·6	46·8	46·1	51·5	50·1	49·1	48·3	47·5
		0·6	63·86	60·4	58·4	57·2	56·2	55·4	61·8	60·2	59.0	58·0	57·1
		0·5	58·70	68·9	66·7	65·4	64·2	63·3	70·6	68·7	67·3	66·2	65·2
Section II	Improvement in per cent of consumption at 2 inches abs. (Hg) for an absolute pressure of	1·5	91·67	3·62	3·32	3·16	3·05	2·96	3·51	3·24	3·09	2·97	2·86
		1·0	78·96	8·56	7·90	7·50	7·20	7·00	8·25	7·65	7·28	7·00	6·77
		0·9	75·77	9·82	9·15	8·57	8·20	8·00	9·43	8·66	8·25	8·00	7·72
		0·75	70·33	12·15	11·15	10·60	10·20	9·88	11·68	10·80	10·30	9·90	9·57
		0·6	63·86	14·60	13·45	12·70	12·20	11·86	14·00	13·00	12·40	11·90	11·50
		0·5	58·70	16·65	15·35	14·55	13·95	13·60	16·00	14·80	14·10	13·55	13·10
Section III	Decrease in exhaust temperature in degrees F. required to improve consumption by 1 per cent	2·0	101·11										
		1·5	91·67	2·6	2·8	3·0	3·1	3·2	2·7	2·9	3·0	3·2	3·3
		1·0	78·96	2·5	2·7	2·8	3·0	3·1	2·6	2·8	2·9	3·0	3·1
		0·9	75·77	2·3	2·5	2·7	2·9	3·0	2·5	2·8	2·9	3·0	3·1
		0·75	70·33	2·3	2·4	2·6	2·7	2·8	2·5	2·7	2·8	2·9	3·0
		0·6	63·86	2·3	2·4	2·5	2·6	2·7	2·4	2·6	2·7	2·8	2·9
		0·5	58·70	2·2	2·3	2·4	2·5	2·6	2·3	2·5	2·6	2·7	2·8

consumption which, as explained above, is obtained directly from the increase in the heat drop.

Section I of Table 12 gives the heat drops for various steam conditions for a vacuum of 28 inches, and the additional heat drops for vacua higher than 28 inches (Hg).

Section II gives the consequent improvement in steam consumption in per cent of the consumption at 28 inches (Hg).

Section III gives the decrease in exhaust steam temperature necessary to obtain 1 per cent improvement in steam consumption or heat consumption at various steam conditions and back pressures.

(*f*) *Improvement due to reheating.*—The heat consumption of a turbine installation can be improved by for variation of the reheat factor as well as for the improvement of the stage efficiencies resulting from the use of drier steam on the basis stated under (*b*) of this Part.

The improvement in heat consumptions becomes a maximum when the steam is reheated at about one-third of the adiabatic heat drop, or at one-fifth of the initial pressure, the improvement being 8½ per cent. Actually, it is more economical to reheat at a higher pressure, say one-quarter of the initial pressure, as the cost of steam piping is reduced, owing to the smaller piping required in such a case.

If the steam is not reheated to the initial temperature, the improvement is reduced practically in proportion to the differences in temperatures, and the maximum

improvement is obtained at slightly lower pressures. The correction due to the reduction in the wetness of steam in the low-pressure stages affects the result considerably, as is evident from a comparison of curves A and B, the former showing the improvement without allowing for the decreased wetness, whereas the latter includes this allowance. It must be left to the results of further tests to prove whether the improvement shown by curve B can actually be obtained.

Another advantage obtained from reheating is the decreased liability of blade corrosion of the low-pressure blades.

(*g*) *Improvement due to feed-water heating.*—The thermal efficiency of a turbine can be improved considerably by heating the feed water by steam taken from one or more stages of the main turbine. The author has fully dealt with the improvements possible in this direction in a recent article,* in which particular attention was drawn to the importance of feed heating at high pressures.

time comparing the losses incident to the particular types of turbines.

Curves for blading efficiencies which may have been obtained from experiments after making a considerable number of allowances—more or less problematical—for the high losses incidental to an experimental turbine, are very bad criteria for the merits of a type of turbine, and the practical engineer will always turn back to the one question: What overall efficiencies have actually been obtained? Have the arguments put forward in favour of some particular type of turbine been substantiated in practice? It is preferred, therefore, to leave the answer to test-results accurately determined.

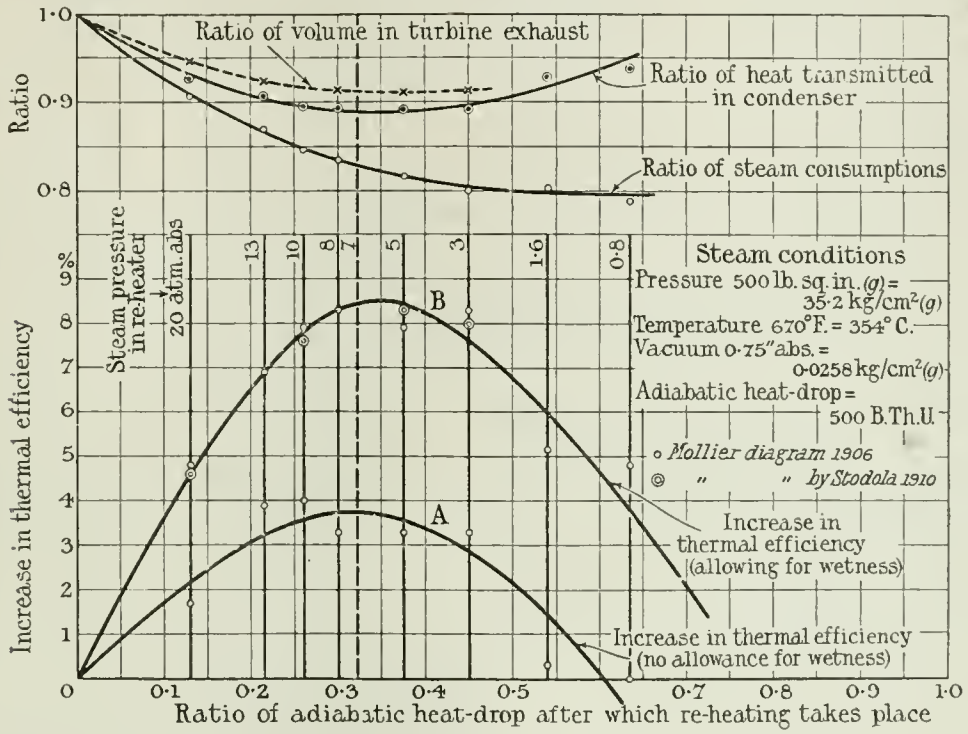


FIG. 18.—Increase in thermal efficiency of steam turbines due to reheating.

Single reheating. No feed-water heaters used in exhaust. Turbine stage efficiency 75 per cent for all stages with superheated steam. Allowance for decrease in stage efficiency due to wetness. No allowance made for drop in pressure in reheater.

(3) The Thermodynamic Efficiency.

Apart from the steam conditions, the thermodynamic efficiency of a turbine depends on the blading efficiency and the losses outside the blading. The losses which occur in the various types of turbines are so vastly different in character and value that it is useless to compare the blading efficiencies without at the same time comparing the losses incident to the particular types of turbines.

It must be the sole object of the designer to choose such types of blading for the different parts of a turbine that the combination will produce the best machine considered from the point of view of reliability, efficiency and costs, and no prejudices of any kind should be allowed to enter the designer's mind in trying to arrive at the best solution.

In 1912 there was a considerable tendency towards the adoption of the velocity wheel for the high-pressure end of medium-sized turbines. Owing to the increase in output at a given speed, however, the advantages of the velocity wheel are outweighed by the increased efficiency of single Rateau stages, except in cases where the diameter used for the first stage is relatively small. There is a tendency for the high-pressure end of reaction turbines to be replaced by impulse stages not

* "Feed" Heating for Land Power Stations," *Engineer*, 1920, vol. 130, pp. 101, 127, and 150.

necessarily of the velocity-wheel type, but also of the ordinary Rateau type.

For the middle portion and the low-pressure end both impulse and reaction blading continue to be used, but, as is evident from the summary of the commercial developments in this country, reaction blading has largely given way to impulse blading.

The merits of one type of blading over the other do not depend so much on the type of blading used as on the detail construction employed in connection with it. It is obvious that such features as lashing soldered to the blades, commonly used in connection with reaction blades, are less satisfactory from the point of view of steam flow through the blades and of reliability than the shrouding riveted to the tips of blades generally used with impulse blading. There is, of course, no reason why this method should not be used with reaction blades, except perhaps the higher costs.

It is necessary that the steam should fill completely the blade passages of the last row of moving blades to ensure that the leaving losses are a minimum for a given leaving area. This condition is met by using a small amount of reaction, i.e. the pressure between the last row of guide blades and the last row of moving blades should be slightly higher than the pressure in the exhaust, a condition which automatically obtains when a turbine is designed without reaction in the last stage but is run at a higher vacuum than that for which it is designed.

The efficiency of a turbine depends, to a great extent, on the average ratio of peripheral speed to the steam speed which would result from the pressure drop over a complete pair of blade rows containing one row of stationary and one row of moving blades.

In order to compare the efficiencies of different turbines with varying steam conditions it is necessary, therefore, in the first place to know the average speed ratios of blade velocity to steam velocity, i.e. u/c_0, for each turbine. Assuming that this speed ratio be the same for each row of blades we get:

$$\frac{u}{c_0} = k\,; \quad u^2 = k^2 c_0^2\,; \quad \Sigma u^2 = k^2 \Sigma c_0^2 \quad . \quad . \quad (1)$$

or, after dividing by $2gJ$,

$$\Sigma \frac{u^2}{2gJ} = k^2 \Sigma \frac{c_0^2}{2gJ} = k^2 \quad \Sigma \Delta_i = k^2 i_2 (1 + a) \quad . \quad (2)$$

where J = mechanical equivalent of heat,
= 424 (metrical units) or 778 (English units),
i_2 = total adiabatic heat drop available,
Δ_i = adiabatic heat drop per stage,
$1 + a$ = reheat factor,
$\Sigma = \Sigma \frac{u^2}{2gJ}$ = heat drop of peripheral speeds.

The average speed-ratio $k = u/c_0$ is therefore

$$k = \sqrt{\left[\frac{\Sigma}{(1 + a) i_2}\right]} \quad . \quad . \quad . \quad . \quad (3)$$

The heat drop of peripheral speeds Σ is a constant for a given turbine, which can be utilized for rough comparisons of blade efficiencies of different turbines at different heat drops.

It is, for metric units (metre, second, ° C.)

$$\Sigma_m = \Sigma \left(\frac{u}{91{\cdot}5}\right)^2 \quad . \quad . \quad . \quad . \quad (4)$$

for English units (ft., second, ° F.)

$$\Sigma_e = \Sigma \left(\frac{u}{224}\right)^2 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The use of this heat drop of peripheral speeds is preferred to the Parsons constant C, which is defined by:

$$C_e = \Sigma \left(\frac{d}{10}\right)^2 \left(\frac{\text{r.p.m.}}{100}\right)^2 \quad . \quad . \quad . \quad . \quad (6)$$

where d = mean blade diameter in inches,
r.p.m. = speed of turbine.

The relation between the two constants is as follows:

$$C_e = 4\,744\,\Sigma_m = 2\,635{\cdot}5\,\Sigma_e \quad . \quad . \quad . \quad (7)$$

It will be seen that this constant is approximately proportional to the cubical volume of the turbine and, consequently, proportional to its cost.

Table 13 gives a list of losses outside the blading of these different frames of impulse turbines. They can be divided into two different categories.

(1) Constant kW losses, i.e. such losses which when expressed in kilowatts are independent of the rating for which the turbine frame may be designed.

(2) c^2 losses, consisting of steam chest, leaving and exhaust losses, which depend largely on the rating and the vacuum for which the turbine is designed. When expressed in per cent of the turbine output, these losses vary as the square of the respective velocities and, consequently, for given inlet and exhaust pressures, approximately as the square of the rating.

These losses largely control the economical ratings for which a given frame may be designed and, consequently, the progress towards larger outputs at a given speed. Owing to the magnitude of this problem it is proposed to review the practical possibilities in this respect in detail in Part III.

PART III.

THE ECONOMIC RATING OF A GIVEN TURBINE FRAME.

(1) General.

The steam consumption of a given turbine designed for a given economical rating under given steam and vacuum conditions varies with the load, as indicated by curve x in Fig. 19, the consumption at the economical rating being shown at X.

If the same turbine frame be designed for a higher economical rating under the same steam and vacuum

conditions, the steam-consumption curve would be as shown by y, the consumption at the economical rating being shown at Y. The consumption at Y is higher than at X, owing to higher leaving losses.

consumptions at such designed ratings will lie along a curve D, which it is proposed to define as the "economical consumption characteristic" for the given turbine frame for given steam and vacuum conditions.

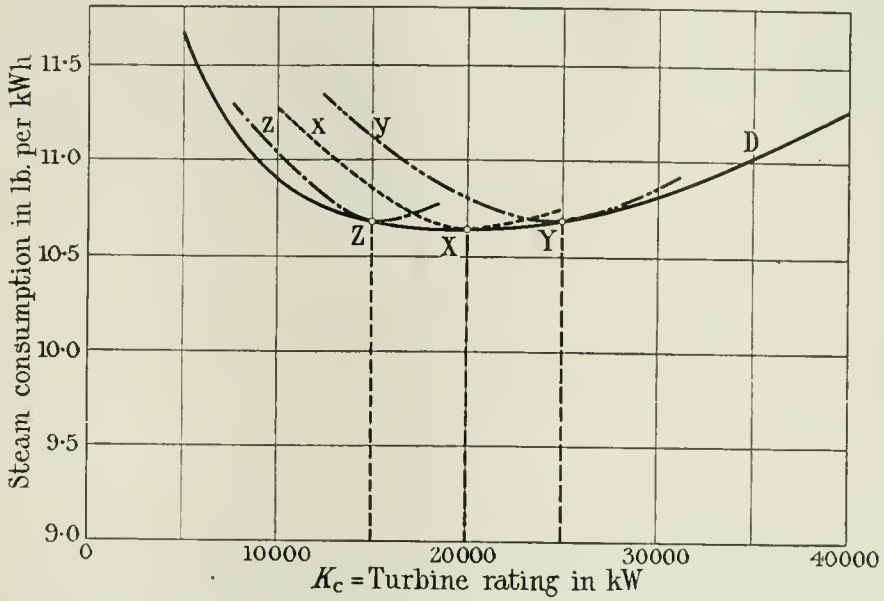


FIG. 19.—Economical consumption characteristic.
Steam conditions:—Pressure 200 lb. per sq. in. (gauge), 200 degrees F. superheat, 1 inch abs. (Hg).

Again, if the same turbine frame be designed for a lower economical rating under the same steam and vacuum conditions, the steam-consumption curve would be as shown by z, the consumption at the economical

The problem investigated in this section of the paper is the determination of the particular point along curve D at which the economical rating can be fixed for given load factor, fuel costs, capital charges, etc.

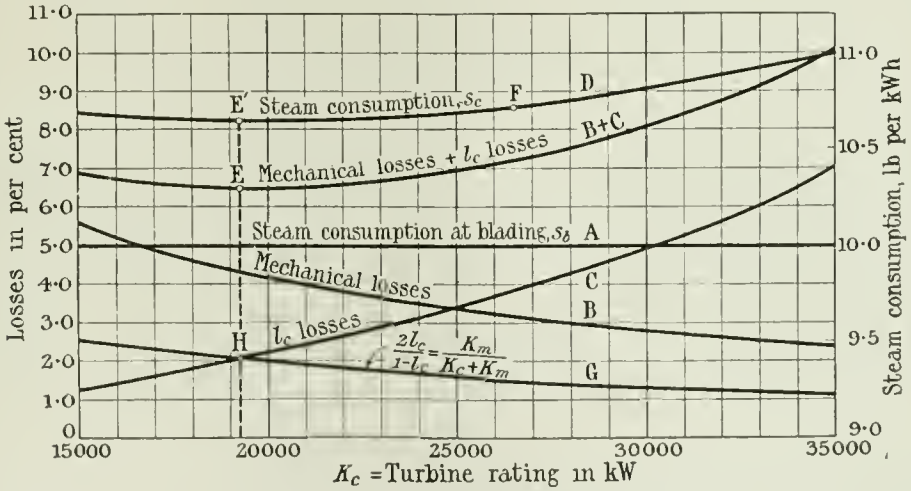


FIG. 20.—Variation of losses and steam consumptions with the ratings of a turbine.
Steam conditions:—Pressure 200 lb. per sq. in. (gauge), 200 degrees F. superheat, and 29 in. vacuum.

rating in this case being shown at Z. The steam consumption at Z may be higher than at X, owing to the mechanical efficiency being lower at the reduced rating.

It is possible to design a given turbine frame for any rating within a given range, and the economical

Any change in the steam or vacuum conditions will affect this curve D, so that there is a definite "economical consumption characteristic" for every set of conditions for which a given turbine frame can be designed.

Blading efficiency is practically independent of rating

within a given range of output except in the case of the last stage, the efficiency of which will decrease for given conditions with increased output, due to increased c^2 losses.

All mechanical losses (Table 13, items 1 to 6) when expressed in kilowatts are practically independent of the rating for which the turbine frame may be designed. The mechanical losses are consequently inversely proportional to the rating.

Steam chest, leaving and exhaust losses, i.e. c^2 losses (Table 13, items 9 to 13) expressed in per cent of the total heat drop available, vary as the square of the quantity of steam flowing through the turbine. The effect of these losses is to reduce the heat drop available for producing mechanical work in the blading.

The manner in which the losses classified in Table 13 vary with the rating of a given size of turbine for given steam and vacuum conditions is shown graphically in Fig. 20, the curves shown in this figure being as follows:

Curve A shows the variation of the steam consumption s_b in lb. per kWh at the blading if c^2 losses do not exist. This consumption is independent of the rating, and curve A is, therefore, a horizontal line.

Curve B shows the variation of the mechanical losses expressed as a percentage of the output measured at the coupling (Table 13, items 1 to 6). In the case which is considered, the mechanical losses amount to $K_m = 835$ kW (Table 13, item 7).

Curve C shows the variation of the c^2 losses expressed in percentage of the total heat drop available for a back pressure of 1 inch abs. These losses vary approximately as the square of the rating.

Curve (B + C) shows the variation of the total losses expressed as a percentage of the output at the turbine coupling.

Curve D is the economical consumption characteristic, and shows the variation of the economical steam consumption for the various designed ratings referred to the outputs at the turbine coupling.

TABLE 13.

Losses in Steam Turbines.

1	2	3	4	5
		Output of turbine at coupling, kW	28 900	
		R.p.m.	1 500	
Item No.	Type of Losses		Loss in kW Measured at Coupling	Loss in per cent
1	Constant kW losses in per cent of turbine output at coupling	Disc friction	315	1·09
2		Diaphragm leakage losses	174	0·6
3		H.P. gland losses	185	0·64
4		Water and steam seal losses	36	0·12
5		Bearing losses	109	0·38
6		Governor and oil pump drive	16	0·05
7	—	Total constant kW losses	835	—
8	—	Total constant losses in per cent of turbine output at coupling	—	2·89
9	c^2 losses in per cent of adiabatic heat drop	Steam chest losses	—	0·53
10		Leaving losses	—	2·85
11		Exhaust losses	—	0·75
12	—	Total c^2 losses in per cent of adiabatic heat drop ..	—	4·13
13	—	Total c^2 losses in per cent of turbine output at coupling	—	4·45
14	—	Total losses in per cent of turbine output at coupling (8) + (13)	—	7·34

The output of the turbine measured at the coupling is given by the following formula:

$$K_c = K_b (1 - l_c) - K_m \quad . \quad . \quad . \quad . \quad (1)$$

where K_c = the rating in kW at the turbine coupling,
K_b = the work possible in kW at the blading if the c^2 losses do not exist,
$K_b(1 - l_c)$ = the work done by the blading in kW,
l_c = the c^2 loss ratio

$$= \frac{c^2 \text{ losses in B.Th.U.'s}}{\text{total heat drop available in B.Th.U.'s}}$$

K_m = mechanical losses in kW.

From this it follows that the economical consumption referred to the output at the coupling is

$$s_c = s_b \frac{1}{1 - l_c - K_m/K_b} \quad . \quad . \quad . \quad . \quad (2)$$

which has been used for the calculation of curve D.

It can be shown that the percentage increase in steam consumption for a given percentage increase in kW rating at any given point F can be calculated from the following formula:

$$\frac{\Delta s_c}{s_c} = \frac{\Delta K_c}{K_c}\left(\frac{2l_c}{1-l_c} - \frac{K_m}{K_c + K_m}\right) \quad . \quad . \quad (3)$$

(2) The Minimum Steam-consumption Rating.*

The rating at which the economical steam consumption becomes a minimum can be obtained from formula (3), since this rating is that at which the increase in steam consumption for an increase in rating becomes zero, i.e. when

$$\frac{2l_c}{1-l_c} = \frac{K_m}{K_c + K_m} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It will be seen from this formula that the minimum steam-consumption rating is a function only of the c^2 and the constant kW losses.

The c^2 losses for various ratings which satisfy formula (4) are shown by curve G. The intersection of curves G and C at H gives the minimum consumption rating, the actual consumption in lb. per kWh being shown at E′ on curve D.

Referring to Fig. 20, it will be seen that for a back pressure of 1 inch abs. (Hg) the rating K_c at which the steam consumption becomes a minimum is 19 250 kW, the corresponding c^2 losses being $l_c = 2{\cdot}04$ per cent. These c^2 losses are made up of the steam-chest losses (inlet losses) $l_i = 0{\cdot}40$ per cent or $0{\cdot}196\ l_o$ and the leaving and exhaust losses (outlet losses) which are $l_0 = 1{\cdot}64$ per cent or $0{\cdot}804\ l_c$.

For a back pressure of 2 inches abs. it has been calculated that the most economical rating becomes $K_c = 24\,000$ kW, or 1·3 times the rating for a back pressure of 1 inch abs. The c^2 losses in this case are $l_c = 1{\cdot}59$ per cent, made up of steam-chest losses $l_i = 0{\cdot}78$ per cent or $0{\cdot}49\ l_c$, and outlet losses $l_0 = 0{\cdot}81$ per cent or $0{\cdot}51\ l_c$.

It will be seen from the above that the steam-chest losses are relatively larger for a back pressure of 2 inches than for a back pressure of 1 inch abs. This indicates that the rating of the frame is handicapped by the fact that the steam chest is on the small side. The steam-chest losses, which include the losses in the stop, emergency and governor valves, can easily be kept within the desired limits by providing a steam chest of appropriate size, the extra cost of providing such a chest being small compared with the total cost of the turbine.

In order to obtain satisfactory governing and a slight margin in output, a pressure drop of 10 per cent is usually allowed. This drop corresponds to a loss in efficiency of approximately 1 per cent. If the usual governing requirements are not essential, as for instance when the turbine is required to run in parallel with other machines, this pressure drop can be considerably reduced by providing larger valves. In any case, the steam-chest losses do not affect the output which it is possible to obtain at a given speed and, for the purpose of determining the economic rating of a turbine, the steam-chest losses can be considered to be a constant percentage loss which results in a fixed reduction of heat drop available for mechanical work.

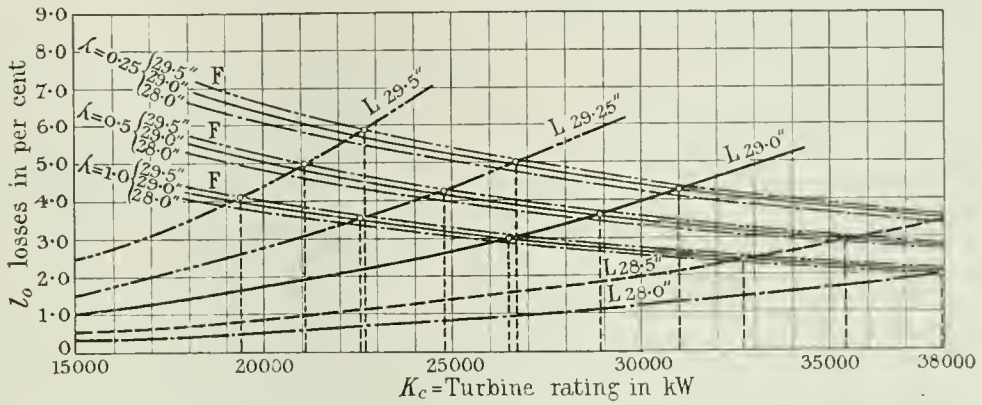


Fig. 21.—Economic ratings of a turbine frame for various vacua and load factors.

Steam conditions:—Pressure 200 lb. per sq. in. (gauge), 200 degrees F. superheat.

For the determination of the most economical rating of a given turbine frame having constant steam-chest losses, formulæ (3) and (4) can be applied if the outlet losses l_0 are substituted for the c^2 losses l_c. We thus get for the percentage increase in steam consumption

$$\frac{\Delta s_c}{s_c} = \frac{\Delta K_c}{K_c}\left(\frac{2l_0}{1-l_0} - \frac{K_m}{K_c + K_m}\right) \quad . \quad . \quad (5)$$

and the steam consumption becomes a minimum for

$$\frac{2l_0}{1-l_0} = \frac{K_m}{K_c + K_m} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The most economical rating of the turbine under consideration, under the stated steam conditions and for a back pressure of 1 inch abs., now becomes $K_c = 20\,900$ kW, the corresponding outlet losses being $l_0 = 1{\cdot}88$ per cent.

For a back pressure of 2 inches abs. the most economical rating is $K_c = 31\,000$ kW, or 1·48 times the rating for a back pressure of 1 inch abs. The corresponding outlet losses are $l_0 = 1{\cdot}29$ per cent, or 68·5 per cent of those obtained for a back pressure of 1 inch abs.

* An appendix to this Section of the paper will be found on page 602.

The variation of the most economical rating with the back pressure is shown in Fig. 22 by points marked 2, the rating for 1 inch abs. being unity. The rating varies

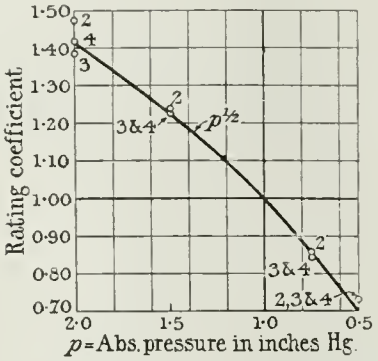


FIG. 22.—Variation of economic turbine rating for varying vacuum.

approximately as the square root of the back pressure.

The variation of the corresponding outlet losses with the back pressure is shown in Fig. 24 by points marked

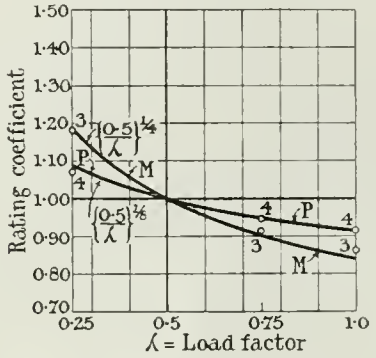


FIG. 23.—Variation of economic turbine rating for varying load factor.

2, the losses for 1 inch abs. being unity. The losses vary approximately inversely as the square root of the back pressure.

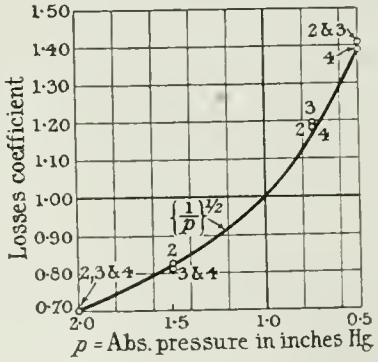


FIG. 24.—Variation of l_0 losses for varying vacuum.

(3) THE ECONOMIC RATING.*

In actual practice it does not pay to design a given turbine frame for the rating which gives the lowest possible steam consumption, because the turbine will be too expensive for such a rating. In order to determine the commercial economic rating of a given turbine frame (or what is termed simply the " economic " rating in this paper), it is necessary to consider the relative influence of the fuel and capital cost charges per kWh on the generating costs. The capital cost of the turbine itself varies inversely as the rating, whereas the fuel charges increase from the point of maximum efficiency with the rating.

It can be shown that the generating costs (u) become a minimum for

$$\frac{\Delta s_c}{s_c} = \frac{\Delta K_c}{K_c}\left(\frac{c_T}{f}\right) \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where c_T = capital charges due to turbine in pence per kWh,

f = fuel costs in pence per kWh.

From (5) and (7) it follows that:

$$\frac{2l_0}{1-l_0} = \frac{K_m}{K_c + K_m} + \frac{c_T}{f} \quad . \quad . \quad . \quad . \quad (8)$$

from which the outlet losses l_0 for the economic rating can be calculated.

This formula (8) can also be written as follows:

$$\frac{2l_0}{1-l_0} = \frac{K_m}{K_c + K_m} + m\,\frac{C_T}{K_c \,.\, s_c} \quad . \quad . \quad . \quad (9)$$

where C_T = initial cost of turbine in £,

$$m = 5{\cdot}11\,\frac{e \,.\, \gamma}{\phi \,.\, \lambda} \quad \text{where} \qquad m = 0{\cdot}409$$

e = evaporation of boiler in lb. per lb. of coal, $e = 8$

ϕ = cost of coal in shillings per ton $\phi = 40$

γ = interest, depreciation and maintenance in per cent of initial cost of turbine, $\gamma = 0{\cdot}20$

λ = load factor referred to the rating of turbine. $\lambda = 0{\cdot}5$

In formula (9) K_m and m are constants for a given frame and given conditions and are independent of the rating. l_0 varies approximately as the square of K_c, whilst the steam consumption varies slightly in accordance with the sum of the mechanical and the outlet losses. The cost of the turbine C_T increases slightly with the rating, mainly on account of the larger steam chest required for the larger ratings. The solution can best be obtained graphically, as shown in Fig. 21, where curves L represent l_0 for varying rating and varying vacua under given conditions, and curves F represent the values for l_0 given by formula (9) for various load factors.

For the turbine frame and the steam conditions already considered, and taking into consideration the influence of the additional factors given in the right-hand column above, the economic ratings for the various back pressures are indicated by the points which are circumscribed in Fig. 21.

It will be seen that the effect of the fuel and capital charges is such that for a back pressure of 1 inch abs.

* Also see page 662.

the rating becomes $K_c = 32\,500$ kW, i.e. 1·55 times the minimum steam-consumption rating of 20 900 kW already referred to. The corresponding outlet losses become $l_0 = 4·6$ per cent. For a back pressure of 2 inches abs. the rating becomes 45 000 kW or 1·38 times the economic rating for a back pressure of 1 inch absolute. The corresponding outlet losses for 2 inches abs. are $l_0 = 3·19$ per cent, or 69·3 per cent of the corresponding losses for a back pressure of 1 inch abs.

The variation of the economic rating with the back pressure is shown in Fig. 22 by points marked 3, the rating for 1 inch abs. being unity. The variation is practically the same as found above for the most economical rating shown by points marked 2.

The outlet losses are shown in Fig. 24 by points marked 3, the variation being similar to that which is indicated by points marked 2.

The economic ratings depend to a great extent on the factor m, which for a given evaporation e, cost of coal ϕ, and a given interest, depreciation, maintenance factor γ, is inversely proportional to the load factor λ.

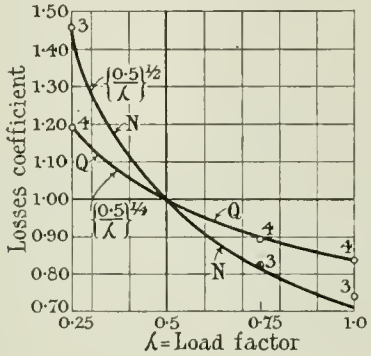


Fig. 25.—Variation of l_0 losses for varying load factor.

The variation of the economic rating and the corresponding outlet losses due to m are practically independent of the vacuum and can be represented by curves M and N respectively in Figs. 23 and 25, in relation to the load factor λ, the values for a load factor of 50 per cent being taken as unity.

For a load factor of 25 per cent the economic rating of a given frame is 20 per cent larger than for a load factor of 50 per cent, the outlet losses being 40 per cent larger; on the other hand, if the load factor is 75 per cent instead of 50 per cent, the rating would be reduced by 10 per cent and the outlet losses by 20 per cent.

The economic rating varies approximately inversely as the fourth root of the load factor, and the losses vary inversely as the square root of the load factor, and similarly it follows that the economic rating varies:

Inversely as the fourth root of the cost of coal; or
Directly as the fourth root of the interest factor; or
Directly as the fourth root of the evaporation; or
Finally, directly as the fourth root of the initial cost of the turbine.

Thus, for an increase of 20 per cent in the cost of the turbine, the rating of the turbine should be increased 5 per cent, provided that the cost of coal remains the same. It will generally be found, however, that the cost of the turbine increases with the cost of coal, these factors being industrially interdependent. The rating of the turbine would accordingly remain unchanged.

The leaving losses vary:

Inversely as the square root of the cost of coal; or
Directly as the square root of the interest factor; or
Directly as the square root of the evaporation; or
Finally, directly as the square root of the initial cost of the turbine.

(4) The Overall Economic Rating.*

In the cases considered above, no account is taken of the fact that, by installing a more efficient turbine, the boiler-house plant may be made smaller, resulting not only in decreased interest and depreciation charges for the boiler plant, but also in a decreased quota of charges for maintenance and operation.

The same applies to the condensing plant, the size of which depends, amongst other things, on the quantity of steam discharged from the turbine. Moreover, an increase in turbine efficiency reduces the heat content per lb. of steam, since the greater the efficiency of the turbine the larger is the proportion of the heat content per lb. of steam entering the turbine which is converted into mechanical work. For every 1 per cent improvement in steam consumption, the amount of heat to be dealt with per kW capacity of the installation is decreased by about 1·4 per cent, which is the amount by which the capital cost of the condensing plant will be decreased.

It may be urged that the sizes of the boiler and the condensing plant are not usually fixed within such fine limits as are contemplated above, and that the advantages of adhering to such fine limits cannot be obtained in actual practice. It is clear, however, that even if the sizes of the various parts of the plant are fixed from other considerations, considerable advantages can be obtained since, for an increase in turbine efficiency, the boiler plant will be more easily rated in actual operation, thereby effecting a reduction in maintenance charges and also an increase in boiler efficiency. The same remarks apply to the condensing plant, where a decrease in turbine consumption results in a higher vacuum and, consequently, in a further improvement in economy.

In fact, the most economical rating of a given boiler or a condenser is ascertained by exactly the same kind of considerations as have been shown to apply in the case of the turbine, i.e. by comparing the decrease in the capital costs resulting from a given increase in rating with the increase in the cost of fuel which results from the same increase in rating.

For the best commercial rating of the boiler or condensing plant the saving in capital cost for a given increase in rating is equal to the extra fuel cost entailed by the same increase in rating. It is obvious, therefore, that the financial gain resulting from the use of a more efficient turbine is equivalent to that which would have resulted had correspondingly smaller boiler and condensing plants been installed.

* Also see page 662.

In Table 14, particulars of capital costs are given of the various parts of a power station.

The cost of a power station with a steam turbine having a steam consumption differing from 10 lb. per kWh can be found as follows:

A. Remains unaltered.
B. Varies in proportion to the steam consumption.
C. Varies as the steam consumption, but in addition a reduction is to be made of 0·4 per cent for every 1 per cent decrease in steam consumption due to decrease in heat content of the steam leaving the turbine.

The cost of the station is therefore

$$C_{st} = 10{\cdot}7K + 1{\cdot}49K\,.\,s - 5{\cdot}2 \times 0{\cdot}4K\,\frac{10 - s}{10} + C_T$$

$$C_{st} = 8{\cdot}62K + 1{\cdot}698K\,.\,s + C_T$$

$$C_{st} = b_e\,.\,K + b_s\,.\,K\,.\,s + C_T \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where K = kW rating of station, s = steam consumption per kWh.

The cost of the plant consists of three main items, viz.

$C_e = b_e\,.\,K$ = that portion of the cost of the plant which is dependent only on the electrical capacity of the station and which may be termed "Electrical Capacity Cost." (It is apparent from the table above that the actual cost of the electrical plant is higher than the electrical capacity cost.)

$C_s = b_s\,.\,K\,.\,s$ = that portion of the cost of the plant which is dependent on the steam capacity of the station and which is termed "Steam Capacity Cost."

C_T = the capital cost of the turbine in £.

TABLE 14.

Approximate Cost of a Power Station of a Total Installed Capacity K_e (Economical Rating) of 100 000 kW Steam Consumption 10·0 lb. per kWh.

	Total Cost in £	Ratio of Total	Cost in £ per kW	
1. Land, buildings and foundation, roads and railway-sidings	850 000	0·303	8·50	*a*. For boiler plant, £3·5 per kW capacity. *b*. For condensing plant, £1·5 per kW capacity. *c*. For electrical plant and turbine, £3·5 per kW capacity.
2. Boiler house plant	480 000	0·171	4·80	Total cost of boiler and coal plant, £6·2 per kW capacity, or £0·62 per lb. per hour of steaming capacity.
3. Coal conveying plant	140 000	0·050	1·40	
4. Turbine	250 000	0·089	2·50	
5. Condensing plant, house set and auxiliaries	220 000	0·078	2·20	Total cost of condensing plant, £3·7 per kW capacity.
6. River work, water culverts	150 000	0·054	1·50	
7. Alternator	220 000	0·078	2·20	Total cost of electrical plant, £7·2 per kW capacity. This is independent of the steam economy of the plant.
8. Transformers	100 000	0·035	1·00	
9. Switchgear and instruments	200 000	0·071	2·00	
10. Converting plant, wiring, cranes, miscellaneous	200 000	0·071	2·00	
Total	2 810 000	1·000	28·10	

A. Cost of plant which depends on the kW capacity of the station (1*c*) + (7) + (8) + (9) + (10) .. .. = £10·7 per kW
B. Cost of plant which depends on the steaming capacity of the station (1*a*) + (2) + (3) .. = £9·7 per kW
C. Cost of condensing plant which depends on the steaming capacity and on the thermodynamic efficiency of the turbine (1*b*) + (5) + (6) .. .. = £5·2 per kW
D. Cost of turbine .. .. = £2·5 per kW

Total £28·1 per kW

For the case given above where $b_e = 8{\cdot}62$, $b_s = 1{\cdot}698$, $s = 10$ lb. per kWh, and $K = 100\,000$, we get

C_{st} = £862 000 + £1 698 000 + £250 000 = £2 810 000, i.e.

Electrical capacity cost = £862 000 = 30·6 % of total
Steam capacity cost = £1 698 000 = 60·5 % of total
Turbine cost = £250 000 = 8·9 % of total

An improvement in the steam consumption of 1 per cent would result in a saving in the steam capacity costs of about £17 000, which is 6·8 per cent of the capital cost of the turbines. On the other hand the fuel charges are

$$F = \frac{Ks}{m}\gamma$$

and 1 per cent improvement in steam consumption results in a saving in fuel charges which is equivalent to a capital value of $\frac{1}{100} \times \frac{Ks}{m}$, or, with $K = 100\,000$ kW,

$s = 10$ lb. per kWh and $m = 0{\cdot}409$, a 1 per cent improvement capitalized is equivalent to about £25 000 or, say, 10 per cent of the capital cost of the turbines.

The saving resulting from the reduction in the steam capacity costs is therefore of the same order as the direct saving in fuel costs, and it is evident that both factors must be considered in determining the economic rating of the turbine.

When the saving in the steam capacity of the station is taken into consideration the outlet losses for the economic rating are given by

$$\frac{2l_0}{1 - l_0} = \frac{K_m}{K_c + K_m} + \frac{c_T}{f + c_s} \quad . \quad . \quad . \quad (11)$$

where f = fuel cost charges in pence per kWh,
c_T = capital cost charges in pence per kWh due to turbine,
c_s = capital cost charges in pence per kWh due to steam capacity costs.

It will be seen that the additional item is now $\frac{c_T}{f + c_s}$, which is considerably less than c_T/f which appears in formula (8).

Formula (11) can be written as follows:

$$\frac{2l_0}{1 - l_0} = \frac{K_m}{K_c + K_m} + \frac{1}{1 + m \cdot b_s} m \frac{C_T}{K_c \cdot s_c} \quad . \quad (12)$$

This equation can be solved in the same manner as formula (8). It is possible, however, by applying the relationship already arrived at under (3) in connection with the determination of the economic rating, to determine the overall economic rating by multiplying the economic rating (i.e. 32 500 kW) by

$$\sqrt[4]{\left(\frac{1}{1 + mb_s}\right)}$$

or, for the case under consideration where $m = 0{\cdot}409$ and $b = 1{\cdot}7$, by

$$\sqrt[4]{\left(\frac{1}{1 + 0{\cdot}409 \times 1{\cdot}70}\right)} = 0{\cdot}875$$

In like manner the corresponding outlet losses can be obtained by multiplying the losses as calculated under (3) by

$$\sqrt[2]{\left(\frac{1}{1 + mb_s}\right)} = 0{\cdot}768$$

Thus, for a back pressure of 1 inch abs. the most economic rating becomes

$$32\,500 \times 0{\cdot}875 = 28\,400 \text{ kW}$$

and the outlet losses are $4{\cdot}6 \times 0{\cdot}768 = 3{\cdot}54$ per cent.

A separate calculation based on formula (12) gives an overall economic rating of 28 900 kW, which is 0·89 times the economic rating, and a leaving loss of 3·6 per cent. For a back pressure of 2 inches abs. the rating now becomes 41 000 kW, or 1·42 times the rating for 1 inch abs., the corresponding outlet losses being 2·52 per cent, or 0·7 times the losses for 1 inch abs.

It follows, therefore, that in this case also the ratings vary as the square root of the back pressure, and the outlet losses vary inversely as the square root of the back pressure.

It will be seen that when allowance is made for the steam-capacity costs the effect of such factors as cost of coal, initial cost of turbine, etc., on the rating becomes smaller simply because the influence of such factors is offset to some extent by the influence of the steam capacity. It will be found that the variation of the overall economic rating with coal cost, load factor, etc., is only about one half of that which is obtained in the case of the economic rating (3).

The variation of the rating with the load factor is shown by curve P in Fig. 23, and the variation of the losses with the load factor is shown by curve Q in Fig. 25, the values for a load factor of 50 per cent being taken as unity.

Calculations for other turbine frames show variations similar to those shown in Figs. 22, 23, 24 and 25, although the actual ratings depend to a great extent on the mechanical losses, leaving area, and the cost of the turbine.

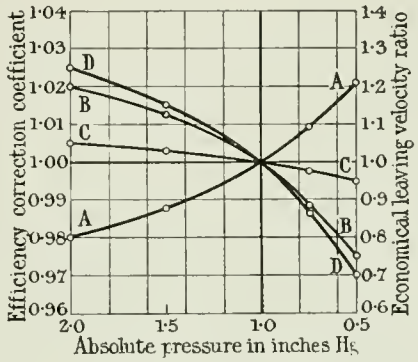


Fig. 25A.—Efficiency correction coefficient and economic leaving-velocity ratio.

As the rating varies as the square root of the back pressure, it follows that for a given turbine frame the cost per kW capacity varies inversely as the square root of the back pressure.

Table 15 gives a summary of the calculations set forth above.

The leaving velocity for the overall economic rating is 778 ft. per second for a back pressure of 1 inch abs. For other back pressures the leaving velocity can be obtained from curve A in Fig. 25A.

For a back pressure of 1 inch abs. the total losses are 6·73 per cent for the overall economic rating, and the correction in thermodynamic efficiency for other back pressures, resulting from the outlet and mechanical losses, is shown by curve B in Fig. 25A. It will be seen that for a back pressure of 2 inches abs. (Hg) the efficiency is 2 per cent better; and for a back pressure of 0·5 inch abs. 2·5 per cent worse than it is for 1 inch abs. (Hg).

This difference in efficiency does not include:

(*a*) The improvement which results from the smaller amount of work done by wet steam with higher absolute back pressures.

(*b*) The decrease in efficiency resulting from reduced reheat factor with higher absolute back pressures.

(*c*) The improvement which results from the smaller steam velocities and the correspondingly higher ratio u/c_0 in some of the stages which is obtained with higher absolute pressures.

It has been shown in Part II that the combined correction for (*a*) and (*b*) is small and can be neglected. The correction for (*c*) is estimated to be of the order of $\frac{1}{2}$ per cent for an absolute pressure ratio of 2. This correction is shown by curve C in Fig. 25A.

The resulting total efficiency correction for different back pressures is shown by curve D in Fig. 25A.

PART IV.

THE MAXIMUM OUTPUT OF TURBINES AT A GIVEN SPEED.

(1) Leaving losses.
(2) The specific speed of turbo machines.
(3) Exhaust losses.
(4) Combined leaving and exhaust losses.
(5) Review of different types of exhaust designs.
(6) Overall dimensions and weights of some large turbo sets.
(7) The law of similarity applied to steam turbines; 1 500-r.p.m. versus 3 000-r.p.m. turbines. Dimensions, weights, costs, efficiency.

TABLE 15.

Summary of Economic Ratings and Losses for Various Vacua.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Item No.	Case Considered	Leaving Area, sq. ft.	Vacuum, in. abs. (Hg)	Load Factor	Economical Rating, kW	Outlet Losses in per cent of		Mechanical Losses in per cent of Output at Coupling	Total Losses in per cent of Output at Coupling	Ratio of Thermodynamic Efficiency referred to 1 in. abs.	Leaving Loss in per cent of Heat Drop	Leaving Loss, B.Th.U. per lb.	Leaving Velocity	
						Heat Drop	Output at Coupling						Ft. per sec.	Ratio
1	No. 1	58·8	1	Any	19 250	1·64	1·74	4·33	6·07	1	1·26	5·35	518	1
2	No. 2	58·8	1	Any	20 900	1·88	2·0	3·99	5·99	1	1·45	6·15	556	1
3	No. 3	58·8	2	0·5	45 000	3·19	3·36	1·85	5·21	1·022	2·5	9·7	697	0·798
4	No. 3	58·8	1·5	0·5	39 600	3·6	3·8	2·11	5·91	1·015	2·82	11·4	755	0·865
5	No. 3	58·8	1·0	0·5	32 500	4·6	4·94	2·57	7·51	1	3·6	15·2	872	1
6	No. 3	58·8	0·75	0·5	27 700	5·5	6·0	3·01	9·01	0·986	4·3	18·8	970	1·112
7	No. 3	58·8	0·5	0·5	23 700	6·5	7·2	3·52	10·72	0·971	5·07	23·1	1 070	1·23
8	No. 4	58·8	2	0·5	41 000	2·52	2·64	2·04	4·68	1·020	1·99	7·72	622	0·798
9	No. 4	58·8	1·5	0·5	35 400	2·9	3·06	2·36	5·42	1·013	2·30	9·3	682	0·874
10	No. 4	58·8	1·0	0·5	28 900	3·6	3·84	2·89	6·73	1	2·85	12·05	778	1
11	No. 4	58·8	0·75	0·5	24 800	4·25	4·6	3·37	7·97	0·986	3·32	14·5	852	1·092
12	No. 4	58·8	0·5	0·5	21 100	5·00	5·47	3·96	9·43	0·975	3·9	17·8	945	1·212

It is evident from the results obtained that in order to determine the rating of a turbine it is necessary to know the average vacuum with which the turbine will be run. To ascertain the vacuum, it is necessary to know the average cooling-water temperature obtaining throughout the year. If too high a temperature is given to the manufacturers, as is often the case, the turbine will be large enough as far as load-carrying capacity, i.e. steam-chest and nozzle capacity, is concerned, but the exhaust end will be too small, thereby impairing the efficiency of the turbine. On the other hand, if too low a temperature is given, the turbine will be unnecessarily expensive.

It is interesting to note that American practice has standardized turbine ratings on the basis of a vacuum of 29 inches (barometer 30 in.) although the average cooling-water temperature would justify a vacuum slightly higher than this.

(1) LEAVING LOSSES.

The most important of the outlet losses are the leaving losses. Leaving losses depend directly on the leaving area (A_l) of the last row of moving blades, which is given by the following formula (see Fig. 26):

$$A_l = \tau \pi D h \quad . \quad . \quad . \quad . \quad . \quad (1)$$

D = mean diameter of blades,
h = useful length of last blades,
τ = thickness coefficient = 0·95.

In actual practice the variation of τ will be negligible, and it is therefore proposed to make all comparisons in this paper on the basis of the blade area

$$A = \pi D h \, . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

It has been proposed to decrease the leaving losses by adding to the last row of moving blades a diffuser

which converts the leaving velocity into pressure energy. The increase in output which can be obtained by adding a diffuser to a given area of moving blades is relatively small because, if a diffuser effect is obtained, the absolute steam pressure in front of the diffuser is less than the pressure in the exhaust and, consequently, the steam volume in front of the diffuser is larger than that in the turbine exhaust. For this reason, increasing the steam velocity through the last row of moving blades increases the weight of steam to a small extent only, particularly for a high vacuum. This is clear from the following example: For a very high vacuum, say 0·75 inch abs. pressure in the turbine exhaust, it is usual to accept leaving velocities of 800 ft. per second. Increasing the velocity through the last row of moving blades to 1 200 ft. per second and reconverting it in a diffuser would produce a pressure in front of the diffuser of 0·55 inch abs., causing an increase in specific

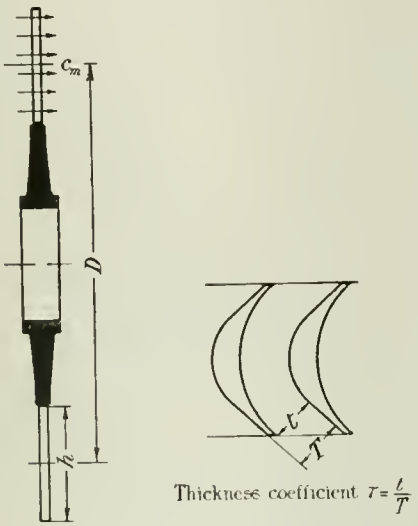


FIG. 26.—Last wheel of a turbine.

volume of 36 per cent. The increase in steam weight is, therefore, only

$$\frac{1\cdot50}{1\cdot36} - 1 = 10 \text{ per cent}$$

with an increase of 50 per cent in the leaving velocity at the last row of moving blades.

As the efficiency of the diffuser is less than unity, the increase in specific volume would not be quite as large as 36 per cent, but, on the other hand, the pressure in front of the diffuser and the consequent additional heat drop will be smaller, and the increase in steam weight for a given leaving loss will be even smaller than that calculated above. It is evident, therefore, that the improvement obtainable with exhaust diffusers is very problematical, and the only effective method of reducing the leaving losses is to provide additional blade area for the last row of moving blades.

The blade area is limited by the stresses at the blade root, which for a blade of uniform cross-sectional area are given by the following formula:

$$\sigma_b = 2\mu \cdot u^2 \Big|_D^h = \tfrac{1}{2}\mu\omega^2 Dh \quad . \quad . \quad . \quad . \quad (3)$$

where σ_b = stress in the blade root,
μ = the specific mass of material (for steel 8×10^{-6} kg cm^{-4} sec.^{2} or $7\cdot35\times10^{-4}$ lb. in.$^{-4}$ sec.^{2}),
u = mean peripheral velocity,
h = blade height,
D = mean diameter of disc,
ω = radians per second.

As the blade area $A = \pi Dh$ we get

$$\sigma_b = \frac{1}{2\pi}\mu\omega^2 A \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and for

$$1\,500 \text{ r.p.m.} \quad \sigma_b = \frac{Dh}{248} \text{ tons per sq. in.} = \frac{A}{5\cdot4} \quad . \quad (5)$$

$$3\,000 \text{ r.p.m.} \quad \sigma_b = \frac{Dh}{62} \text{ tons per sq. in.} = \frac{A}{1\cdot35} \quad . \quad (6)$$

(σ_b in tons per sq. in., D and h in inches, A in sq. ft.)

It will be seen that these stresses do not depend upon the mean diameter but upon the blade area only. For a given stress the blade area varies inversely as the square of the r.p.m., and for a given r.p.m. the blade area is proportional to the stress.

The blade area can be increased without increasing the stresses by providing blades, the cross-sectional area of which increases towards the root. The maximum increase is obtained by tapering the blade towards the root in such a way that the stress in the tapered portion is constant. The increase thus possible is given by the following formula:

$$\Delta A = A \log \frac{f_1}{f} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

and the total blade area is

$$A_1 = A + \Delta A = A\left(1 + \log \frac{f_1}{f}\right) \quad . \quad . \quad (8)$$

where A = maximum possible blade area for uniform blade section for a given stress,
A_1 = maximum possible blade area for taper section for the same stress,
f = cross-sectional area of blade at parallel portion,
f_1 = cross-sectional area of blade at root.

Fig. 26A shows the increase in the blade area which can be obtained by increasing the cross-sectional area. For an increase of 50 per cent in the blade area, it is necessary to increase the cross-sectional area 65 per cent. To double the blade area, it is necessary to increase the cross-sectional area by 172 per cent.

The increase in cross-sectional area can be obtained by increasing the thickness of the blade, commencing with a thickness at the tip of the blade to be fixed mainly from considerations of strength of rivets required to hold the shrouding. The maximum thickness at

the root is limited by the condition that the passage left between the blades must be such that the steam can pass without undue throttling. Referring to Fig. 27, the width of this passage is calculated from the velocity diagram W, and the pitch p of the blades from the formula :

$$\frac{a}{p} = \frac{w_{1a}}{w_1} = \frac{\text{axial component of relative velocity}}{\text{relative velocity}}$$

The thickness t of the blade is obtained from :

$$\frac{t}{p} = \frac{p - a}{p} = 1 - \frac{w_{1a}}{w_1}$$

Curve A shows the variation of this ratio as a function of the speed ratio u/c_0 and, consequently, also as a function of the relative diameter at any point on the blade.

The pitch p of the blades is proportional to the diameter, and is fixed by the condition that the steam is properly guided at the tips of the blade. The permissible thickness t is given by curve B, which shows that it is a maximum for $u/c_0 = 0{\cdot}425$, i.e. for usual design practice at about the mean diameter. For usual blade heights, t is practically constant.

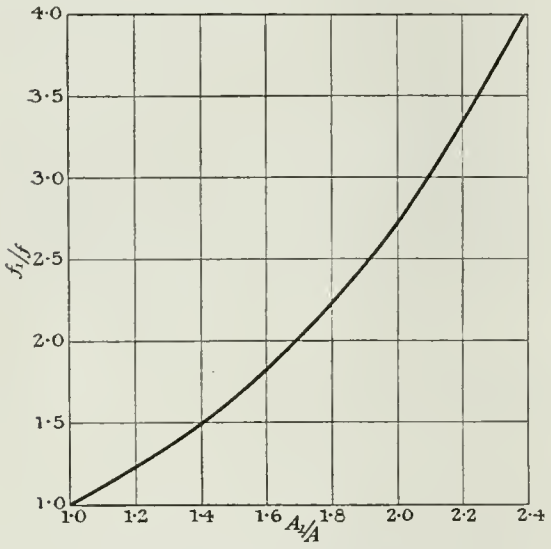


Fig. 26A.—Increase in blade leaving-area as a function of the increase in cross-sectional area.

Blade sections C, D, and E show the resulting theoretical blade sections at various diameters for blades of uniform width, in thin lines. The sections which are possible in practice are shown in thick lines.

With a parallel blade the increase in the cross-sectional area is about 60 per cent, and the consequent increase in leaving area is about 45 per cent, according to Fig. 26A.

To obtain a larger increase it would be necessary to increase the width of the blade as shown by F, G, and H in Fig. 27 ; in which case an increase in cross-sectional area at the root of about 190 per cent could be obtained.

With such a blade it would be possible to obtain an increase in the blade area of about 105 per cent, i.e. a blade area about 40 per cent larger than that possible with a parallel blade tapered in thickness towards the root.

These relations depend to a small extent on the ratio h/D since, with a small height of blades, the pitch at the root of the blades is relatively larger and the possible thickness of the blades is consequently also larger, but this influence is not material within a fairly wide range, as is shown by curve B.

As the stresses are dependent upon the blade area only, and not on the height of the blades relative to the mean diameter, the blade area permissible for a

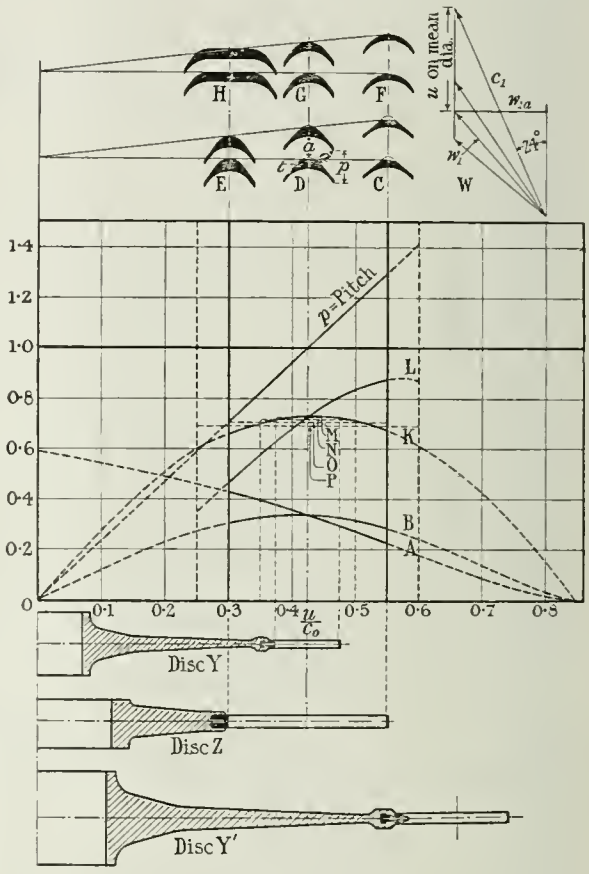


Fig. 27.

given stress may be obtained either with a large mean diameter and a relatively short blade, or with a small mean diameter and a long blade. In the first case, the disc becomes relatively large and thin, and the danger of disc vibration must be faced, with the consequent possibility of breakage of disc or blades. By adopting long blades, discs of small diameter can be obtained ; such discs can easily be made sufficiently rigid to prevent vibration and, if the support of the blade is rigid, the possibility of blade vibration is greatly minimized. For these reasons a fairly large ratio h/D is preferable.

The disc Y shows a wheel with blades of an A.E.G.

TABLE 16.

Particulars of Last Row of Moving Blades.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	16a	17	18	19
Item No.	Reference to Tables 1 and 2	Reference and Date		Manufacturer	Speed	Mean Diameter, D	Useful Length of Blade, h	Ratio, $\frac{D}{h}$	Mean Peripheral Speed	Tip Peripheral Speed	Leaving Area, $\tau = 1{\cdot}0$ A_0	Stress in Blade Root		Particulars of Blade		$\frac{f_1}{f}$	Particulars of Discs		
												Parallel Blade, Centrifugal Stress	Actual Blade, Actual Stress C.F. + Bending	Axial Width of Blade Section at Tip	Axial Width of Blade Section at Root		Thickness at Boss	Outside Disc Diameter, D_d	Max. Stress at Bore at Normal Speed
					r.p.m.	in.	in.		ft./sec.	ft./sec.	sq. ft.	tons per sq. in.	tons per sq. in.	in.	in.		in.	in.	tons per sq. in.
1	10/1	Chicago, North-West	1917	W.E.	1 200	98	18	5·45	514	608	2 × 38·4	4·55	—	—	—	—	—	—	—
2	11/1	Narragansett	1917	W.E.	1 200	98	18	5·45	514	608	2 × 38·4	4·55	—	—	—	—	—	—	—
3	13/1	Interborough	1918	W.E.	1 500	87	15	5·80	570	668	2 × 28·5	5·26	—	—	—	—	—	—	—
4	19/1	Muscle Shoals	1919	W.E.	1 200	98	18	5·45	514	608	2 × 38·4	4·55	—	—	—	—	—	—	—
5	12/1	Detroit Edison	1918	G.E.	1 200	140	34*	4·12	734	912	104	12·3	—	—	—	—	—	—	—
6†	—	Boston	*Electrical Review* 12/4/18	G.E.	1 500	117	28	4·18	766	950	71·4	13·2	—	—	—	—	—	—	—
7‡	15/1	Chicago, North-West	1918	G.E.	1 500	120	28	4·28	787	970	73·3	13·6	—	—	—	—	—	—	—
8‡	16/1	Chicago, Fisk Street	1918	G.E.	1 800	100	24	4·16	787	975	52·3	14·0	—	—	—	—	—	—	—
9§	—	*Proc. A.I.E.E.*	Nov. 1919	G.E.	1 800	88	22	4·00	693	865	42·3	11·3	—	—	—	—	—	—	—
10	—	*Proc. A.I.E.E.*	Nov. 1919	G.E.	3 600	51	9½	5·60	803	946	10·2	10·8	—	—	—	—	—	—	—
11	18/1	Cologne	1919	A.E.G.	1 000	134	20	6·70	585	674	58·5	4·8	—	—	—	—	—	—	—
12	11/2	Author	1917	M.-V.	3 000	54	9½	5·68	708	832	11·2	8·27	6·6	$1\frac{3}{16}$	$1\frac{3}{16}$	2·08	$1\frac{7}{8}$	$43\frac{1}{4}$	10·0
13	21/1	Author	1919	M.-V.	1 500	84	24	3·50	550	708	44·0	8·12	6·5	2	2	1·86	5	$59\frac{1}{2}$	7·43
14	22/2	Author	1921	M.-V.	3 000	42	12	3·50	550	708	11·0	8·12	8·14	$1\frac{1}{16}$	$1\frac{1}{16}$	1·72	$2\frac{13}{16}$	$28\frac{3}{8}$	9·65

* Has been reduced to 28 inches owing to vibration trouble.

† Turbine wrecked due to deflection of 17th diaphragm.

‡ Difficulties due to disc and blade vibration.

§ Proposed dimensions to supersede dimensions given under item 8.

turbine with $h/D = 1/8{\cdot}5$, and disc Z shows the practice preferred by the author with $h/D = 1/3{\cdot}5$. As shown, the mean diameter is the same and the blade area of Z is 2·4 times that of Y. To obtain the same blade area it is necessary to increase the size of Y by 55 per cent, thus obtaining the wheel Y′ shown below Z for the purpose of comparison. The peripheral speed of Y′ is 55 per cent greater than that of Z and, whilst the blade stresses are the same, the disc is much larger and consequently its stresses are increased and, further, it is more liable to vibration.

It is sometimes argued that the efficiency of a relatively long blade is much worse than that of a shorter blade. In Fig. 27, curve K shows the efficiency obtained in the different parts of the blade, and curve L the relative work done per inch length of blade, from which the mean efficiencies for various blade heights have been calculated. These are : for

$\frac{h}{D} = \frac{1}{8{\cdot}5}$, η (mean) = 72·5 per cent (shown by M)

$\frac{h}{D} = \frac{1}{5{\cdot}67}$, η (mean) = 71·5 per cent (shown by N)

$\frac{h}{D} = \frac{1}{3{\cdot}5}$, η (mean) = 70·5 per cent (shown by O)

$\frac{h}{D} = \frac{1}{2{\cdot}6}$, η (mean) = 69 per cent (shown by P)

The loss in efficiency resulting from the use of long blades is therefore very small, the difference between the rotors shown being only 2 per cent, and, considering that only the last few rows are affected, it is certainly not worth while running the additional risks connected with large discs, which for reasons of costs are not usually made of sufficient strength to prevent dangerous vibration. In any case, this slight difference in efficiency can be compensated for by providing a slightly larger leaving area.

To obtain the highest possible efficiency with long blades it is desirable to vary the inlet angle of the blade slightly, and there is little additional cost involved in providing this. To prevent the vibration of long blades it is good practice to provide intermediate lugs, which form a complete circle when the blades are assembled.

The principal difficulty in connection with relatively long blades is the fixing of the blade on the disc. This is the real limiting factor, owing to the fact that the root of the blade becomes relatively thin. With a root of the kind shown in disc Z, however, the stress can be kept below that at the root of the blade itself.

In calculating the stresses in long blades it is necessary to consider the bending stresses at the root due to the action of the steam, and to keep this stress within reasonable limits (say 20 per cent of the centrifugal stresses) it is necessary to increase the axial width of the blade either over the whole length or at least towards the root. In general, the axial width of the blade at the root should not be less than 1/12th of the length of the blade.

It is interesting to compare the practice of various manufacturers as shown in Table 16. In the G.E. turbine (35 000 kW, 1 500 r.p.m., item 7) installed at the North-West station in Chicago, the blade area of the last row of blades is 73·3 sq. ft., from which the stress in a blade of uniform cross-sectional area can be calculated from formula (5).

$$\sigma_b = \frac{73{\cdot}3}{5{\cdot}4} = 13{\cdot}6 \text{ tons per sq. in.}$$

The bending stress in this blade would increase this stress by at least 20 per cent but, owing to the increase in cross-sectional area possible with this parallel blade, the stress would be reduced in the ratio of 1·45 to 1·0, and the resulting stress is therefore about 11·3 tons per square inch as compared with 6·5 tons per square inch obtained in multi-exhaust turbines of similar capacities (see item 13, col. 14). The difficulties which are experienced in connection with these American machines, however, fully justify the more conservative policy which has been made possible by the use of the multi-exhaust principle.

The fact that in the American machines steel of higher physical qualities may have been used, as would appear from the information published (see items 5-8 of Table 17) does not materially affect the position, as it is very risky to take full advantage of such properties, having in mind that such higher properties are generally accompanied by increased brittleness which may actually result in decreased safety. Heat treatment by quenching turbine discs of 8 ft. diameter to obtain an elastic limit (limit of proportionality) of 24·5 tons per square inch is very doubtful procedure, and it is not safe to assume that the treated discs will be free from internal shrinkage stresses.

These contentions are fully supported by the papers presented at the 355th Meeting of the American Institute of Electrical Engineers, 14th November, 1919, by representatives of the manufacturers of large turbo sets, and by the remarks made during the discussion.*

These papers deal with speed limits and the power of large single-shaft turbines and alternators, and give the result of the latest experience in connection with large units. They constitute a most important addition to the literature on steam turbine practice, and a good deal of the information given there has been embodied in the tables and diagrams given in this paper.

In the paper dealing with the practice of the General Electric Company, it is interesting to note that the largest turbines running at 1 800 r.p.m. are now being built for an economical capacity of 21 000 kW at 28½ in. vacuum, the mean diameter of the last wheel being 88 in., and the blade height being 22 in. A great number of turbines had been built with a mean diameter of 100 in. and a blade height of 24 in., the economical rating being 22 500 kW for a vacuum of 29 in. (see items 8 and 9 of Table 16).

The blade area in the latest type is 42·3 sq. ft., whereas the older type had a blade area of 52·3 sq. ft. ; the economical capacity is decreased by approximately

* J. F. Johnson: "Present Limits of Speed and Power of Single-shaft Steam Turbines," *Proceedings of the American Institute of Electrical Engineers*, 1919, vol. 38, p. 1515.
E. Berg: "Present Limits of Speed and Power of Single-shaft Curtis Steam Turbines," *ibid.*, p. 1527.
F. D. Newbury: "Present Limits of Speed and Output of Single-shaft Turbo-generators," *ibid.*, p. 1537.
Discussion on all three papers, *ibid.*, p. 1547.

TABLE 17.

Material Specification, Stresses and Factors of Safety.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
					Material Specification						Overspeed		Stress	Factor of Safety at Normal Speed referred to			
Item No.	Reference to Tables 1 and 2	Reference and Date	Manufacturer	Stressed Part	Kind	T.S.	Y.P.	Proportional Limit	Elongation	Minimum Reduction in Area	Emergency Governor per cent over Normal	Over Speed Test per cent over Normal	At Normal Speed	T.S.	Y.P.	Proportional Limit	Mean of T.S. and Y.P.
						tons per sq. in.	tons per sq. in.	tons per sq. in.	%	%	%	%	tons per sq. in.				
1	23/1	Author	M.-V.	Disc	Carbon Steel	40	20	16	18	32	10	20	7·85	5·1	2·55	2·04	3·82
2	23/1	Author	M.-V.	Blades	5 % Nickel Steel	40	30	20	25	60–70	10	20	6·5	6·15	4·62	3·08	5·38
3	18/2	Author	M.-V.	Disc	Chrome Nickel Steel	40	20	16	18	35	10	20	7·83	5·1	2·55	2·04	3·82
4	18/2	Author	M.-V.	Blades	5 % Nickel Steel	40	30	20	25	60-70	10	20	8·14	4·9	3·68	2·46	4·29
5	—	*Proc. A.I.E.E.*, Nov. 1919, p. 1247	W.E.	Turbine Rotor	0·25 Cast Steel	29–31	13·4	9·8/11	15–18	20–25	10	20	6·2	4·68	2·17	1·58	3·43
6	—	*Proc. A.I.E.E.*, Nov. 1919, p. 1249	W.E.	Blades	5 % * Nickel Steel	29	20	15·6	30	60	10	20	7·75	3·74	2·58	2·01	3·16
7	—	*Proc. A.I.E.E.*, Nov. 1919, p. 1227	G.E.	Disc	3 % † Nickel Steel	—	—	24·5	—	—	—	20	—	—	—	—	—
8	—	*Proc. A.I.E.E.*, Nov. 1919, p. 1227	G.E.	Blades	—	—	—	—	—	—	—	20	—	—	—	—	—
9	—	" Konstruktion und Material im Bau von Dampfturbinen und Turbodynamos," by O. Lasche. Publisher, J. Springer, Berlin	A.E.G.	Disc	—	44·5	31·7	—	18	—	—	—	9·75	4·56	3·26	—	3·91
10‡	—		A.E.G.	Blades	5 % Nickel Steel	36·8	29·2	—	20	—	—	—	9·5	3·87	3·08	—	3·47
11§	—		A.E.G.	Blades	5 % Nickel Steel	36·8	29·2	—	20	—	—	—	11·4	3·22	2·56	—	2·89

* Less than 0·08% carbon. † Quenched and tempered. ‡ Tensile stress at root of blade. § Tensile stress at T-portion of blade.

20 per cent, but the machine is safer as the stresses will be reduced in proportion.

It is obvious that a reduction in the economical capacity of a unit would not have been introduced unless it had been rendered advisable by the experience gained in connection with the larger unit.

(2) Specific Speed of Turbo Machines.

The problem of passing a maximum quantity of working fluid through a rotor of minimum dimensions is by no means confined to a steam turbine. Exactly the same problem applies to all types of turbo machines, such as water turbines, centrifugal pumps, blowers, fans, etc. The tendency in all these machines is that a given volume should be passed through a rotor of minimum size, so as to enable the machine to be run at the highest possible speed. This decreases not only the cost of the machine itself but also that of the driven generator or the motor driving it.

It has become standard practice in water turbines to compare rotors on the basis of their capacity for dealing with water at a given head and speed, or, in other words, their capacity for producing power by means of what has been termed "the specific speed," which is represented by:

$$n_s = \frac{n\sqrt{N}}{H^{5/4}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where n = actual r.p.m. for a given brake horse-power N and head H, in metric units.

For a rotor of given proportions and given relations between water velocities and peripheral speeds, the specific speed is independent of the actual size of the rotor.

The author proposes to revert to the original practice by introducing into this formula the volume (Q), as this gives a more rational basis for comparing prime movers with driven machines.

In a water turbine, the brake horse-power when Q and H are expressed in metric units is:

$$N = \frac{1\,000\,QH\eta}{75} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or if, to eliminate efficiency, we assume $\eta = 0{\cdot}75$ we get $N = 10QH$ and therefore

$$n_s = \frac{n\sqrt{(10Q)}}{H^{3/4}} \quad . \quad . \quad . \quad . \quad (3a)$$

where n is given in r.p.m.,
Q is given in cubic metres per sec.,
and H is given in metres,

or

$$n_s = 1{\cdot}3n\,\frac{\sqrt{Q}}{H^{3/4}} \quad . \quad . \quad . \quad . \quad . \quad (3b)$$

where n is given in r.p.m.,
Q is given in cubic feet per sec.,
and H is given in feet,

or

$$n_s = n\,\frac{\sqrt{Q}}{14{\cdot}9\,H^{3/4}} \quad . \quad . \quad . \quad . \quad (3c)$$

where n is given in r.p.m.,
Q is given in gallons per min.,
and H is given in feet.

As the specific speed n_s is a function of the relations between the dimensions, water velocities and peripheral speeds, but is independent of the actual size and actual velocities, it is evident that an expression can be obtained for n_s containing the dimension, velocity and speed ratios.

The volume flowing through any part of a rotor is proportional to the area of any circumferential cross-section of the passages and to the mean velocity perpendicular to that area, so that

$$Q = \pi Dhc_m \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and the mean peripheral speed is given by

$$u = \frac{\pi Dn}{60} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The water velocity due to the head to be utilized is given by

$$c_0 = \sqrt{(2gH)} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Introducing this expression into (3) we get

$$n_s = 1\,000\,\frac{u}{c_0}\sqrt{\left(\frac{c_m}{c_0}\cdot\frac{h}{D}\right)} \quad . \quad . \quad . \quad (7)$$

This formula contains the simple relation between the specific speed and the dimension, velocity and peripheral speed ratios, and indicates to the designer all the steps he has to take to increase the speed of a machine. The established formula merely indicates to the estimator the approximate speed of a machine for given conditions, i.e. for a given head and quantity.

It is clear that the speed of a machine can be increased:—

(1) By increasing u/c_0. This ratio, however, determines principally the efficiency of the machine, and to increase it beyond certain limits may cause a considerable falling off in the efficiency.
(2) By increasing c_m/c_0, which factor largely determines the leaving losses and thus also affects the efficiency.
(3) By increasing h/D, which is limited mainly by the flow through the runner.

By means of this formula (7), it is possible to compare runners of all types of turbo machines on a rational basis. Such a comparison is given in Table 18 for water turbines, pumps, blowers, compressors and steam turbines. This list also contains the ratio δ introduced by Professor Rateau. This ratio is also based on the volume Q which an impeller of given dimensions can deal with, and as this volume is proportional to the peripheral speed and the square of the radius ($\frac{1}{2}D$) we get:

$$Q = \delta u\left(\frac{D^2}{4}\right) \quad . \quad . \quad . \quad . \quad . \quad (8)$$

and thus

$$\delta = \frac{4Q}{u}\cdot\frac{1}{D^2} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

TABLE 18.

Comparison of Different Types of Turbo Machines.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
					Operation Characteristics						Design Characteristics				
Item No.	Customer	Year Installed	Manufacturer	Type of Machine	Volume, Q	Total Head for One Stage, H	Power for One Stage, N	Speed, n	Efficiency η	Specific Speed, n_t	$\frac{h}{D}$	$\frac{u}{c_0}$	$\frac{c_m}{c_0}$	$\frac{c_m}{u}$	Rateau Factor, δ
					m^3/sec.	m	b.h.p.	r.p.m.	%						
1*	Chute de l'Anse	1915	E. W. Cie	Water Turbines: A. Horizontal double spiral enclosed	2·5	140	3 990	600	85·5	52·5	0·073	0·659	0·086	0·13	0·119
2*	Yentavon, Hautes Alpes	1910	P. P. Cie	B Horizontal double spiral enclosed	12·0	50	6 875	300	86·0	123·5	0·118	0·705	0·26	0·367	0·545
3*	Massboden	1916	P. P. Cie	C. Horizontal double spiral enclosed	7·9	43·2	4 100	500	90·0	187·0	0·241	0·746	0·26	0·349	1·06
4*	Isarwerk	1905	E. W. Cie	D. Horizontal double open	18·75	11·6	2 500	150	86·0	230·0	0·337	0·82	0·235	0·287	1·220
5*	Cedar Rapids	1916	Morris	E. Vertical single enclosed	100	9·15	10 900	55·6	89·5	336	0·55	0·80	0·32	0·40	2·76
6*	Ravenshurg Test	1915	E. W. Cie	E. Vertical single open	1·5	1·0	16·4	113	82·0	436	0·39	0·85	0·672	0·79	3·87
7†	Proposed (1918)	—	—	F. Zuppinger	61	10	6 350	200	78·0	878	0·44	1·55	0·732	0·470	2·60
8‡	Graimat Neusiedl.	1919	I. Storek	G. Kaplan	1·2	2·18	27·2	460	78·0	885	0·687	1·245	0·736	0·59	5·080
					galls. per min	ft.									
9§	Anglo-Persian Oil Co., Ltd.	1920	Mather & Platt	Water Pumps: High lift	1 200	420	206	2 000	74·0	50·2	0·0667	0·875	0·0492	0·0562	0·047
10§	Anglo-Persian Oil Co., Ltd	1920	Mather & Platt	Medium lift	3 800	230	324	1 475	82·0	103·5	0 090	1·042	0·110	0·1052	0·1190
11§	Shanghai Municipal Council	1921	Mather & Platt	Low lift	18 000	51	355	490	83·0	221	0·251	1·135	0·151	0·133	0·420
12§	S. Bullock & Co., Ltd.	1921	Mather & Platt	Low lift	2 200	35	29·2	1 250	80·0	274	0·268	1·265	0·1755	0·1385	0·467
13	Penalta	1911	Metro.-Vickers	Low lift (turbine driven)	5 500	50	166·5	2 500	50·0	660	0·328	1·5	0·597	0·397	0·82
					ft.^{3}/sec.	ft.			Isoth.						
14	New Hucknall	1912	Metro.-Vickers	Compressor	125	3 150	89	3 940	60·0	136	0·0417	1·15	0·335	0·291	0·152
15	Whitwell	1921	Metro.-Vickers	Blower	270	3 350	192	2 950	Adiab. 71·5	144	0·0697	1·0	0·296	0·296	0·258
16	Metro.-Vickers	1920	Davidson	Fan	334	545	41	1 150	62·0	242	0·474	0·765	0·212	0·277	1·65
					m^3/sec.										
17	Northampton	1912	Metro.-Vickers	Steam Turbines: Last stage	29·3	6 800 m 28·8 B.Th.U.	135·5	3 000	77·0	69·0	0·112	0·458	0·201	0·437	0·615
18	Sydney	1921	Metro.-Vickers	Last stage	114·5	9 400 m 39·6 B.Th.U.	588	3 000	73·5	107·0	0·286	0·392	0·261	0·665	2·39
19	Standard practice	1920	Metro.-Vickers	Last stage	308	11 900 m 44·8 B.Th.U.	752	2 400	71·0	117·0	0·272	0·414	0·296	0·715	2·41

* These particulars have been kindly supplied by Mr. Eric M. Bergstrom, who dealt with this subject in his paper on "Recent Advances in Utilization of Water Power" before the I.M.E. on 23rd January 1920.
† *Schweizerische Bauzeitung*, 1st December, 1917, p. 255.
‡ *Zeit. f. d. g. Turbinenwesen*, 10th July, 1920, p. 129.
§ This information has been kindly supplied by Mr. T. Y. Sherwell, of Messrs. Mather & Platt.

or as $$Q = c_m \pi D h \quad . \quad . \quad . \quad . \quad . \quad (10)$$

we get: $$\delta = 4\pi \left(\frac{c_m}{u}\right)\left(\frac{h}{D}\right) \quad . \quad . \quad . \quad . \quad (11)^*$$

It is interesting to find that the development which has taken place in the design during recent years corresponds to the development which has occurred in connection with steam turbines. This similarity of development is particularly noticeable in the case of water turbines designed for low heads, and is clearly illustrated in Fig. 28 and summarized in Table 18.

In the case of the steam turbine the designer has the advantage over the water-turbine designer of being

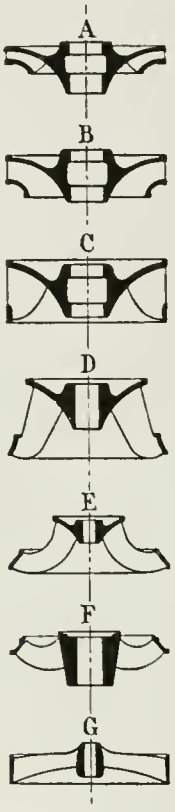


Fig. 28.—Development of water-turbine runners.

able to choose the head, i.e. the heat drop for the last stage.

For a given specific speed the volume which can be dealt with by the runner is given by:

$$Q = \left(\frac{c_0}{9{\cdot}55}\right)^3 \left(\frac{n_s}{n}\right)^2 = \left(\frac{u}{9{\cdot}55}\right)^3 \times \frac{(n_s/n)^2}{(u/c_0)^3} \quad . \quad (12)$$

i.e. the volume is proportional to the third power of the steam velocity; or, for a given ratio u/c_0, it is proportional to the third power of the peripheral speed.

In this case, however, the leaving velocity is increased in direct proportion to the peripheral speed. This will not affect the efficiency of the last stage, but the overall efficiency of the turbine will usually be affected, because a proportionately larger heat drop will have to be dealt with by the last stage, the efficiency of which is usually lower than that of most of the other stages.

The advantages which are claimed * to be possible with very high peripheral speeds for the last stage are not, therefore, realized in actual practice. For this reason, the author prefers to make the comparison, as far as steam turbines are concerned, on the basis of the leaving velocity, and on this basis the capacity of a turbine increases as the square of the peripheral speed only for a given ratio h/D.

(3) Exhaust Losses.

The exhaust losses depend not so much on the size of the exhaust opening, as on the shape and arrangement of it with relation to the last row of blades and to the condenser.

One of the main essentials in the design of large capacities for high vacuum is the proper shape of the turbine exhaust.

In 1912, the general practice was a more or less correctly-shaped exhaust casing without any attempt to guide the steam from the last row of exhaust blades to the exhaust port, which was generally of a circular shape. From this port the steam had to be distributed over the condenser surface by means of deflector plates. The circular exhaust port was usually kept as small as possible, and in addition was partially covered by the turbine cylinder, which overhung into the turbine exhaust. The flow of steam from the turbine to the condenser was thereby unduly throttled, with the result that in some cases a drop in absolute pressure of as much as 2 inches occurred between the turbine blades and the condenser (see *Journal I.E.E.*, 1919,

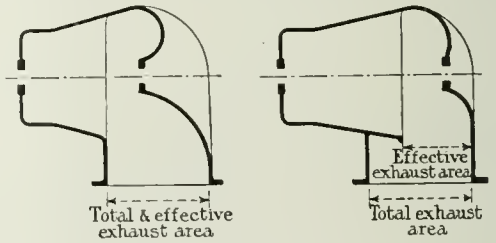


Fig. 29.—Exhaust shapes, showing exhaust area.

vol. 57, p. 101). Even turbines quite recently constructed are incorrectly designed in this respect.

In Fig. 12, for instance, 50 per cent of the exhaust port is covered by the turbine cylinder which extends into the turbine casing. This results in 50 per cent of the total exhaust area being rendered ineffective, and the losses are consequently four times larger than with an exhaust properly arranged. In Fig. 11 the exhaust steam is forced across the passages provided to take one-half of the steam from the last but one stage to the extreme half of the last stage. Thus, the exhaust passages are very restricted and, in addition, the steam is made to change its direction of flow very abruptly, thus causing losses which are difficult to calculate.

* See *Engineering*, 1920, vol. 109, p. 355.

* E. Berg: *loc. cit.*

In Figs. 13 and 14, turbine exhausts are shown in which the steam is directed by means of simple baffles from the last row of blades to the condenser surface in such a manner that the path of the steam is simple and short. Eddy losses are reduced to a minimum, and the steam is uniformly distributed over the whole condenser surface.

The actual losses in the turbine exhaust are very difficult to determine when the pressure drop is small due to the exhaust being properly designed.

It is not expected that in a well-designed exhaust the drop would be more than that which corresponds to the head which is necessary to produce the velocity at the effective exhaust opening (A_x) shown in Fig. 29, assuming that the head due to the leaving velocity is also lost.

On this basis the losses are given by:

$$l_x = \frac{c_x^2}{2gJ}$$

$$l_x = \left(\frac{c_x}{91 \cdot 5}\right) \text{ in metric units}$$

$$l_x = \left(\frac{c_x}{224}\right) \text{ in English units}$$

where c_x = velocity through exhaust opening in metres per second or ft. per second,
l_x = loss in heat drop in calories per kilogram and B.Th.U. per lb. respectively.

(4) Combined Leaving and Exhaust Losses.

The combined leaving and exhaust losses are the sum of the leaving and exhaust losses and are given by the formula:

$$l_0 = l_l + l_x = \left(\frac{c_l}{91 \cdot 5}\right)^2 + \left(\frac{c_x}{91 \cdot 5}\right)^2 = \left(\frac{c_q}{91 \cdot 5}\right)^2$$

in metric units.

$$l_0 = l_l + l_x = \left(\frac{c_l}{224}\right)^2 + \left(\frac{c_x}{224}\right)^2 = \left(\frac{c_q}{224}\right)^2$$

in English units.

c_q is the velocity which would produce the same loss as the exhaust and leaving losses combined. The size of an exhaust port required to pass the exhaust steam volume at the velocity c_q gives us an equivalent exhaust area A_q, which provides a convenient measure for comparing the capacity of turbine exhausts.

This equivalent exhaust area A_q is that area which would produce the same loss in pressure or heat drop as would be produced by the leaving and exhaust losses combined. It can be calculated from the leaving and exhaust areas as follows:

$$\left(\frac{A_l}{A_q}\right)^2 = 1 + \left(\frac{A_l}{A_x}\right)^2$$

For usual proportions, i.e. when $A_x = 2A_l$, we get

$$A_q = \frac{A_l}{1 \cdot 12}$$

(5) Review of Different Types of Exhaust Designs.

The demand for large and efficient turbines for large power stations, which has followed as a sequel to the continuous rise in the cost of coal, has brought in its wake the various problems connected with the development of an efficient exhaust end for large turbines working at high vacuum.

Turbine designers have devoted considerable attention to these problems, and the various solutions which have been put forward with a view to reducing the leaving losses to a minimum may be summarized as follows:—

(*a*) *Reduction in speed* (*r.p.m.*).—By designing a turbine for a lower speed (r.p.m.) any desired blade area can be obtained. Owing to the large decrease in speed usually necessary (i.e. from 3 000 r.p.m. to 1 500 r.p.m. or from 1 500 r.p.m. to 1 000 r.p.m.) this solution is very costly, since it increases the cost per kilowatt of turbine capacity. Moreover, it has an adverse effect on the turbine efficiency.

(*b*) *Increasing blade lengths and speeds.*—This can be done by increasing the length of the blades and the mean diameter. As already pointed out, the blade area can be increased by increasing the blade section towards the root, particularly by increasing the width of the blade. The increase which can be obtained in this manner to-day is 50 per cent greater than that which was obtainable in 1912. Larger increases are difficult to obtain unless stresses are increased at the same time. The factor of safety can be maintained by using materials possessing physical properties which are superior to those which were formerly obtainable. There are, however, fundamental objections to introducing special materials, particularly in connection with large turbines. Similar objections arise against the use of higher stresses by reducing the factor of safety, and as a result of recent experience the present tendency is rather towards lower stresses and higher factors of safety.

(*c*) *Double-flow exhaust end.*—By adopting the double-flow exhaust principle the capacity of the machine at a given speed can be practically doubled. However, in order to meet critical-speed requirements, it is necessary to adopt the two-cylinder design. It increases the cost of the turbine and the buildings out of proportion to the gain in economy due to decreased leaving losses.

The A.E.G. turbine shown in Fig. 11 is a notable attempt to solve the problem with a single-cylinder construction. This solution, however, generally entails a sacrifice of efficiency due to excessive exhaust losses. Moreover, a solution such as this is possible only when the stages are exceedingly few in number, when the peripheral speeds are high, and when the blades are short relative to the mean diameter, the ratio h/D being 1/8·5, which gives a much shorter blade length than that which is used for single-flow turbines. This method, therefore, provides a capacity which is considerably less than double that which is possible with a single-flow turbine.

In reviewing the problems connected with the possibilities of increasing the capacity of the exhaust end

of the turbine, it is not possible to avoid being impressed by the importance of obtaining a solution which will meet the following conditions:—

(1) *Factor of safety.*—The increase must be obtained without reducing the factor of safety and without reducing the reliability of the machine in other directions.

(2) *Reasonable cost.*—The increase must be obtained at a reasonable cost, otherwise the increase in economy due to the reduction in leaving losses will not be justified. To meet this condition a single-cylinder construction is eminently desirable.

(3) *Avoidance of special precautions in manufacture.*—The increase should be obtained without having to

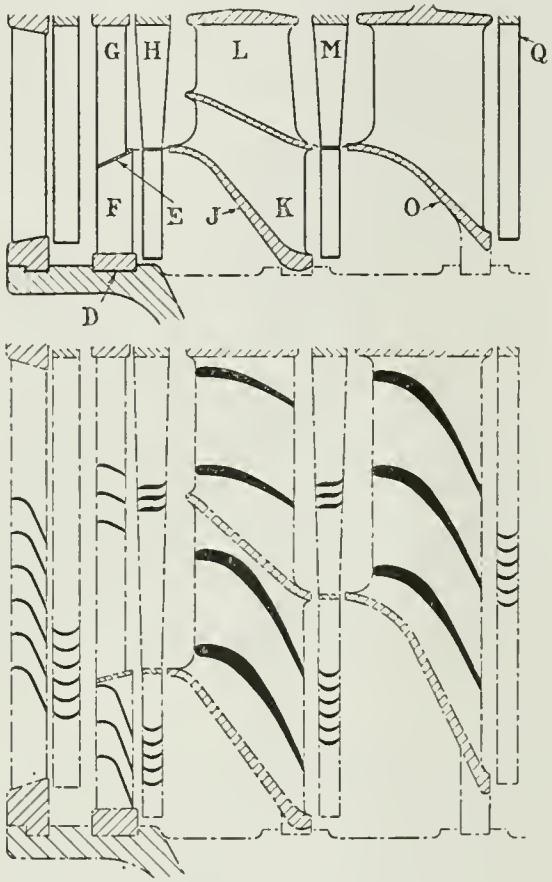


Fig. 30.—Multi-exhaust turbine diagram.

rely upon the skill and care of the workmen and staff in the steel works in producing high-quality material.

(4) *Overall dimensions.*—The increase should be obtained without increasing the overall dimensions, since this would entail increased cost of foundations and buildings.

The author contends that the multi-exhaust construction introduced by the Metropolitan-Vickers Electrical Co. meets the conditions outlined above. The principle of the multi-exhaust turbine is best explained by means of the diagram shown in Fig. 30.

The steam passes through the high-pressure end of the turbine in the usual way until it reaches the special diaphragm D. At this point the steam is divided into two parts by the annular division ring E in the diaphragm. Passing through the outer annulus F, the steam is expanded to condenser pressure, the shape of the diaphragm blades and that of the co-operating row of moving blades at this part being suitably proportioned. In the inner annulus G, the steam passes without appreciable expansion through suitably formed guide blades. Opposite G is the inner portion of the row of moving blades H, shaped in such a way that the stresses due to the centrifugal force are within the limits fixed by the material, the passages left between the blades being such as to pass the maximum quantity of steam with the smallest drop in pressure. The shape most suitable for this purpose is that of a reaction blade with nearly 100 per cent reaction, as shown in the diagram.

The steam then reaches the diaphragm J, which also has two distinct annular parts, the outer part K being suitably shaped to expand the steam to condenser pressure, and the inner part L to pass the steam on without appreciable expansion through suitably-shaped moving blades M to another diaphragm O, where the steam is expanded finally to condenser pressure over the full lengths of the blades Q. Due to the difference in pressure existing in the inner and outer annular passages, a small leakage occurs through the clearance spaces between the guide blades and the corresponding moving blades. This leakage is of quite a minor order, having regard to the large volume of steam dealt with at this part of the turbine.

With this construction it is possible to increase the leaving area of the machine in the ratio of 1 to 1·6 with one additional row of exhaust blades, in the ration of 1 to 2·2 with two additional rows, and in the ratio of 1 to 2·7 with three additional rows. The output of the machine can therefore be increased over a considerable range, and without the use of the two-cylinder construction. Adding to this increase the increase possible by increasing the widths of the blade towards the root, we find that the output of a modern multi-exhaust turbine may be four times as large as that of a single-flow turbine with blades of constant width, the factors of safety being the same in both cases. Thus, if 5 000 kW is the maximum output for a single-flow 3 000-r.p.m. turbine designed for a vacuum of 29 inches and having parallel blades and a given leaving loss, the corresponding output obtainable from a modern multi-exhaust type turbine having the same leaving loss and factor of safety is 20 000 kW.

In the multi-exhaust turbine the steam is divided into two separate flows only when it has become impossible to deal with the whole of it in a single row of blades. This is not the case in the ordinary types of double-flow turbines.

On account of the fact that in a multi-exhaust turbine the steam quantity flowing through the high-pressure stages is much larger than usual, the efficiency of these stages is considerably higher than those of the corresponding stages of previous turbine designs, because the leakage, disc friction and gland losses, etc., are inversely proportional to the output.

With the increased outputs possible with the multi-

exhaust turbine, it is possible with Rateau turbines to adopt full admission, even for the first row of blades, and special nozzle boxes have been designed to this end.

Turbines designed for large outputs at high speed (r.p.m.) are particularly suitable for very high steam pressures, because with high speeds (r.p.m.) a small diameter is permissible at the high-pressure end, and all the parts subject to high pressure can be made strong enough without great expenditure of material. An extra-high-pressure turbine running at a relatively high speed for a given output (r.p.m.) is more reliable than a turbine running at a low speed (r.p.m.) having large diameters at the high-pressure end.

For this reason the turbines of some generating units have been built in two separate portions consisting of high- and low-pressure elements, the former running at a higher speed than the latter. This arrangement, however, increases the cost and complicates the running of the plant.

In the manufacture of the multi-exhaust type of turbine it is possible to derive considerable advantages which are not obtainable with other designs. It is possible to construct different multi-exhaust turbine frames for relative exhaust capacities of 1, 1·6, 2·2, and 2·7 on the same mean diameter and with the same length and type of blades. It is also possible to use the same set of patterns for moulding the turbine cylinders on all four frames.

Thus, for instance, it was possible to build the turbines illustrated in Figs. 13 and 14 on the same set of patterns, a distance piece having an axial length of 2 ft. being necessary to obtain the pattern for the latter frame from that of the former.

(6) Overall Dimensions and Weights of Some Large Turbo Sets.

Table 19 gives particulars of overall dimensions, floor space, exhaust areas and weights of some large turbines. During the period under review, the floor space per 1 000 kW economical capacity has been reduced from roughly 40 sq. ft. to below 20 sq. ft., and the weight per 1 000 kW economical capacity has been reduced from 14 tons to below 7 tons.

Table 20 gives similar particulars for large turbo sets, consisting of turbine and alternator. The floor space per 1 000 kW economical capacity has been reduced from over 60 sq. ft. to below 40 sq. ft., and the weight from over 20 tons to below 15 tons.

Table 21 gives particulars of heights of sets including turbines, alternators, and condensing plant. The total height of the plant is of particular importance, as increased height increases the cost of foundations and of the building. The total height of a turbine of 25 000-kW economical capacity with a condenser of 40 000 sq. ft. can be made less than 35 ft. by bolting the condenser directly to the turbine.

(7) The Law of Similarity Applied to Steam Turbines.

(*a*) *Dimensions, weights, costs.*—A turbine of a capacity of 20 000 kW running at 1 500 r.p.m. can be made with a certain weight (say 100 tons) and at a given cost

TABLE 19.

Dimensions and Weights of Turbines.

Item No.	Reference to Tables 1 and 2	Power Station and Date Installed		Manufacturer	Economical Load			Speed	Overall Dimensions			Floor Space		Exhaust Area			Weights	
					Actual	Vacuum	Economical Load reduced to 1 in. abs.		Length, *L*	Width, *B*	Height, *H*	Area *L* × *B*	Area for (2) 1 000 kW Economical Capacity	Full Exhaust Area	Useful Exhaust Area	Ratio of (15) (12)	Total	Weight for (7) 1 000 kW Economical Capacity
					kW	in. abs.	kW	r.p.m.	ft. in.	ft. in.	ft. in.	sq. ft.	sq. ft.	sq. ft.	sq. ft.		tons	tons
1	2/1	Fisk Street	1912	Parsons	20 000	1·0	20 000	750	51 4	13 0	15 7	670	33·5	252	95·5	0·142	280	14·0
2	5/1	Lots Road	1914	Parsons	15 000	1·0	15 000	1 000	46 4	13 9	12 9	637	42·5	120	120	0·1885	—	—
3	6/1	Philadelphia	1915	G.E.	25 000	1·5	20 400	1 200	31 6	22 0	16 0	694	34·0	168	114	0·164	434	21·3
4	13/1	Interboro'	1918	Westinghouse	40 000	1·0	40 000	1 500	32 6	27 0	18 0	877	21·9	190	128	0·146	—	—
5	15/1	North-West, Chicago	1918	G.E.	25 000	1·0	25 000	1 500	35 4	18 4	18 0	646	25·8	180*	141*	0·223*	—	—
6	23/1	Manchester	1921	Met.-Vick.	25 000	0·9	26 400	1 500	26 0	17 8	14 41	460	17·4	176	128	0·278	173	6·55
7	18/2	Liverpool	1920	Met.-Vick.	12 500	2·5	7 900	3 000	17 2	9 5	9 8	161	20·4	53·3	37·3	0·232	50	6·32

* Approximate.

TABLE 20.

Dimensions and Weights of Turbines and Alternators.

1	2	3		4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
					Turbine			Alternator				Overall Dimensions			Floor Space		Weights	
Item No.	Reference to Tables 1 and 2	Power Station and Date Installed		Manufacturer	Economical Load	Vacuum	Economical Load for 1 in. abs.	Max. Cont. Load	Power Factor	Max. Cont.	Speed	Length, *L*	Width, *B*	Height, *H*	Area, $L \times B$	Area for (7) 1 000 kW Economical Capacity	Total	Weight for (7) 1 000 kW Capacity
					kW	in. abs.	kW	kW		kVA	r.p.m.	ft. in.	ft. in.	ft. in.	sq. ft.	sq. ft.	tons	tons
1	2/1	Fisk Street	1912	Parsons	20 000	1·0	20 000	25 000	1·0	25 000	750	76 2	18 0	15 7	1 370	68·5	450*	22·5*
2	5/1	Lots Road	1914	Parsons	15 000	1·0	15 000	18 000	0·95	18 950	1 000	66 4	13 8	12 9	906	60·4	—	—
3	6/1	Philadelphia	1915	G.E.	25 000	1·5	20 400	35 000	1·0	35 000	1 200	63 0	21 4	15 8½	1 340	65·7	600	29·4
4	7/1	Philadelphia	1915	G.E.	22 500	1·5	18 350	30 000	1·0	30 000	1 500	59 0	20 0	15 0	1 180	64·2	488	26·6
5	11/1	Narragansett	1917	Westinghouse	33 000	2·0	23 300	20 000	0·95	21 000	1 800/1 200	46 9	37 0	19 2	1 730	74·2	535†	23·0†
6	12/1	Detroit	1918	G.E.	35 000	1·0	35 000	45 000	0·9	50 000	1 200	64 9	—	—	—	—	446	12·75
7	13/1	Interboro'	1918	Westinghouse	40 000	1·0	40 000	20 000	1·0	20 000	1 500	52 0	50 0	18 0	2 600	65·0	690†	17·3†
8	15/1	North-West, Chicago	1918	G.E.	25 000	1·0	25 000	35 000	1·0	35 000	1 500	59 6	18 4	14 2	1 091	43·7	—	—
9	18/1	Cologne	1919	A.E.G.	40 000	2·0	28 300	50 000	0·83	60 000	1 000	—	—	—	—	—	475	16·8
10	19/1	Muscle Shoals	1919	Westinghouse	50 000	1·0	50 000	20 000	0·85	23 500	1 800/1 200	53 8	56 2	19 3	3 010	60·2	1 160†	23·2†
11	23/1	Manchester	1921	Met.-Vick.	25 000	0·9	26 400	27 500	0·8	34 400	1 500	49 0	17 8	14 4½	865	32·8	324	12·25
12	18/2	Liverpool	1920	Met.-Vick.	12 500	2·5	7 900	13 750	0·8	17 200	3 000	37 2	9 5	9 8	350	44·2	127	16·1

* Approximate. † Exciter not included.

TABLE 21.

Dimensions of Turbo Sets with Condensers.

1	2	3		4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
					Turbine			Condenser			Turbine Heights				Condenser Heights			Total Height	
Item No.	Reference to Tables 1 and 2	Power Station and Date Installed		Manufacturer	Economical Load	Vacuum	Economical Load for 1 in. abs.	Surface	Diameter of Shell	Length of Tubes	Turbine Centre line above Floor	Bottom Half	Top Half	Turbine Centre-line to Crane Hook	Height of Shell	Condenser Bottom to Extraction Pump Centre	Pump Centre to Basement Level	Actual from Top of Turbine to Basement	For 1 000 kW Economical Capacity (7)
					kW	in. abs.	kW	sq. ft.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft. in.	ft.
1	2/1	Fisk Street	1912	Parsons	20 000	1·0	20 000	39 300	—	15 0	8 0	8 0	7 7	—	14 0½	—	—	—	—
2*	5/1	Lots Road	1914	Parsons	15 000	1·0	15 000	30 000 Total	11 6 Each	14 0	5 6	6 9	6 0	—	14 2	4 0	2 6	33 5	2·23
3	6/1	Philadelphia	1915	G.E.	25 000	1·5	20 400	50 000	18 0	18 0	—	—	—	—	20 0	—	—	—	—
4	7/1	Philadelphia	1915	G.E.	22 500	1·5	18 350	50 000	18 0	18 0	—	—	—	—	20 0	—	—	—	—
5†	13/1	Interboro'	1918	Westinghouse	40 000	1·0	40 000	100 000 Total	—	—	—	—	—	—	—	—	—	—	—
6	15/1	North-West, Chicago	1918	G.E.	25 000	1·0	25 000	35 000	—	18 0	—	—	—	—	—	—	—	—	—
7†	19/1	Muscle Shoals	1919	Westinghouse	50 000	1·0	50 000	100 000 Total	—	18 3¾	—	—	—	—	—	—	—	—	—
8	21/1	Glasgow	1919	Met.-Vick.	15 000	0·9	15 800	26 000	11 0	18 0	3 0	7 0	7 4½	23 0	13 0	3 6	5 6	36 4½	2·3
9	23/1	Manchester	1921	Met.-Vick.	25 000	0·9	26 400	40 000	14 0	—	3 0	7 0	7 4½	—	14 0½	3 4	2 6	34 2½	1·3
10	18/2	Liverpool	1920	Met.-Vick.	12 500	2·5	7 900	23 500	9 9	16 0	2 0	4 0	5 8	—	12 3½	3 0	2 6‡	22 5½	2·85

* Two condensers. † Four condensers. ‡ Pump below basement.

(say £50 000) and with a given efficiency (say 80 per cent). By reducing all the dimensions to one-half, a turbine of 3 000 r.p.m. would be obtained, the rating being reduced to 5 000 kW, the areas being reduced to one-quarter, and the volumes to one-eighth. The weight of the 3 000-r.p.m. turbine would accordingly be only one-eighth of the weight of the larger turbine, and the weight per kilowatt only one-half. Actually, however, it is not practical to obtain such conditions for the following reasons:—

The clearances between the moving and stationary parts would be reduced to one-half, and this would prove to be too small for similar workmanship. Further, present foundry practice does not allow a reduction of the thickness of the castings to one-half. Consequently, the weight of the turbine will be considerably increased, but there is no doubt that it is practical to make the higher-speed turbine with a smaller weight per kW capacity than the larger turbine. Present practice would indicate that the weight of the smaller machine would be about 10 per cent smaller per kW capacity than that of the larger turbine.

On the other hand, the manufacturing costs are about 20 per cent higher per ton on the smaller machine and, consequently, the cost per kilowatt of the smaller machine would be about 10 per cent higher than that of the larger low-speed turbine. This difference is, however, very small, considering that the output must be increased four times to obtain a decrease of 10 per cent in the cost per kilowatt.

(*b*) *Efficiency.*—If all dimensions of the smaller high-speed turbine, including clearances, were one-half of those of the larger machine, then the losses in the high-speed machine would be slightly smaller, due to the decreased disc-friction losses. On the other hand, the blading efficiency of the high-speed turbine would be lower owing to more abrupt changes in the velocity of the steam passing through smaller blades, so that on the whole there would be very little difference between the two cases as regards efficiency. Actually, however, the clearances are slightly larger in proportion on the high-speed machine and, consequently, the efficiency of this machine is reduced. As a rough guide, this difference in efficiency may be taken as being 2 per cent.

If a turbine were designed at the higher speed for double the output, i.e. 10 000 kW, it would be possible to obtain an efficiency about 2 per cent higher than for a 5 000-kW machine, provided that the leaving losses are similar, so that the efficiency would then be about the same as that of the lower-speed machine with an output of 20 000 kW. It is therefore seen that, as far as the efficiency is concerned, a turbine of 10 000-kW capacity running at 3 000 r.p.m. would be equivalent to a turbine of 20 000-kW capacity running at 1 500 r.p.m.

This rough estimate gives us a basis for comparing efficiencies of turbines of different capacities and different speeds. If we multiply the capacity by the speed it will be found that, where the same product is obtained, the turbine efficiency ought to be about the same.

An increase in the capacity-speed factor of 100 per cent should correspond to an efficiency increase of 2 per cent in favour of the turbine having the higher factor, always provided the leaving losses are the same in all cases.

PART V.

GOVERNING OF STEAM TURBINES.

(1) Nozzle versus throttling governing (by-passing at high temperatures).
(2) Design of nozzle boxes.
(3) Steam chests.
(4) Design of high-pressure end (C.S.).

The use of oil relays for the operation of the governor valves is now universal for all large turbines.

In respect of the admission of steam to the turbine, existing methods of governing can be classified under

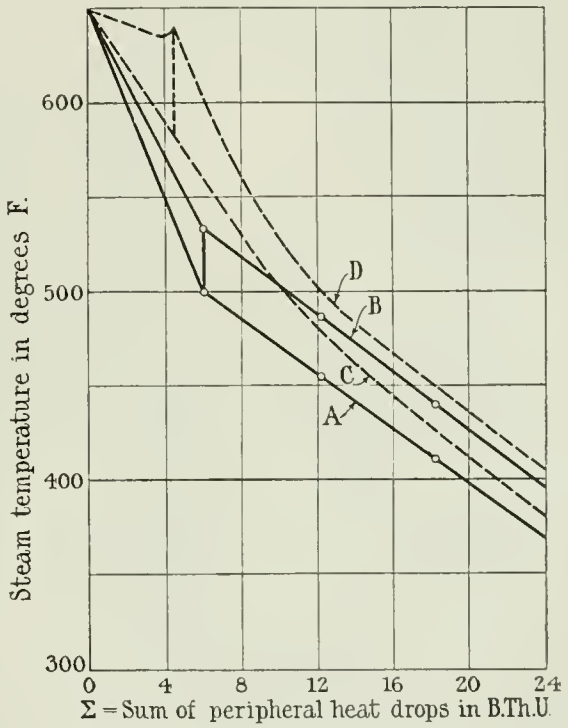


Fig. 31.—Variation of steam temperature in high-pressure end with variation in load.

two headings, viz. throttle governing, and nozzle cut-out governing.

The characteristic feature of throttle governing is that at all loads below the economical load the steam is throttled to the pressure required to carry any particular load, and that loads above the economical are obtained by by-passing steam to an intermediate stage. In some cases two by-pass valves are used, each of which is connected to one of two intermediate stages. Each main throttle valve is usually designed to pass the full quantity of steam required at the loads which it is designed to carry.

The principal disadvantage of throttle compared with nozzle cut-out governing is due, not so much to the fact that at light loads the former is less efficient than the latter, but to the necessity in the case of throttle governing of by-passing high-pressure steam to an intermediate stage in order to obtain loads greater than the economical load. It is clear that intermediate stages to which high-pressure steam is occasionally admitted must be designed to stand the high pressures and temperatures as if they were high-pressure stages.

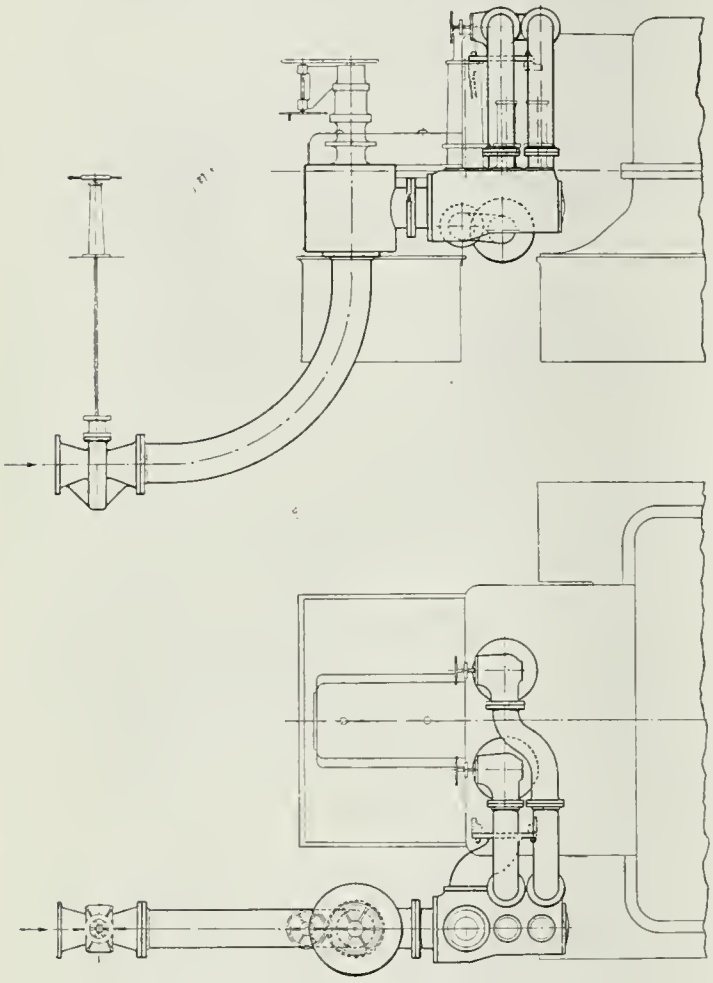
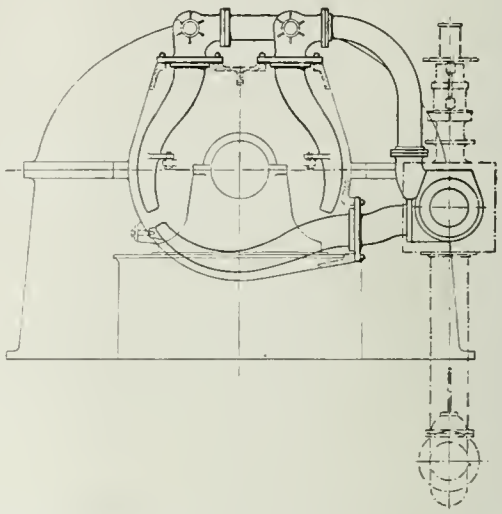


Fig. 32.—Arrangement of nozzle boxes.

The fact that such intermediate stages may be only occasionally subjected to high-pressure conditions does not in any way mitigate the objections to by-passing. The conditions under which these intermediate stages have to operate are in reality much more severe than those to which the initial high-pressure stages are subjected, since the magnitude of the temperature changes is greater in the intermediate stages than that of the temperature changes which occur in the initial stages with either throttle or nozzle cut-out governing. Such changes cause distortion and stresses in both the casing and the rotor.

Furthermore, all parts which may come in contact with steam at high temperatures must be made of cast steel. Cast iron will not withstand high temperatures. It is particularly unsuitable for parts which are subjected to variations in temperature, as it is the repeated heating and cooling which accelerates the growth of cast iron.

The drop in temperature in the initial and in the by-pass stages for normal and maximum load are shown by A and B respectively in Fig. 31. These



conditions are much easier where nozzle governing is employed, since in this case all loads up to the maximum load are obtained not by by-passing the steam, but by admitting it to additional nozzles in front of the first stage.

The variation of temperature with load in the first and second stages when nozzle governing is employed is shown by curves C and D respectively. It will be seen that besides being smaller in magnitude these variations are less sudden than those which take place with throttle governing.

Owing to the practice for steam conditions tending towards higher pressures and temperatures, these facts become of great importance, no matter whether the admission and overload valves are automatically operated or hand-operated.

It is interesting to note that the practice adopted by the General Electric Company during the period under review is exactly opposed to that which would follow from a consideration of the phenomena described above. Up to 1914 this firm carried the development of multiple-nozzle control to almost its furthest limits, employing on some turbines as many as 14 separate nozzle-control valves. Since then, however, their design has developed in an opposite direction.

On their very large turbines the Curtis wheel with two rows of blades was replaced by Rateau stages of relatively small diameters, probably with a view to improving the efficiency of the high-pressure end. This, however, necessitated the abandonment of nozzle-control governing since, owing to the heat drop being too small in the first stage, it becomes necessary to

by-pass the steam to the fifth and, in some cases, seventh stage in order to obtain the necessary overloads.

Most large turbines are required to give loads beyond the economical load automatically. This involves the use of at least two automatic valves and, in the author's opinion, all commercial needs can satisfactorily be met by the use of three automatic valves, with the addition of probably one or two hand valves to be used for emergency conditions.

Fig. 32 shows an arrangement of nozzles and steam valves. The only parts which are subjected to full steam pressure and temperature are the steam chests and the nozzle boxes. The latter are made in the shape of a pipe, as shown in Fig. 32. Suitable slots are provided for the admission of steam to the nozzles, which are bolted to the boxes. The nozzle box itself is bolted at one end to the steam chest or to a pipe connecting the nozzle box to the steam chest. All joints are of a simple circular shape of relatively small dimensions and can be kept steam-tight without any difficulty. The other end of the nozzle box is free to expand independently of the turbine casing, its movement being guided by means of a pin.

The steam chest and the governor gear are shown in Fig. 33. The body of the steam chest consists of two pipes, an inner and an outer pipe. The inner pipe to which steam is supplied from the main stop valve is provided with three valves, which admit the steam to three compartments formed within the steam belt between the inner and the outer pipes. Each of the compartments in turn communicates with a group of nozzles. This design provides a very compact arrangement, and the steel castings are of a simple shape.

The governor gear is arranged in such a manner that the forces are acting in one plane, and the parts subject to wear are kept apart from those which are subjected to high temperatures.

Referring to Fig. 33, it will be seen that the three governor valves are actuated by means of oil pressure under the control of relay mechanism fitted to governor valve No. 1. As the load exceeds that which valve No. 1 is designed to carry when fully opened, the spindle

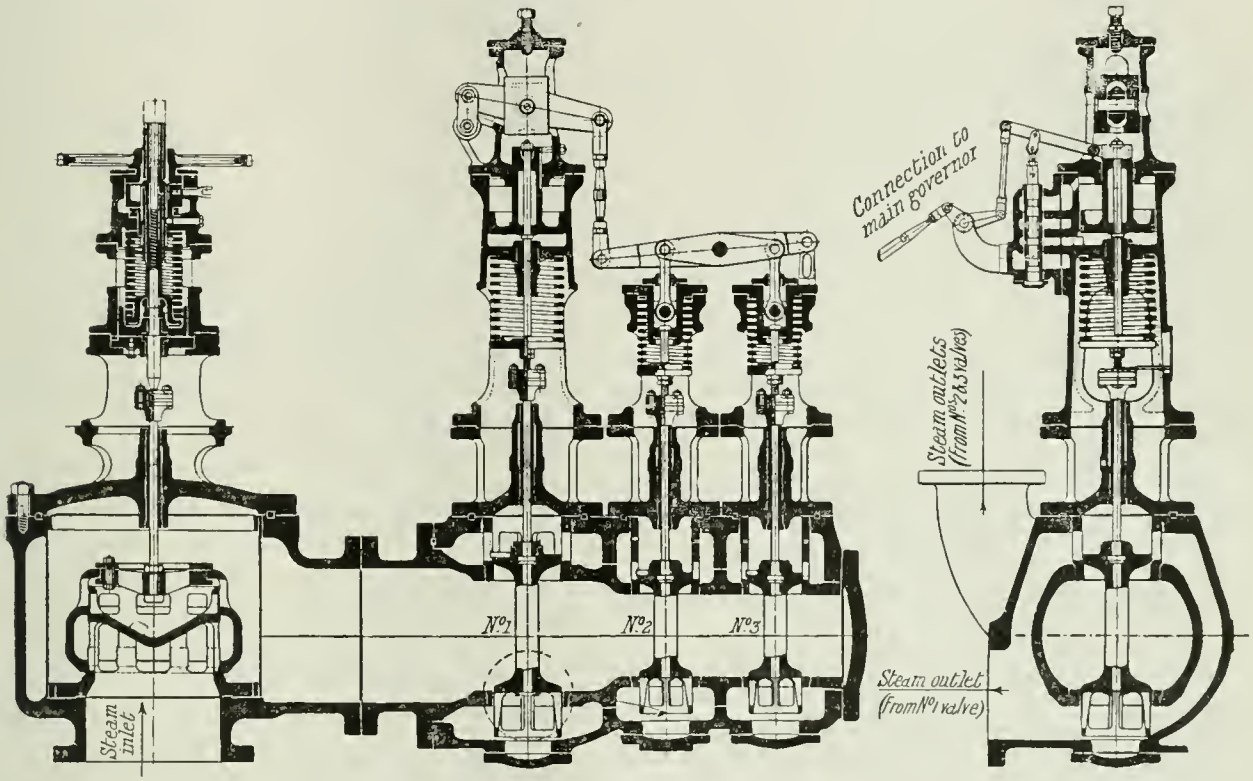


Fig. 33.—Sectional arrangement of steam chest and governor gear.

of this valve continues to move upwards, carrying with it a lever pivoted at one end and attached at the other end by means of a short vertical adjustable link to one end of a second lever, the other end of which is pivoted at a fixed point above governor valve No. 3. This lever moves in the same plane as the lever situated above valve No. 1, and carries with it a floating lever, which is connected at its extremities to governor valves Nos. 2 and 3. The springs which close valves Nos. 2 and 3 are so adjusted that, for an increasing load, valve No. 3 remains closed until valve No. 2 is fully opened, and for a decreasing load valve No. 2 does not begin to close before valve No. 3 is fully closed. In like manner valve No. 1 does not begin to close on a decrease in load until valve No. 2 is fully closed.

The high-pressure end of the turbine casing is made suitable for the reception of the nozzle boxes, and the

shape thus obtained is one of extreme rigidity as far as deflection under the internal pressure is concerned, but at the same time it is sufficiently flexible in a vertical direction to ensure a tight joint between the upper and lower halves of the turbine casing.

PART VI.

SOME RECENT STEAM CONSUMPTION GUARANTEES AND TEST-RESULTS OF LARGE TURBINES.

Correction Factors for Thermodynamic Efficiency.

It has been shown that the thermodynamic efficiency of a turbine depends on the output, speed, and working conditions for which it is designed, and also on the ratio u/c_0 which controls the price of the machine to a considerable extent.

In order to assess the economic value of various turbines, it is necessary to reduce their thermodynamic efficiencies to a common basis by applying appropriate corrections in respect of the following controlling features:

(1) Steam Conditions.

The higher the steam pressure, the more difficult it is to design a turbine for a given efficiency. On the other hand, the efficiency of a given turbine increases with the superheat.

The standard conditions adopted in the author's previous paper for pressure and superheat were 180 lb. per square inch and 150 degrees F., respectively. The range of corrections extended to 200 lb. per square inch and 300 degrees F. These corrections are reproduced in Fig. 34 by curves A and B.

It will be seen that the pressure and superheat corrections are similar in value, but that they operate in opposite directions. For instance, for a pressure of 200 lb. per square inch, and a superheat of 200 degrees F., the efficiency of a given turbine frame would be approximately the same as that which it would possess for a pressure of 150 lb. per square inch (gauge) and a superheat of 150 degrees F.

This relationship between pressure and superheat corrections is reasonably correct for higher pressures, so that for a pressure of 300 lb. per square inch and a superheat of 300 degrees F., approximately the same efficiency would be obtained with a given turbine frame as for a pressure of 200 lb. per square inch and a superheat of 200 degrees F.

From curves A and B in Fig. 34 it will be seen that the general effect on thermodynamic efficiency of variations in the pressure and superheat for which a given turbine frame is designed is approximately as follows:

An increase in pressure of 100 per cent decreases the efficiency by approximately 2 per cent.

An increase in superheat of 100 per cent increases the efficiency by approximately 2 per cent.

These simple rules are in accordance with those obtained in Part II, Section (2).

Having regard to the improvement in working conditions which has taken place during the period under review, and also with a view to facilitating calculations, it is considered desirable to change the standard steam conditions from 180 lb. per square inch and 150 degrees F. superheat to 200 lb. per square inch (gauge) and 200 degrees F. superheat. Revised efficiency correction curves have therefore been prepared on this more up-to-date basis and in accordance with the rule given above. The pressure and superheat corrections are shown in Fig. 34 by curves C and D respectively for pressures from 50 lb. per square inch (gauge) to 500 lb. per square inch (gauge) and for superheats from 50 degrees F. to 400 degrees F.

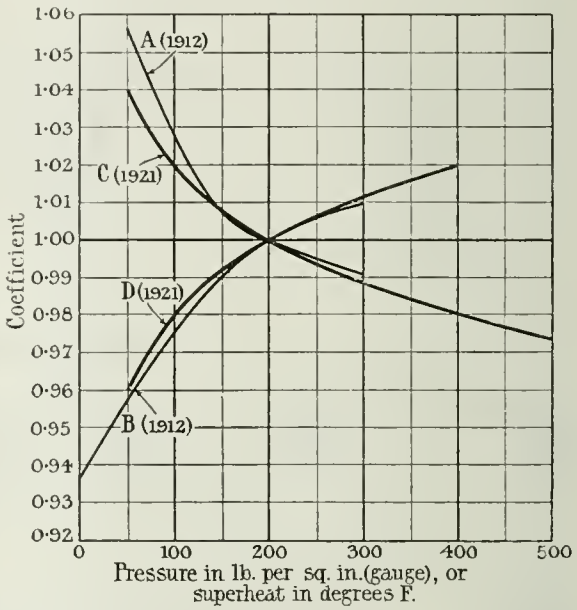


Fig. 34.—Efficiency correction for pressure and superheat.

From Fig. 34 it will be seen that the efficiency correction for a steam pressure of 400 lb. per square inch (gauge) and a superheat of 200 degrees F. is 2 per cent. This means that the efficiency of a given turbine would be 2 per cent lower when designed for 400 lb. per square inch (gauge) and 200 degrees F. than it would be if designed for 200 lb. per square inch (gauge) and 200 degrees F. superheat.

It is frequently useful, for purposes of comparison, to convert the actual efficiency of a turbine when working under given steam conditions for which it has been designed, to the efficiency it would have if it had been designed for standard conditions. Applying the above corrections, it is seen that in order to convert an actual test efficiency obtained with a pressure of 400 lb. per square inch and 200 degrees F. to the efficiency at standard conditions, it is necessary to divide the actual test efficiency by 0·98. This conversion from actual to standard conditions must not be confused with the correction factors which may

have to be applied to correct for variation of actual test conditions from the design or guarantee conditions.

(2) Vacuum.

The standard previously adopted for vacuum was 28 inches (barometer 30 inches). It is now proposed to adopt 29 inches as the standard vacuum condition, the reasons for this change being the same as those which apply to the change in pressure and superheat standards. Both the original and the revised vacuum corrections for efficiency are shown in Fig. 35, from which it will be seen that the revised corrections extend to $29\frac{1}{2}$ inches.

The vacuum corrections for a given turbine depend very largely on the degree to which the particular turbine is rated. At a higher rating the correction for 1 inch vacuum would be higher than it would be were

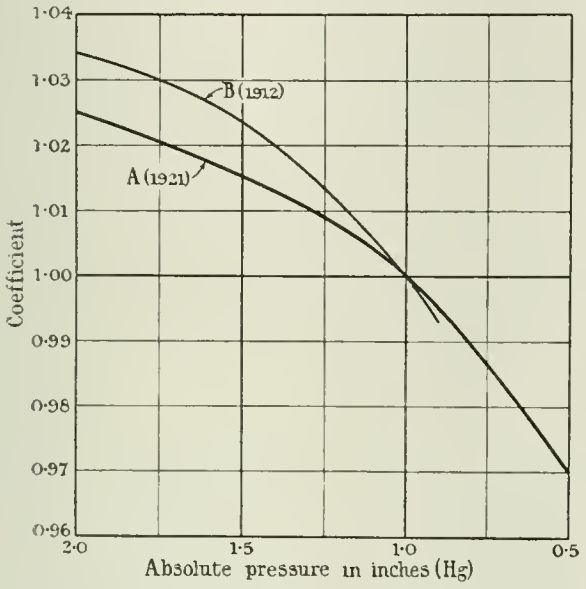


Fig. 35.—Efficiency correction for vacuum.

the turbine lightly rated. For the purpose of the corrections shown in Fig. 35 it has been assumed that the rating of the turbine is the overall economic rating as calculated in Part III. Curve A in Fig. 35 is the curve D from Fig. 25A.

The vacuum correction given in the earlier paper (1912) for a given turbine and given capacity is shown by curve B. It is slightly larger than the new corrections which apply for a given turbine frame rated at capacities which depend on the vacuum.

(3) Output, Speed and Price.

Sections (1) and (2) of this Part enable us, for comparison purposes, to reduce the thermodynamic efficiencies of turbines to a common standard basis in respect of steam and vacuum conditions. In order to obtain a comparison on a wider basis, i.e. on the basis of the economic value of turbines, it is necessary to take into consideration the corrections indicated in Part IV in respect of the output-speed characteristic and price.

(*a*) *Output correction for standard back pressure.*—In the first place it is necessary to correct the output for back pressure in accordance with the relationships stated in Part III, Section (2), where it is shown that the actual capacity under actual vacuum conditions should be divided by the square root of the actual back pressure, the standard back pressure being 1 inch abs. (Hg).

(*b*) *Output-speed factor.*—A standard output-speed product of 30 000 000 kW-r.p.m. has been adopted, this being equivalent to 10 000 kW at 3 000 r.p.m. or 20 000 kW at 1 500 r.p.m.

In accordance with Section (7) of Part IV, the correction for the output-speed product is 2 per cent increase in efficiency for every 100 per cent increase in the output-speed product.

(*c*) *Price.*—Since the efficiency of a turbine can be improved at the expense of increased cost per kilowatt, an allowance in efficiency in respect of such additional cost should be made in making a comparison between two designs or types of turbines on an "economic" basis.

Some of the methods by means of which efficiency can be increased at the expense of extra cost are as follows:

(*a*) By under-rating the turbine.
(*b*) By increasing the number of stages. With single-cylinder units of large size there is not much scope in this direction.
(*c*) By increasing the number of stages and adopting the two-cylinder design. In addition to entailing extra cost, this last method suffers from further disadvantages arising from increased floor space and increased complication of arrangement.

The number of bearings and glands is increased, resulting in additional losses. In addition, there may be a considerable drop in pressure in the pipe connecting the two cylinders. With impulse turbines and usual steam conditions the advantage to be gained from the adoption of a two-cylinder design is very small and would not be justified commercially, having in mind the additional disadvantages stated above. In turbines of very large capacities a two-cylinder design may become commercial if, by the adoption of a double-flow low-pressure end, the leaving losses can be decreased appreciably.

It is estimated that unless by the adoption of a two-cylinder design a saving of at least 2 per cent is effected, the extra cost involved would not usually be justified.

In order to compare the economic value of a two-cylinder design with that of a single-cylinder design on the basis of the thermodynamic efficiencies, the thermodynamic efficiency of the two-cylinder design should therefore be reduced by 2 per cent.

For similar reasons, an additional 2 per cent correction is proposed when the turbine is made with three cylinders.

In order to illustrate the application of the above-mentioned correction factors for reducing a turbine to a standard basis for purposes of comparison, the follow-

TABLE 22.

Steam-Consumption Guarantees and Test-Results.

1	2	3	4	5	6	7	8	9	10	11	12
	Item No.	Reference to Tables 1 and 2	Reference and Date	Manufacturer	Power Station	Year	Output	Speed	Steam Conditions at Stop Valve		
									Pressure	Temperature	Superheat
							kW	r.p.m.	lb./sq. in. (g)	°F.	deg. F.
Guarantees	1*	2/1	*Engineering* (17/10/13)	Parsons	Chicago	1913	20 000	750	200	588	200
	2†‡	4/1	*Electric Journal* (June 1915)	W.E. Mfg. Co.	Interboro' Rapid	1914	25 000	1 500 750	200	508	120
	3*	—	*Engineering* (17/10/13)	Parsons	Melbourne	1914	12 500	1 500	200	600	212
	4*	5/1	*Engineering* (17/10/13 and 18/5/17)	Parsons	Lots Road	1915	15 000	1 000	185	500	118
	5*	6/1	*Mech. Eng.* (9/6/14)	G.E.	Philadelphia	1915	25 000	1 200	215	594·3	200
	6*	7/1	*Mech. Eng.* (9/6/14)	G.E.	Philadelphia	1915	22 500	1 500	215	594·3	200
	7*	10/1	*Electric Journal* (Feb. 1918)	W.E. Mfg. Co.	—	1917	25 000	1 200	220	596	200
	8‡†	13/1	*Electric Journal* (Feb. 1918)	W.E. Mfg. Co.	—	1917	40 000	1 500	205	540	150
	9§	15/2	*Times Eng. Supplement* (Feb. 1920)	G.E.	Regina	1920	5 000	3 600	200	488	100
Test-Results	10†	4/1	*Transactions A.S.M.E.* (1916, vol. 38, p. 667)	W.E. Mfg. Co.	Interboro'	1914	26 740	1 500 750	209·3	500	108·5

* Single shaft, two cylinders. † Two-shaft arrangement.

ing example is worked out on the basis of the published test-results obtained in connection with a 30 000-kW cross-compound steam turbine installed at 74th-street Station, New York (Item 4, Table 1), by the Westinghouse Electric and Manufacturing Co., Ltd.*

Maximum continuous rating ..	30 000 kW
Economical rating	25 000 kW
Speed of H.P. turbine	1 500 r.p.m.
Speed of L.P. turbine	750 r.p.m.

I. *Actual test-results.*

Total steam temperature ..	524·3° F.
Pressure at stop valve	203·2lb. per sq. in. (gauge)
Superheat	135·2 degrees F.
Vacuum	1·239 in. abs.
Actual output	26 153 kW
Actual speed	1 500 r.p.m.
Consumption	11·381 lb. per kWh
Heat drop (adiabatic)	397·8 B.Th.U.'s

Actual overall thermodynamic efficiency

$$\frac{3\,420}{\text{heat drop} \times \text{consumption in B.Th.U.'s}} = 75{\cdot}56 \text{ per cent}$$

II. *Standard conditions.*

1. Steam conditions :	
(*a*) Pressure	200 lb. per sq. in. (gauge)
(*b*) Superheat	200 degrees F.
2. Vacuum	1 in. abs. (Hg)
3. Output, speed and price :	
(*a*) Output correction for standard back pressure ..	1 in. abs. (Hg)
(*b*) Output-speed product ..	30 000 000 kW-r.p.m.
(*c*) Type of design	One-cylinder

III. *Corrections.*

1*a*. Pressure (Fig. 34)	0·9995
1*b*. Superheat (Fig. 34)	0·9887
2. Vacuum (Fig. 35)	1·0085
3*a*. Output back-pressure correction $\sqrt{1{\cdot}239} = 1{\cdot}11$	
3*b*. Actual output-speed product *	

$$1\,500 \times \frac{26\,153}{1{\cdot}11} = 35{\cdot}4 \times 10^6$$

* Report upon Efficiency Tests of a 30 000-kW cross-compound steam turbine by H. G. Stott and W. S. Finlay, *Transactions of the American Society of Mechanical Engineers*, 1916, p. 655.

* For this correction the speed of the high-pressure turbine is used. The lower speed for the low-pressure turbine was adopted with a view to decreasing the leaving losses. This procedure involves an increased cost for the turbine and, from an economic point of view, does not represent an advance in turbine design. The deduction of an additional 1 per cent, which results from the application of the higher speed only for the calculation of the output-speed correction, is therefore fully justified.

TABLE 22—*continued.*

Steam-Consumption Guarantees and Test-Results.

13	14	15	16	17	18	19	20	21	22	23	24	25
			Overall Thermodynamic Efficiency in per cent									
Vacuum at Exhaust	Heat Drop	Steam Consumption	Actual	To obtain Corrected Efficiency divide Actual Efficiency by these Correction Factors					Corrected	Heat in Steam less Heat in Condensate	Actual Heat Consumption	Thermal Efficiency
				Pressure	Superheat	Vacuum	Output and Speed	Design				
in. ab. (Hg)	B.Th.U./lb.	lb./kWh	per cent						per cent	B.Th.U./lb.	B.Th.U./kWh	per cent
1·0	423·2	11·25	71·85	1·000	1·000	1·000	0·980	1·02	71·85	1 272·2	14 312·2	23·90
1·0	403·2	11·27	75·30	1·000	0·985	1·000	1·006	1·02	74·53	1 229·0	13 850·8	24·70
1·25	415·4	12·0	68·62	1·000	1·002	1·009	0·985	1·02	67·60	1 271·5	15 258·0	22·41
1·0	397·8	12·0	71·63	1·002	0·985	1·000	0·980	1·02	72·63	1 226·1	14 713·2	23·24
1·5	408·3	11·9	70·4	0·998	1·000	1·015	0·995	1·02	68·50	1 261·6	15 013·0	22·78
1·5	408·3	11·9	70·4	0·998	1·000	1·015	0·998	1·02	68·29	1 261·6	15 013·0	22·78
1·0	429·7	10·65	74·72	0·997	1·000	1·000	1·000	1·02	73·47	1 274·9	13 577·6	25·20
1·0	412·2	10·75	77·15	0·999	0·992	1·000	1·020	1·02	74·84	1 246·1	13 395·5	25·52
2·0	365·1	13·68	68·47	1·000	0·980	1·025	0·973	1·00	70·07	1 195·9	16 359·9	20·90
1·138	397·3	11·47	75·05	0·997	0·983	1·004	1·006	1·02	74·3	1 220·0	13 993·4	24·43

‡ Three-shaft arrangement. § Single cylinder.

Actual output-speed factor

$$\frac{35{\cdot}4 \times 10^6}{30 \times 10^6} = 1{\cdot}18$$

Correction factor corresponding to an output-speed factor of 1·18 .. .. 1·0036

3*c*. Correction factor for two-cylinder design .. .. 1·02

Corrected overall thermodynamic efficiency

$$= 75{\cdot}56 \frac{1}{0{\cdot}9995 \times 0{\cdot}9887 \times 1{\cdot}0085 \times 1{\cdot}0036 \times 1{\cdot}02}$$

$$= 74{\cdot}06 \text{ per cent}$$

This means that the economic value of this turbine is equivalent to that of a single-cylinder machine of a capacity of 20 000 kW and a speed of 1 500 r.p.m., the efficiency of which when designed for standard conditions would be 74·06 per cent.

In order to illustrate the use of the corrections here outlined, certain published steam-consumption guarantees have been analysed and corrected in the manner proposed, and are tabulated in Table 22.

It is to be regretted that only one test-result on a large turbine, with the necessary details to enable the actual performance to be judged, has been published during the period under review.

PART VII.

OPERATION EXPERIENCE WITH LARGE TURBINES.

With all new developments which necessitate the crossing of the border line of past experience, a certain amount of difficulty must be expected. In connection with large turbines the trend of development towards larger capacities at a given speed necessitated, in the case of single-flow turbines, an increase in the peripheral speed of the last wheels, with a consequent increase in stresses. The difficulties which have been experienced, however, were generally not directly due to this increase in stresses, but to the increased tendency of the various parts to vibrate. These difficulties have become of the greatest importance owing to the great losses which have been incurred as a result of the various accidents which have taken place. It is the author's considered opinion that turbine development has arrived at the most critical stage in the history of the steam turbine, and a review of the difficulties experienced, together with a full and frank discussion between turbine users and manufacturers, of the means whereby these difficulties may be overcome, will be in the interest of all and will facilitate further turbine development generally. The principal difficulties which have been experienced are reported below in detail and a summary is given in Table 23.

TABLE 23.

Turbine Troubles.

1	2	3	4	5	6	7	8	9	10
Item No.	Reference and Date		Customer and Power Station	Manufacturer.	Reference to Tables 1 and 2	Max. Cont. Rating, kW	r.p.m.	Date started up	Date of Accident
1	*Electric Journ.*, p. 34	Jan. 1919	Commonwealth Edison, North-West, Chicago	W.E.	10/1	30 000	1 200	Sept. 1917	Sept. 1917
2	*Power*	12/2/18 26/2/18	American Gas & Electric Co., Windsor Power Station	G.E.	9/1	30 000	1 800	—	Oct. 1917
3	Author	—	New Bedford Gas & Edison Light Co.	G.E.	—	15 000	1 800	—	6/2/18
4	*Power* *Elec. Review*	19/3/19 { 4/3/16 12/4/18	Boston Elevated Railway	G.E.	8/1	35 000	1 500	—	14/2/18
5	*Electric Journ.*, p. 35	Jan. 1919	Brunot's Island Station, Duquesne Light Co.	W.E.	11/1	40 000	H.P. 1 800 L.P. 1 200	Dec. 1917	18/2/18
6	Author	—	Manchester Corp., Stuart Street, Manchester	M.-V.	7/2	6 250	3 000	16/12/15	3/4/18
7	*Elec. Review*	4/10/18	Commonwealth Edison, North-West, Chicago	G.E.	15/1	35 000	1 500	Dec. 1917	31/7/18
8	*Electric Journ.*, p. 36	Jan. 1919	Interborough Rapid T. Co. (New York)	W.E.	13/1	60 000	1 500	18/4/18	Aug. 1918
9	*Elec. Review*	20/10/18	Union Gas & Electric Co., Cincinnati	G.E.	—	25 000	1 800	July 1918	4/9/18
10	*Power*, p. 816	3/12/18	New Orleans Railway & Light Co.	G.E.	—	15 000	1 800	26/5/18	26/9/18
11	Author	—	Borough of Wallasey	M.-V.	—	3 000	3 000	11/1/18	Nov. 1918
12	*Power*, pp. 816, 884	3/12/18 17/12/18	Detroit Edison Co., Connor's Creek Station	G.E.	12/1	45 000	1 200	1918	26/11/18
13	*Electric Journ.*, p. 34	Jan. 1919	Interborough Rapid T. Co. (New York)	W.E.	4/1	30 000	H.P. 1 500 L.P. 750	Dec. 1914	—
14	*Times Eng. Supplement*	Feb. 1920	City of Regina, Canada	G.E.	15/2	5 000	3 600	July 1919	8/1/20
15	Detroit Edison Synchroscope	Sept. 1920	Detroit Edison Co., Connor's Creek Station	G.E.	1/1	20 000	1 200	1916	15/7/20
16	Detroit Edison Synchroscope	Sept. 1920	Detroit Edison Co., Connor's Creek Station	G.E.	12/1	45 000	1 200	1918	16/7/20
17	Author	—	South Metropolitan	M.-V.	—	5 000	3 000	30/6/20	29/9/20

TABLE 23—*continued.*

Turbine Troubles.

11	12	13
Extent of Damage	Cause of Accident and Special Features	Remedy
Labyrinth packing on low-pressure element failed	Buckling of cylinder caused by rigid piping between the two surface condensers which were rigidly bolted to the turbine. Expansion of the turbine cylinder was thus prevented	Flexible piping between condensers installed.
Gland seizing. 11 blades falling off last wheel. Bearing cover broken. Shrouding off one side of last 10 rows	May have been caused by gland seizure, but it is possible that blade vibration was the primary cause. During vibration tips of blades were rubbing against cylinder	
Wrecked through bursting of last disc	Fracture starting at the edge of a balancing hole	
No. 18 diaphragm deflected causing rubbing against disc. Diaphragm welded to disc, causing diaphragm to revolve until the two halves were thrown off, bursting turbine cylinder and wrecking turbine	Diaphragms made in cast iron were too weak. Sudden overload, opening the overload by-pass valve, resulting in pressures which caused excessive deflection of diaphragms	Machine replaced and started in spring of 1920. Diaphragms made in cast steel
Main bearing at coupling end of H.P. turbine burned out. Rather heavy blade rubs throughout the machine	Apparently due to interruption of the oil service to this bearing	
Wrecked through bursting of first disc	Possibility of vibration on four nodal diameters causing fracture to start from local weakness in faulty material. Duplicate disc running in duplicate machine installed in the same station was running satisfactorily for 4 years. Accident occurred on a replace disc which had been running for 10 days only. Previous disc was replaced owing to crack starting from keyway after 2¼ years' running	Disc was stiffened to avoid critical vibration and special care taken to ensure uniform material
19th wheel burst, wrecking complete machine. Previous trouble due to vibration and shrouding coming off	Crack from balance hole of disc close to rim. Factor of safety referred to ultimate strength said to be 4. Balance holes were roughly machined and had sharp corners	Machine was completely replaced. Stiffer discs but higher stresses at hub. Smoother balance holes and corners well rounded off
Some intermittent vibration on first L.P. machine	Caused by lack of sufficient clearance on one of the spindle rings resulting in distortion of spindle when changes in temperature occurred	Clearance between rings increased
20 blades broken clean and sharp off last (17th) wheel (apparently fatigue fracture) and most other blades burned and crushed	Rubbing of disc against diaphragm which is reported to be due to diaphragm distorting but the fatigue fracture on the blades indicates the vibration of the disc and blades as the primary cause	Turbine was run without last row of blades
Failure of last wheel causing wrecking of low-pressure cylinder	Crack between two balance holes. Peripheral speed about 700 ft. per sec. Design of turbine comparatively old	
Distortion of last diaphragm causing rubbing between diaphragm and disc	Sudden change in temperature of diaphragm as a result of changing over from condensing to non-condensing	Outside clearance of diaphragm increased to take care of the expansion of the diaphragm
Serious vibration caused by 4 broken buckets. Vibration on governor put set out of service. About 100 condenser tubes were cut, causing flooding of condenser. Turbine shaft was bent	Vibration of 34-in. blades on 21st wheel (last) causing fatigue fracture near root of blade	Blades were removed and turbine was run without last row until 1920, when set was reconstructed and new blades 28-in. long were fitted
Labyrinth packing on H.P. element failed 3 times, requiring renewal of some parts	Excessive lost motion in the thrust bearing and heavy distortional stresses caused by rigid bracing of the steam pipe near the turbine	
Complete wreck through bursting of No. 3 disc	5-stage turbine consisting of one velocity wheel of 35·5-in. mean diameter and 4 Rateau wheels of approx. 51-in. mean diameter. Fatigue crack developed from balance hole in 3rd wheel, causing fracture	Discs replaced by stiffer ones on similar machines
Severe vibration. All blades stripped off 7th wheel damaging diaphragm. Examination showed radial crack in 5th wheel	Wheels were made of cast steel, and cracks have developed in other discs of similar machines	Cast-steel discs, replaced by forged steel discs
Cracked H.P. cylinder	Portion of H.P. cylinder was made in semi-steel owing to war conditions. High temperatures caused growth and cracks	Turbine reconstructed with cast-steel end
Wrecked through bursting of first disc	Possibility of vibration on 4 nodal diameters causing fracture to start from local weakness in material. Duplicate disc was running in a duplicate turbine in the same station for 3 years	Disc was stiffened to avoid critical vibration

(1) Distortion of Turbine Rotor and Stator.

Considerable difficulties have been experienced due to distortion of the rotor and stator causing excessive rubbing and consequent heating up of rotor and stator parts. Unless these large turbines are carefully warmed up so as to ensure a uniform temperature all round the cylinder and rotor, distortion is likely to cause rubbing between the turbine blades and the rotor or stator, and in the balance piston in the case of reaction turbines, or between the diaphragm glands and the main glands and the turbine shaft in the case of impulse turbines. If the rubbing takes place in the rotor, due to a distortion of the shaft, the rubbing will be local as far as the shaft is concerned, causing further distortion and increased rubbing. Unless the machine is shut down at once this rubbing will usually result in a bent shaft or rotor. The uniform heating-up of large low-speed turbines is much more difficult than that of small high-speed turbines, and greater attention will have to be given to the method of starting large turbines than it is customary to apply to the starting of smaller sets. To facilitate uniform heating up, the rotor should, during the warming-up operation, be kept turning round at a low speed—say 10 per cent of the normal speed.

The difficulties can be minimized considerably by arranging the parts likely to rub independent of the shaft itself, so that the heat will take some time before reaching the shaft and, when it does so, will be more uniformly distributed all round the shaft. Thin sleeves fitting on the shaft are beneficial, but to be effective a space should be arranged or the sleeve should be of such thickness that it will distribute the heat uniformly all round the shaft.

Another solution has been introduced by the General Electric Company, who arrange the dummy strips on the rotor, the strips being thin and relatively high. In case of rubbing, the heat will be generated on the tips of the strips, and as the cooling surface of the strips is relatively large, the heat will be absorbed by the surrounding steam before it is transmitted to the shaft through the thin strips. It is reported that these strips work quite satisfactorily, but they have the disadvantage that they cannot be replaced without taking the rotor discs off the shaft.

Several cases of trouble have occurred, particularly in connection with reaction turbines, which were directly due to the distortion of the turbine casing caused by the stresses resulting from the expansion and contraction of the steam piping supplying the unit. It is important that the steam piping should be made as flexible as possible to prevent excessive stresses in the steam turbine.

Some difficulties have been experienced from the relatively weak support provided for the turbine bearing at the exhaust end. In turbines of the type shown in Fig. 10 this support was strengthened considerably to overcome such difficulties. Prior to strengthening the support it was found that, when the turbine was put under vacuum, the bearing support dropped considerably, thus throwing the rotor out of line.

(2) Deflection of Diaphragms.

The deflection of diaphragms may be due to two different causes:

(*a*) The deflection may be due to the difference in pressure in front of and behind the diaphragm. In this case the deflection increases with the load on the machine, and it may become larger than the axial clearance arranged between the diaphragm and the turbine wheel, causing violent rubbing between these two parts. The complete wrecking of the 35 000-kW turbine at Boston, U.S.A., was due to this cause. The accident took place when, due to a breakdown of another machine on the system, the large unit was called upon to supply the additional load, to which the governor promptly responded by causing the opening of the by-pass valve. The rubbing between the diaphragm and the disc was so intense that it caused the welding of the diaphragm to the disc. The diaphragm consequently started revolving with the rotor until the two halves were thrown off, bursting the turbine casing.

For a given thickness and pressure difference the deflection of a plate diaphragm varies as the fourth power of the diameter. If the diameter is increased it is usual to increase the steam velocity in proportion. The pressure difference will accordingly increase as the square of the diameter, so that the deflection of the diaphragm for a given thickness will increase as the sixth power of the diameter. To maintain the deflection at a given figure it would therefore be necessary to increase the thickness of the diaphragm as the square of the diameter. The actual deflection of a diaphragm composed of two portions, viz. the stationary blade portion and the remainder of the diaphragm, is the resultant of the deflection of each portion. It is very difficult to determine this resultant deflection by means of calculation, especially as the diaphragm consists of two halves. It is essential, therefore, that the diaphragms should be tested for deflection before they are actually used in the turbine, so as to ascertain whether or not the deflection is well within the limits given by the clearance between the diaphragm and the rotor. It is clear, however, that there are considerable difficulties with diaphragms having large diameters and long blades, due to excessive deflection, and past experience has shown that the diaphragms which have been used were in many cases much too weak.

(*b*) The diaphragm usually fits into a groove in the turbine cylinder with a given radial clearance. At no load, or when the turbine is running non-condensing, the temperature of the steam flowing through the last rows of blades is considerably higher than when the turbine is running under load. If the load is decreased suddenly, or if the turbine changes over to atmosphere, the last diaphragms will heat up much more quickly than the more massive exhaust casing. The diaphragms will consequently expand at a greater rate than the casing and, if the clearances are not sufficient, the diaphragms may distort in an axial direction under the pressure exerted by the casing in a radial direction. This will cause rubbing between diaphragm and rotor, with disastrous consequences unless the turbine is immediately shut down.

It is obvious that the difficulties in this connection are more acute with diaphragms having large diameters and long blades than with diaphragms of moderate size and blades of moderate length.

(3) Cracking and Breaking of Turbine Discs.

The difficulty with turbine discs may be due to various causes:

(*a*) Excessive stresses due to centrifugal forces.
(*b*) Vibration of turbine discs.
(*c*) Excessive stresses due to uneven distribution of temperature.
(*d*) Faulty material.
(*e*) Faulty design and manufacture.

Considerable difficulties have been experienced during the last 8 years due to the cracking of turbine discs, as is evident from the list of breakdowns given in Table 23. In many cases the cracking of the disc results in increased vibration of the machine, and it has been possible to avoid serious accidents by inspecting the machine carefully as soon as increased vibration became evident, and removing the faulty disc before an actual burst occurred. In other cases, however, the turbine disc actually burst, wrecking the turbine, damaging adjoining machinery and, in some cases, causing serious loss of life. With the exception of those accidents which were unquestionably due to the turbine running away, the failure of the disc was primarily due to fatigue fracture which generally takes considerable time to develop. This fatigue fracture is caused by the vibration of the turbine disc giving rise to considerable vibratory stresses being superimposed on the static stresses due to centrifugal force and uneven distribution of temperature. With reasonably sound material, excessive static stresses would simply cause an elongation of the material without causing a fracture. High local static stresses, however, such as those which occur at balance holes, keyways with sharp corners, sharp edges or rough machining work in any part of the disc, or owing to faults in the material, are contributory causes, and such local stressing of the material should be avoided as much as possible. The additional vibratory stresses result in the local stresses exceeding the elastic limit, and this will ultimately result in a fatigue fracture.

It is very important, therefore, that the turbine discs should be designed in such a way that synchronous vibration due to small periodic disturbing forces of any kind is not possible. There is no difficulty in this connection if the discs are running at comparatively low peripheral speeds, but, if the mean peripheral speed exceeds 650 ft. per second, the discs have to be made much heavier than would be necessary from the consideration of the centrifugal stresses only. It is obvious that discs of the type used at the exhaust end shown in Figs. 13 and 14 are much preferable from the point of view of vibration than those used in the turbine shown in Fig. 10, which have been proved to be too weak.

A turbine disc is liable to vibrate in various ways. The vibration may be of the umbrella-shape type, or it may be of the segmental type with two or more nodal diameters. The former vibration is likely to be set up by some axial disturbance of the shaft such as may result from an inaccuracy of the thrust bearing or flexible coupling. The latter type of vibration may be caused by the uneven distribution of the admission of the steam over the periphery of the turbine wheel, such as may occur with partial admission, or even with small inaccuracies in the stationary blades, such as usually exist near the horizontal joint of the diaphragm. In such a case the nodal diameters rotate at the same speed as the disc, but in a direction opposed to the direction of rotation of the disc. They are therefore stationary in space, and the disc assumes a deformed shape stationary in space. There is evidence, however, that dangerous vibrations also occur in cases where the nodal diameters do not rotate relatively to the disc, or only at a speed lower than the running speed. Such vibrations may be set up by a slight vibration of the machine resulting from a badly balanced rotor.

These various types of vibrations may exist at the same time, and the frequency depends not only on the shape of the disc and the physical properties of the material, but also on the internal forging stresses and the distribution of temperature in the disc. These factors greatly complicate the problem of predetermining the natural frequency of vibration by calculation, and a good deal of research work will be necessary to clear up the situation.* In these circumstances the careful designer will prefer to keep well within safe limits, for the present at any rate, rather than try to find out by trial and error the exact thickness and shape of disc which will just be strong enough to avoid danger, particularly as these experiments can only be carried out satisfactorily on the complete machine under actual working conditions.

There have been some difficulties with 5 000-kW turbines of an old type made by the author's firm, due to cracking of the first disc where partial admission combined with nozzle cut-out governing was used (see item 6 of Table 23). In order to increase the capacity of turbines running at 3 000 r.p.m. the mean blade speed was increased to 710 ft. per second for all discs. Whilst the discs were quite strong enough, as far as centrifugal stresses were concerned, under the vibrations set up in the first disc, fatigue fracture developed in this disc on some of the turbines. The difficulty on these machines was overcome by the use of a much stiffer disc in the same casing, sufficient space being available to accommodate a disc of the required rigidity. As the increased capacity can be obtained more safely, and with standard material, by the use of the multi-exhaust design, the manufacture of this turbine with high peripheral speeds was abandoned some time before the real difficulty became acute.

The General Electric Company of America have experienced considerable difficulties in connection with the last discs on some of their large turbines (see items 3, 7, 10 of Table 23). The mean blade speed of these discs is 787 ft. per second, the tip speeds being nearly 1 000 ft. per second. This speed cannot be reduced

* The only treatise which is available on this subject is that of Professor Stodola, "Über die Schwingungen von Dampfturbinen Laufrädern," published in *Schweizerische Bauzeitung*, 1914, vol. 63, pp. 251, 271, and 919.

without a corresponding decrease in exhaust capacity and, therefore, in the economical rating of the turbine. As the blade is of considerable length and weight, an increase in the thickness of the disc in the outer portion only, as has been proposed, will cause increased stresses at the hub where the stress conditions are considerably complicated due to the existence of the keyway. The proposed provision of balancing holes closer to the hub of the wheel does not, in the author's opinion, improve the stress conditions. It is difficult to see how the difficulties can be overcome without considerably strengthening the disc near the hub, involving an increase in the length of the machine or a decrease in the number of stages, resulting in a decrease in the efficiency of the machine. On the other hand, if it was found necessary to cut down the diameter of the last stages, the economical rating would be reduced in proportion to the leaving area. This course has actually been adopted in some of the frames.

Owing to the lack of satisfactory proposals to overcome the difficulties which have been experienced, due directly or indirectly to the use of high peripheral speeds, the tendency towards the employment of such high peripheral speeds has received a definite check, and a more conservative policy at the present time is more likely to produce satisfactory results.

In connection with some turbine accidents which have occurred, it has been suggested at various times that the difficulties were primarily due to faulty material. There is no doubt that material deteriorated considerably during the war, and additional precautions have become necessary in examining the material; but the conclusion now is that the difficulties were not primarily due to faulty material; that is to say, if vibration had not existed, the fault in the material would not have resulted in the fracture of the disc. It is proved, however, that in some instances the faulty material did accelerate the fracture, as discs, the design of which is identical to that of those which have given trouble, have been running for a considerably longer time under the same conditions without distress.

It has at times been suggested that testing the discs at speeds considerably higher than the normal would go a long way towards preventing failure of turbine discs. While testing the discs in such a way may be a desirable precaution where vibration under working conditions does not exist, it is evident that the disc may easily pass an over-speed test of short duration, even if it is faulty; moreover, it is quite probable that the test itself may cause further local weakening which would not be detected, and which would accelerate the failure of the disc when subject to vibration under normal running conditions.

It is therefore essential that the rigidity of the discs should be such as to preclude their vibration. A good indication of correct rigidity can be obtained by supporting one end of the stationary rotor on a part of another machine which can be rotated at various speeds up to the maximum running speed for which the supported rotor is designed. Such a test would provide a desirable check on the difficult calculations relating to possible vibration. If any parts of the stationary rotor vibrate in synchronism with the running machine, there is a possibility of similar vibration occurring when the rotor is run. It is true that the natural frequencies of the parts are increased when running, but, due to uneven distribution of temperature possible in turbine discs, the natural frequency of vibration may also be lowered.

In impulse turbines the disc is heated or cooled by the steam leaking past the diaphragm glands, and it is thus kept at a fairly uniform temperature except for the lag occurring in the heating of the more massive hub as compared with the time required to heat the relatively thinner rim. This lag may be considerable with large discs, but will be much smaller with discs at the exhaust end such as are shown in Figs. 13 and 14. This point is of considerable importance for the low-pressure discs, where the largest temperature-changes occur, because at very light loads the steam at the exhaust end may become superheated owing to the windage losses due to the long low-pressure blades, and also because when changing over to atmosphere the temperature may suddenly be raised from 100° F. to considerably over 200° F.

In some types of reaction turbines discs are used for the low-pressure end. The spaces between such discs are filled with stagnant steam, and it is evident that when changes of load occur the difference between the temperatures at the rim and the hub will be considerably larger than in the case of impulse turbines. The danger due to warping or vibration of the discs is thereby increased, unless the spaces between the discs are ventilated by steam at approximately the same temperature as the steam flowing through the blades.

(4) Breaking of Moving Blades.

This may be due to four causes:

(*a*) High stresses due to centrifugal forces, and bending stresses due to the action of the steam.
(*b*) Vibration.
(*c*) Faulty material.
(*d*) Faulty design and manufacture.

There have been difficulties due to blades being stressed too highly, and in this connection the blades which are fixed to the turbine discs by means of a T-root have been particularly vulnerable, owing to the very high local stresses caused by the sharp corners which are associated with this type of fastening. There have also been difficulties due to faulty material, design and manufacture; but the principal difficulties here are again due to the posssibility of vibration. This vibration may be set up by synchronous vibration of the turbine disc, or by excessive vibration of the machine due to unsatisfactory dynamic balance of the rotor, or to unsatisfactory working of the thrust bearing or coupling, or finally it may be due to a synchronous vibration of the blades themselves. The cure of the disease depends on its cause. If the cause originates from synchronous vibration of the blade, it is necessary to alter the shape of the blade in such a way that its periodicity of vibration will be altered, or to obtain a more rigid blade element by staying intermediate points of the blade. The lashing or bracing strips used in

connection with light reaction blades are well known. With larger and heavier blades, such as are used on units of large capacity, these precautions are not entirely satisfactory, since more robust designs are necessary.

(5) Corrosion and Erosion of Turbine Blades.

In all large turbines the last rows of blades are made of steel containing more or less nickel, generally not more than 5 per cent. If the feed water is kept reasonably free from corrosive matter, and particularly free from CO_2 and oxygen, and, if the turbine is kept free from moisture when standing, corrosion can be practically eliminated.

To prevent the absorption of CO_2 and oxygen it is desirable that the feed water should not be exposed to atmosphere at any part of its cycle. To ensure the turbine being in dry condition when standing, the air pump should be kept running for some time after shutting down, and the steam space between the closed stop valve and the closed emergency or governor valves should be open to atmosphere to lead away any steam which may leak past the stop valve.

It has been found beneficial to inject a small quantity of kerosene into the steam entering the turbine. This kerosene forms a coating on the turbine blades which affords protection against corrosion.

Erosion occurs where the steam contains a considerable amount of condensed water. It can be diminished considerably by draining the water from every stage in such a manner that the water does not come into contact with the moving blades. The annular slot in the casing, such as is used for bleeding the turbine for feed-water heating (see Figs. 13 and 14), is beneficial in this respect.

It is also essential to prevent the accumulation of water in pockets and bends in the steam piping, valves and turbine casing. Steam spaces which are liable to be under vacuum at light loads should be drained into the condenser, or preferably into a heater working under vacuum. The use of automatic steam traps, whilst satisfactory when draining steam which is always at a pressure above atmosphere, has not proved entirely satisfactory when draining steam spaces after the governor valves, mainly because their operation cannot be observed.

(6) Lubrication and Bearings.

During the period under review, bearing speeds have increased from 60 ft. per second to over 120 ft. per second, and, at the same time, bearing pressures have increased from 60 lb. per square inch to over 120 lb. per square inch. In spite of this rapid development, there has been relatively little trouble due to the failure of the bearings themselves, but there are some cases where, due to the failure of the oil pumps, the bearings heated up and ran out. For large steam turbines it is essential that the lubricating system be made specially safe, and it is desirable to have a reliable stand-by pump which will start automatically in case of the failure of the main oil supply. This is now recognized to be standard practice.

Sight drains or sight feeds for the oil supplied to each bearing are also most desirable. Electrical alarms should be installed for giving warning of excessive bearing or bearing-oil temperatures. It is also desirable to have an indicating alarm device for giving warning of wear of the thrust bearing, which is now usually made on the Michell principle.

The temperature of the oil should not exceed 160° F. in any part of the system.

Water-cooled bearings have been used extensively by some manufacturers, but considerable trouble has been experienced due to water leakage. There is no necessity for water cooling, and this additional complication should be abolished even with large high-speed bearings.

It is essential that the oil be kept in perfect operating condition; this can readily be done by the partial filtration system in which a small quantity of oil is drained from the system and passed through a filter, from which it will be returned to the operating tank either direct or via a storage tank. Provision should be made for effectively draining water which may accumulate in the main oil tank.

(7) Governing.

In large turbines it is desirable to provide three types of valves—a stop valve, a separate emergency valve, and the governor valves. The stop valve should be of the usual standard type which is installed with the main steam piping. The emergency valve should be connected to the emergency governor, but so arranged that it can be hand-operated to act as a stop valve in case of emergency. The governor valves are operated from the governor by means of an oil relay; the oil supplied to the relay being preferably under the control of the emergency governor so that the governor valves will close whenever the emergency governor acts.

To ensure that the emergency governor is kept in good condition, it should be tested whenever the turbine is shut down, by actually running the turbine to the over-speed required to trip the governor.

(8) Steam Leakage.

Steam leakage from the main turbine glands is a considerable nuisance for several reasons. Leakage steam is liable to find its way into the pedestals, where it condenses, heating up the oil and mixing with it. Heat and water are very detrimental to the lubricating qualities of oil, and their access to oil should be prevented by all means. In the Metropolitan-Vickers turbine the possibility of steam leakage is entirely eliminated by the use of water-sealed glands on both the high- and low-pressure ends of the shaft.

Steam leakage into the engine room is very undesirable, as the steam condenses in the building, causing excessive corrosion and dirt.

It is interesting to note that in view of the difficulties experienced in the United States, the General Electric Company have now adopted a water-sealed gland for the low-pressure end, thus partially adopting the standard practice of the Westinghouse Electric Manufacturing Co., Ltd.

Steam leakage from the joints of the turbine should also be eliminated by using joints of simple shape.

In view of the increased tendency towards high pressures and temperatures, this point deserves special attention.

(9) Operation.

The experiences outlined above in connection with large turbines emphasize the necessity of exercising greater care in their operation than it has been customary to devote in the case of smaller machines. The success of large turbines depends to such a great extent on their proper treatment during operation that it may not be out of place to enumerate here the principal points upon which great attention should be bestowed:

(*a*) *Starting up.*—The principal object in starting up must be to obtain temperature conditions as close as possible to those existing when the turbine is running under load, i.e. the high-pressure end must be heated up, whilst the exhaust end should be kept as cool as possible during the starting-up operation. The warming-up of the exhaust end should not be started until a considerable vacuum has been obtained—say 20 inches (Hg). Warming-up will be carried out to some extent by steam-sealing the glands, which should be designed in such a way that the quantity of steam necessary for sealing purposes should not be large, even when the glands are designed for water-sealing. When a vacuum of 25 inches has been reached, the turbine should be started by passing a puff of steam through the governor valve. The turbine should be kept running at about 10 per cent of the normal speed for about 15 minutes, after which the speed may be increased to full speed at the rate of not more than 10 per cent of the full-load speed in one minute. When the turbine is brought to full speed, the low-pressure end will be hotter than when the turbine is running under load and, in order to effect a gradual stabilization in temperature, the load on the turbine should be increased slowly at the rate of not more than 10 per cent of full load in one minute.

The above particulars apply to large 1 500-r.p.m. turbines. For 3 000-r.p.m. machines, the time allowed for warming-up can be considerably reduced. It may be of interest to repeat here the result of inquiries made by the Prime Movers Committee of the National Electric Light Association as reported at the 43rd Convention at Pasadena, California, 18th–22nd May, 1920 (Report of Committee on Prime Movers, page 5 of advance copy, published by National Electric Light Association).

"Starting of large turbines."

"One of the most important points in connection with the operation of large turbines of 20 000-kW capacity and over is the necessity for extreme care in the starting and loading of units, especially when the machine has been idle for a period long enough to lower materially the temperature at which the various parts are normally operated. With the introduction of steam to a cold unit there is at once set up a difference in temperature between the various elements, which, unless a careful procedure is followed, may result in unequal expansions, affecting the shafts, blading, diaphragms, packing or casing joints. In some cases on record, sudden and unequal expansion has caused deformation which has resulted in permanent distortions to casings and bearings, materially affecting the subsequent operation of the machine.

"A survey of the operating methods in use in a number of companies indicates that there is considerable variance in the practice which is followed in starting large turbines. The time required to heat up a cold unit to the point where full load can be carried will, naturally, vary with the type, size, and class of turbine. No hard-and-fast rule of procedure can be definitely laid down, except that in all cases no large unit can be safely loaded which has not been given sufficient time to allow all parts of the unit to reach evenly and gradually the individual normal operating temperatures corresponding to load conditions.

"Data secured from operating companies show that the procedure generally followed consists in establishing a partial vacuum of from 15 to 20 inches prior to starting up the unit, and maintaining this during the period of warming up.

"In warming up the turbine and bringing it to speed, the first operation of introducing steam should admit a sufficient volume to start the rotor turning immediately. Steam admission should then be so regulated that the unit will continue turning at a speed not exceeding 10 per cent of the normal operating speed until the warming-up process is completed. Additional steam should then be admitted gradually to the turbine in order to bring it up to full speed.

"While the time taken for this operation varies among different operating companies, the safe minimum time allowed, based on best operating experience, is given as 20 minutes for units of 20 000-kW capacity, and 30 minutes for units of 30 000-kW capacity.

"In applying load to the unit, the rule observed for safe operation by a number of operating companies is to increase the load on the unit at a rate not exceeding 1 000 kW per minute for units of the above size. While there is considerable variation from the procedure noted above, this is due in many cases to certain local requirements, or to special plant conditions which govern each particular case. It will be noted, therefore, that in following the above practice, the minimum time for heating up and loading large units will vary from 40 minutes for a 20 000-kW turbine to 60 minutes for a 30 000-kW unit, or a rate of 2 minutes for each 1 000-kW turbine capacity.

"These figures do not refer to machines which have not been idle long enough to cool off perceptibly, in which case a shorter length of time can be taken, depending on the individual circumstances."

(*b*) *Vibration when starting.*—If vibration occurs during starting up, reduce the speed at once until vibration disappears, and keep running at the reduced speed for some time, after which the speed may again be increased.

(*c*) *Vibration when running.*—Keep the turbine and the generator in good running balance; if vibration increases for no apparent reason, shut the turbine down and examine the rotors.

(*d*) *Lubrication.*—Keep the lubricating system in

perfect running condition; see that the stand-by oil pump is always in starting condition, if necessary, by keeping it running at low speed at all times.

(*e*) Test the emergency governor and valves before shutting down.

(*f*) Keep all drains in good working order.

The thanks of the author are due to Messrs. C. A. Parsons & Co., Ltd., for preparing Fig. 7 and for checking the particulars referring to their turbines in Tables 1 and 2; to Messrs. The Metropolitan-Vickers Electrical Co., Ltd., for permission to publish the drawings showing their recent practice in turbine construction and also numerous tables and diagrams prepared by members of the staff under the author's supervision; to Messrs. Brown Boveri & Co., Ltd., for particulars of their high-speed turbines given in Table 2; to Messrs. The Westinghouse Electric and Manufacturing Co., for checking the particulars referring to their turbines and alternators in Table 1; to Messrs. Edward Arnold, publishers, for the loan of the block for Fig. 11; to Mr. N. Chilton for the information given in Table 9; to Mr. Eric M. Bergstrom for particulars of water turbines given in Table 18; to Mr. T. Y. Sherwell for similar particulars of water pumps given in Table 18; to Mr. A. Eagle for the calculation of heat drops and heat contents given in Table 12; to Mr. B. H. Hodkinson for the calculation of reheat factors shown in Fig. 16; to Mr. H. L. Guy and Mr. J. D. Walker for many valuable suggestions and help rendered during the final revision of the text; and to Mr. J. R. Finniecome, who has assisted the author in the completion and checking of most of the tables, and in the calculation and preparation of the various diagrams.

Discussion before The Institution, 7 April, 1921.

Mr. W. H. Patchell: Last year I went over the stations of the Detroit Edison Company, and arrived at an opportune time to see what the Company had done to get over their troubles. Those members present who have not followed the advance in turbine sizes in the United States will, I think, be surprised by the figures given in Tables 1 and 2. In this country we look on a 15 000-kW machine as a large one, and on a 20 000-kW machine as a very large one, but it will be noticed that the author begins at 20 000 kW and goes upwards. I went through the General Electric shops in Schenectady, where there was nothing smaller than 15 000-kW units in one shop. When the war broke out, 20 000-kW machines were the largest standard. The 30 000-kW machine had hardly been made, when there was an epidemic of orders for them, and it was not surprising that since the makers had not had time thoroughly to test them out there was trouble. The Connor's Creek station of the Detroit Edison Company had three 20 000-kW machines and there was also one 45 000-kW machine. A 30 000-kW machine which was put in about the end of 1919, or early in 1920, has never had anything the matter with it. When I was in Detroit a 30 000-kW machine at Delray was running with a few wheels short. The three 20 000-kW machines were run during the war at their full capacity; as a matter of fact, they were overrun, and were overdue for repair when the wheels of one machine gave out. This machine had to be taken out bodily, and a 10 000-kW machine was erected on the site in 7 days. The small part of the steam end of the 45 000-kW turbine ought to be made of steel; owing, however, to steel not being available, it was made of what is called "semi-steel," which split very badly during work, and it was lucky that it could be taken off load. It was hoped that only a joint had gone, but the casting split further while it was cooling down and practically went to pieces. I recently received a letter saying: "The 45 000-kW machine has been in operation almost continuously since the time it was put into service last year; it is performing beautifully and gives promise of being one of the best units we have ever had. The re-built 45 000-kW set, known as No. 4, seems to be equally satisfactory." One of the biggest machines that the Westinghouse Company have built is of 70 000-kW capacity, but I think the author calls it 60 000 kW. It is in the 74th Street station of the Interborough Rapid Transit Company of New York, with some 30 000-kW double-shaft machines. The 70 000-kW set is a 3-shaft machine with a high-pressure turbine in the middle and two low-pressure portions at the sides. There are valves and special devices on it which enable a low-pressure portion to be cut out in the event of trouble, or high-pressure steam to be put on to the low-pressure portion through a reducing valve in the event of the high-pressure turbine cutting out. When I saw that machine early in November one low-pressure portion was down. The low-pressure end is a double-flow machine. After the machine had been in the makers' hands for three months for repair it ran for three-quarters of an hour and vibrated. The machine was cut out in 8 seconds, and pulled up "dead," when it was found that the labyrinth packing in the middle had seized. One end fused completely away; the other end was not so badly damaged. It was very fortunate indeed that the machine was cut out in 8 seconds, otherwise there might have been a repetition of the trouble that has occurred at Boston and other places. At the Boston Elevated Railway plant a 30 000-kW machine began to vibrate. It was thought that something could be done by changing the load, but the wreckage went through the roof.

Mr. A. Q. Carnegie: I am disappointed at the complete absence of any test-results showing the efficiency of impulse turbines. On page 587 the author mentions blading efficiencies as obtained in an experimental turbine, and states that he prefers to leave the answer to test-results accurately determined, but, with the exception of tests for one reaction turbine, I cannot find any actual test-results given in the paper. The reason given is that only one test has been published in the period under review. In the discussion on Mr. I. V. Robinson's paper before the Incorporated

Municipal Electrical Association in June, 1920, I gave some figures of the 1913 20 000-kW Parsons turbine at Chicago, and also of three of the 10 000-kW machines at Carville, but in case the figures I then gave were

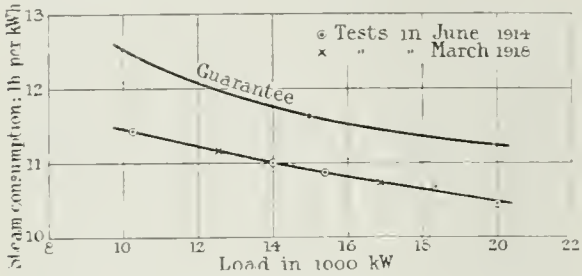


FIG. A.—Steam consumption guarantees and actual tests corrected to guarantee conditions on a 20 000-kW Parsons turbo-alternator.

Guarantee conditions: 200 lb. per sq. in. (g) stop-valve pressure; 200 degs. F. superheat; 29 inches vacuum; barometer 30 inches at all loads.

not sufficiently detailed to enable the actual performances to be judged, I propose to repeat them now in full, together with one more Carville test, also one test of a somewhat larger turbine of the same type will thus be able to compare the actual with the guaranteed figures. In all cases the tests were made with weigh-tanks for the condensate, and with specially calibrated instruments for the electrical measurements. Taking Chicago first, Fig. A shows the guaranteed and corrected test line for this machine. It will be seen that the results obtained were well below the guaranteed line, and it will be observed that the actual result line contains points from two tests, the first taken when the machine was installed in 1914, and the second a repeat test taken in 1918, to ascertain whether any deterioration had occurred after 4 years' commercial working. Table A gives a summary of the figures obtained in both tests, and all the details necessary to enable the performance to be judged. Coming now to the Carville machines, there are in "B" station five 10 000-kW tandem reaction turbines, and Fig. B shows the guaranteed Willans line and the results actually obtained for four out of these five machines, the fifth being actually under test at the present time. I wish to draw particular attention to the periods of commercial working that each machine had completed before being submitted for official test: No. 2, $2\frac{1}{2}$ years; No. 3, $2\frac{1}{2}$ years;

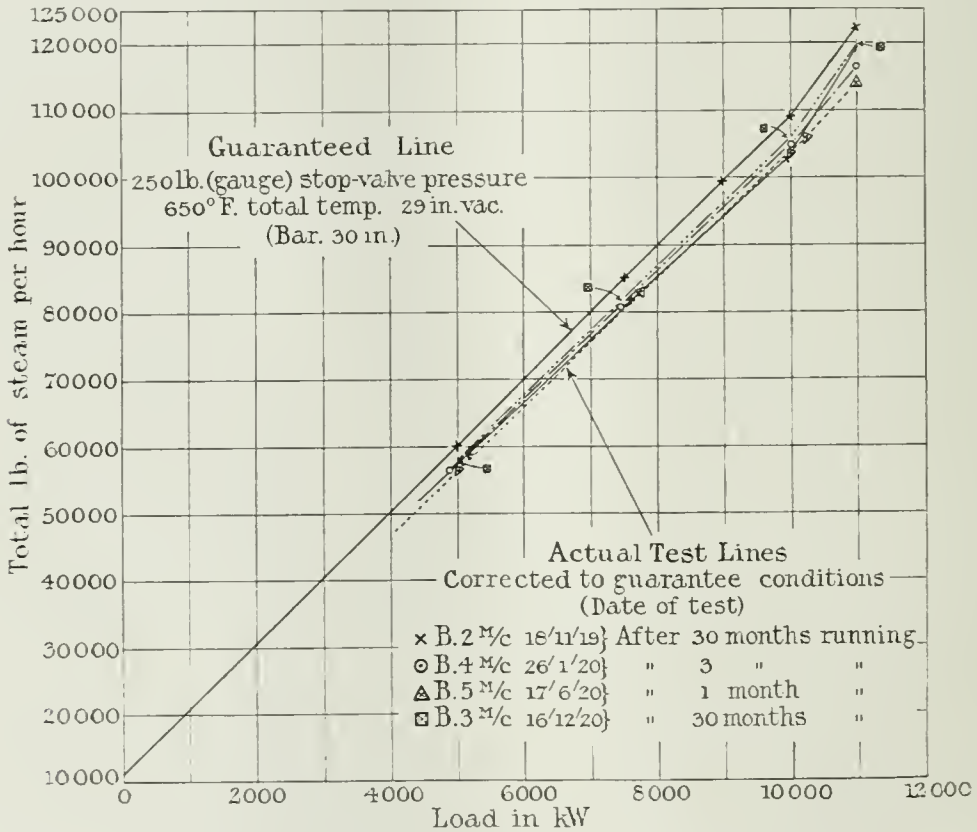


FIG. B.—Tests on three 10 000-kW, 2 400-r.p.m. Parsons turbo-alternators.

at Dunston, and a test-result on the 15 000-kW machine at Lots-road. With the exception of the Dunston machine, all these are mentioned in the paper, and in several cases the guaranteed figures are given; members

No. 4, 3 months; No. 5, 1 month; No. 1, $4\frac{1}{2}$ years. The actual number of hours on load before the official test are as follows: No. 2, 12 824 hours; No. 3, 14 501 hours; No. 1, 26 274 hours. It will be observed

that Nos. 3 and 1 had both run over 68 per cent of the total possible number of hours, taking 24 hours per day and 7 days per week. Figures which have just been received from the test on No. 1 machine, uncorrected, are as follows: Stop-valve pressure, 240 lb. per sq. in.; stop-valve temperature, 692° F.; back pressure, 0·975 in.; steam consumption, 10·3 lb. per kWh. Table B gives details of the results of these four tests. This series of tests, which were taken by the staff of the Newcastle Electric Supply Company on four similar machines, demonstrates first of all the remarkable accuracy with which tests on turbo-alternators can be carried out. Secondly, that, at any rate in the case of a reaction turbine,

TABLE A.

Parsons Reaction Turbo-Alternator at Chicago. 20 000 kW Economical Rating; 25 000 kW Maximum Continuous Rating.

Output	Speed	Stop-valve Pressure	Temperature	Superheat	Back Pressure	Heat Drop, per lb.	Steam Consumption	Efficiency Ratio	Thermal Efficiency	Remarks
kW	r.p.m.	lb. per sq. in.	° F.	deg. F.	in. abs. (Hg)	B.Th.U.	lb. per kWh	per cent	per cent	
20 000	750	200	588	200	1·0	423·2	11·25	71·78	23·90	Guaranteed 1913
20 060	750	192	539·8	155	1·65	385	11·7	75·9	23·77	Actual test, June 1914. Uncorrected
21 227	750	202·4	548·7	159·8	1·13	408	10·84	77·4	25·31	Actual test, Nov. 1918. Uncorrected

TABLE B.

Parsons Tandem Turbo-Alternator at Carville "B" Power Station. 10 000 kW Economical Rating; 11 000 kW Maximum Continuous Rating.

No. of Turbine.	Load	Speed	Stop-valve Pressure	Stop-valve Temperature	Superheat	Back Pressure	Steam Consumption	Heat Drop per lb.	Theoretical Steam Consumption	Efficiency Ratio	Total Heat above 32° F.	Temperature of Vacuum	Total Heat above Vacuum Temperature	Thermal Efficiency	Remarks
	kW	r.p.m.	lb. (g)	° F.	° F.	in. abs. (Hg)	lb. per kWh	B.Th.U.	lb. per kWh	per cent	B.Th.U.	° F.	B.Th.U.	per cent	
B_2	9 991	2 442	251	687	281	0·96	10·04	459·4	7·425	73·98	1 368	77·9	1 322·1	25·7	Actual test, Nov. 1919
	10 000	2 400	250	650	244	1·00	10·35	447·3	7·625	73·65	1 349	79·5	1 301·6	25·31	Corrected
	Guarantee Conditions			—	—	—	10·90	447·3	7·625	69·95	1 349	79·5	1 301·6	24·04	Guarantee
B_3	9 844	2 414	234	707	306	1·04	10·14	460·8	7·491	73·0	1 380·6	80·5	1 332·1	25·28	Actual test, Dec. 1920
	10 000	2 400	250	650	244	1·00	10·53	447·3	7·625	72·4	1 349	79·5	1 301·6	24·91	Corrected
B_4	10 053	2 441	241	678	275	1·00	10·28	452·7	7·538	73·26	1 363·5	79·5	1 316	25·22	Actual test, Jan. 1920
	10 000	2 400	250	650	244	1·00	10·51	447·3	7·625	72·58	1 349	79·5	1 301·6	24·94	Corrected
B_5	10 172	2 440	237	696	295	0·96	10·04	459·7	7·42	73·92	1 373·4	77·9	1 327·5	25·60	Actual test, June 1920
	10 000	2 400	250	650	244	1·00	10·44	447·3	7·625	73·08	1 349	79·5	1 301·6	25·13	Corrected

NOTE.—Efficiencies on Callender's steam tables and stop-valve conditions.

TABLE C.

Parsons 1 000-r.p.m. Reaction Turbo-Alternator at Lots-road Power Station. 15 000 kW Economical Rating; 18 000 kW Maximum Continuous Rating.

Output	Speed	Stop-valve Pressure	Temperature	Superheat	Back Pressure	Heat Drop, per lb.	Steam Consumption	Efficiency Ratio	Thermal Efficiency	Remarks
kW	r.p.m.	lb. (g)	° F.	deg. F.	in. abs. (Hg)	B.Th.U.	lb. per kWh	per cent	per cent	
15 000	1 000	185	501·9	120	1·0	397·8	12·00	71·7	23·21	Guaranteed, 1913
15 047	1 000	176·5	524·3	146·1	0·97	402·8	11·82	71·8	23·35	Actual test, June 1916. Uncorrected

the steam consumption is not by any means a matter of chance, but can be repeated with extraordinary consistency. Thirdly, that the running of a reaction turbine commercially for several years does not reduce the efficiency. I mention this point specially, as there seems to be a general idea that the efficiency of turbines falls off at the rate of about 1 per cent per annum. This is certainly not the case with reaction turbines, and it would be very interesting if the author would give some figures for impulse turbines. Table C shows the results obtained on the final tests of the 15 000-kW, 1 000-r.p.m. Lots-road machine. They can be compared with the guaranteed figures given in Table 22 of the paper. Table D gives the guaranteed and

TABLE D.

Parsons 2 400-r.p.m. Tandem Reaction Turbo-Alternator at Dunston Power Station. 12 000 kW Economical Rating; 15 000 kW Maximum Continuous Rating.

Output	Speed	Stop-valve Pressure	Stop-valve Temperature	Superheat	Back Pressure	Heat Drop, per lb.	Steam Consumption	Efficiency Ratio	Thermal Efficiency	Remarks
kW	r.p.m.	lb. (g)	° F.	deg. F.	in. abs. (Hg)	B.Th.U.	lb. per kWh	per cent	per cent	
12 000	2 400	190	600	216	1·0	424·1	11·0	73·3	24·32	Guaranteed, 1917
11 967	2 400	175·7	607·1	229·2	1·02	421·2	10·8	75·2	24·70	Actual test, March 1921. Uncorrected

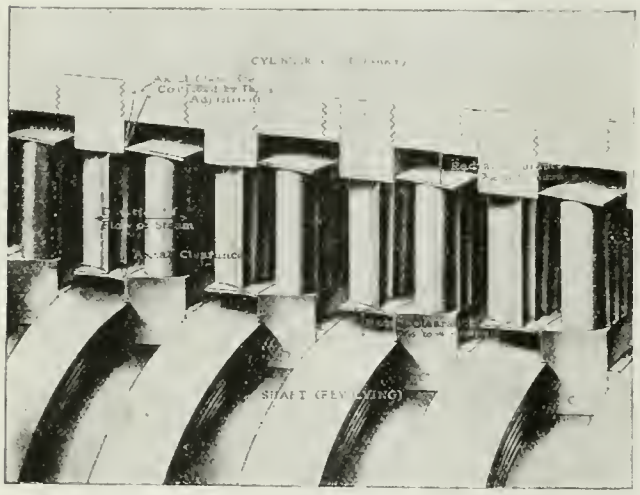


FIG. C.—View of Parsons end-tightened blading, showing large radial and fine axial clearances.

actual uncorrected test figures for one of the 12 000-kW tandem reaction turbines at Dunston power station. The author has omitted to mention the introduction of end-tightened blading, which has overcome the classical objection to fine radial clearance in reaction turbines. In this type of blading the radial clearance ranges from 3/16 inch to $\frac{1}{4}$ inch (and could easily be made $\frac{1}{4}$ yard if desired). Leakage of steam over the blade tips is prevented by the use of fine edged shrouds, part of the Parsons reaction turbine is subject to axial vibration, quite small clearances can be used with perfect safety. In the machines at Carville the clearances when measured hot ranged from about 0·01 inch at the first row to about 0·02 inch at the end of the high-pressure turbine shaft. Fig. C shows the construction and arrangement of end-tightened blading, and also the method now adopted for fixing the blades into their grooves, this being done by means of short serrated pieces driven circumferentially in the groove at the side on the blade base. This method of fixing depends but little on the skill of the workman, as it is impossible to drive one of these short locking pieces home against its neighbour without swelling it enough to fill completely the very minute spaces not naturally filled by the serrations. It is well known that the author is connected with many improvements to the impulse turbine, but he has not hesitated to call attention to many points which are greatly in favour of the reaction turbine. For instance, all the best consumption figures in his tables refer to turbines of the reaction type, and I am sure that anyone reading carefully through the list of troubles given on page 617 would gather that reaction turbines develop only very minor diseases, such as touching of labyrinth packings and failures of lubrication, while the impulse turbines are subject to serious and dangerous troubles such as to cause considerable anxiety for the safety of the engine-room staff. Most of the troubles are due to lateral vibration of discs, and the worst feature is that no one can calculate definitely from the design whether a disc is safe or not. I quite endorse the author's remarks about the use of high-tensile alloy steels, and personally much prefer to keep to a solid construction with stresses well within the limits permissible for ordinary good mild steel, and this is easy in the case of the reaction turbine. The arrangement of multiple-exhaust blading which the author has developed is very ingenious, but offers less advantage than double-ending the low-pressure cylinder in a tandem turbine. Has the author carried out any

actual tests on his multiple-exhaust arrangement? All turbine designers have had experience of designs which are theoretically quite satisfactory, but in which the losses turn out in practice to be considerably larger than expected, with the result that the promised improvement becomes no improvement at all. There are undoubtedly certain considerable losses to be faced in connection with the author's design; steam does not like to be coerced into passing through rows of idle blades without protesting against the treatment, and for this reason it would be interesting to hear that the author has obtained in actual practice results sufficient to justify the optimism he expresses in the paper. In connection with his general statement that tandem turbines are too expensive to build, this has not been my experience, otherwise we should not have been able to sell them. Two-cylinder turbines are free from all troubles due to critical speeds; the parts are lighter and more easily handled, and the latter consideration has a great bearing on shop costs. The losses due to an extra pair of glands and bearings are nothing like so serious as the author estimates. On page 620, column 2, it is stated that the use of discs at the exhaust end of reaction turbines renders such turbines liable to warping and vibration troubles. The author has overlooked the fact that when discs are used in reaction turbines they butt up against one another, and are so mutually supported that no vibration can take place. On the other hand, in impulse turbines the discs are several inches apart, and are delicately placed between stationary diaphragms which are themselves liable to distortion under pressure and temperature, so that if vibration does start, the slightest axial touch makes matters infinitely worse and causes either a fused diaphragm and disc, or an ultimate burst. On the same page the author says: "Owing to the lack of satisfactory proposals to overcome the difficulties which have been experienced (that is, the vibration and bursting of discs), due directly or indirectly to the use of high peripheral speeds, the tendency towards the employment of such high peripheral speeds has received a definite check." Is this not tantamount to saying that, in their anxiety to make turbines cheap, attractive and efficient, makers of impulse turbines have been tempted to adopt high peripheral speeds without being aware of the dangers that lay ahead of them? The adoption of a more conservative policy under circumstances such as these is quite understandable. The author's statement that the use of intermediate binding strip in reaction turbines is detrimental to the efficiency, owing to the disturbances introduced into the flow of the steam through the blades, is not detailed enough. He omits to mention that the steam velocities through reaction blades average only about one-third the velocity through impulse blades, so that much greater liberties can be taken with the steam without incurring appreciable losses, and in addition to this the binding strips are inserted at the inlet edge of the blades where the steam velocity is roughly only one-third of its value at the outlet edge. In impulse turbines, with the exception of frictional losses in the blades themselves, the velocity of the steam is uniform from the inlet to the outlet edge and, with the high velocities that have to be dealt with in impulse blades, I agree that binding strips would be seriously detrimental to efficiency. This disturbance in efficiency is to some extent present in the case of the author's special blades, in which he provides a continuous chain of spacing pieces between each blade and its neighbour. His criticism of the use of silver solder for securing shrouding and binding wire cannot be maintained. Its use has been justified by the experience of at least 25 years, and the troubles due to it are so few as to be negligible. He himself gives the reason why shrouds are secured by silver solder instead of by riveting, when he points out that a distinct limit is imposed on the thinning away of the tips of impulse blades, on account of the necessity for providing a rivet of sufficient strength to hold the shrouding. Reaction blades are admittedly much thinner in the body than impulse blades, and do not lend themselves at all well to a riveted construction. Further, no form of riveting will give the secure hold which is obtained by the use of silver soldering right up to the extreme edge of the blade. The use of a perforated shroud fitted over tenons formed on the ends of the blades and secured by silver solder has a further advantage, namely, that the replacement of shrouding can easily be carried out in the event of damage in handling or otherwise, whereas if the shrouds were riveted into position it would be necessary to replace the entire blading as well as the shrouds.

Mr. R. A. Chattock: The paper is particularly interesting to designers, but it is also valuable to the users of large turbines. When I first read it I was rather astonished by the opening remarks, in which the author describes the present position as a "race for the maximum output at the highest possible speed." However, he goes on to explain that the highest possible speed is only 1 500 r.p.m. for these very large machines, and on that point I am very glad to have his assurance that the speed will not be increased. I feel that these very high speeds are decreasing the reliability of the sets to a very great extent. Reliability is what the user has to consider as his first plank. In Part II the author emphasizes the need for reliability, and I quite agree with him that this is probably the most important matter for the user. The thermodynamic efficiency comes next, but that should not be developed at the expense of reliability. The author points out that both these factors have a considerable effect on cost; however, I feel that it is justifiable to go to almost any reasonable cost to attain reliability. The author also touches upon the question of geared turbines. I have had experience of gears of only comparatively small capacity, but from my experience with these I feel that, from the reliability point of view, we should try to eliminate that type of apparatus as far as possible. We had much better adopt the lower speed than the very high speed of geared turbines. Gearing is always a source of weakness; it is another link in the chain which may cause trouble, and it is as well to eliminate it. I was glad to see that the author emphasizes the necessity for not passing through the critical speed of machines. Apparently it is quite possible to keep within the critical speed on the turbine, but he does

not mention the question of the alternator, and in many cases the alternator builders require to run through the critical speed, with consequent possible injury. It is always undesirable to subject a machine to vibration, even if it is only at the solid end, because the vibration may be transmitted to the more delicate turbine end and trouble may result. If, therefore, it is possible to keep within the critical speed it is much better to do so. I should like to refer to the operation of the turbines mentioned on pages 620 and 621. First of all, as regards the erosion of turbine blades, I quite agree that it is highly advisable to eliminate any CO_2 and oxygen from the steam that is passing through the turbines, but I believe the erosion is chiefly due to moisture in the steam itself. I should like to ask the author if the present trend of higher pressures is not tending to create more condensation in the turbine. We are now calling on the turbine to work through a greater heat range, whilst the total temperature of the steam remains the same (we have not got much beyond 700° F. yet), so that more heat is taken out of the steam over this greater range, with the result that in the lower stages there is more chance of condensation occurring. I am, in consequence, rather afraid that with these higher pressures we shall run more risk of erosion in the lower stages. I should like the author to express an opinion on that point. Then again, taking the temperatures of the bearings, I have had experience of one rather serious case in which the thrust bearing of a turbine failed, although the temperature of the oil leaving the bearing was practically normal. It is impossible to say what happened. I myself believe it was due to the bad fitting of the bearing in the first instance, and to the fact that there was an unequal distribution of the load on the thrust. However, the bearing failed and there was no indication of the oil heating. I think it would be much better if temperature indicators could be provided for the metal in the bearings, so that any dangerous temperature could at once be indicated by, say, an electrical alarm. Again, in these large turbines I think it is advisable, if possible, to fit two thrust bearings. So much depends on the action of the thrust bearing that a stand-by is advisable, and would be quite justified in the cost of the machine. If the thrust bearing fails, it is fairly certain that the blading will be wrecked. There is one other question I should like to ask the author, and that is with reference to filtering the oil. He suggests partial filtration of the oil in small quantities, but it seems to me we are so dependent on the proper condition of the oil that any tendency to sludge, or the possibility of anything getting into the oil system, should be eliminated at once. Would it not be possible to provide a proper filtration of the oil in circulation—some arrangement whereby it could be filtered on its passage through the machine ? The author has been very frank in explaining the troubles that have been met with in some of these large turbines. His paper will be valuable in guiding users to anticipate, if possible, what they may have to expect, and in assisting manufacturers in eliminating these troubles.

Mr. H. L. Guy: In Table 10, line 15, is shown the decrease in heat consumption per kilowatt-hour with increased steam pressure which would be expected from a consideration of the properties of steam and including the influence of wetness, but neglecting the influence of other losses which vary with the pressure. It is of importance to consider the net improvement in heat consumption which can be expected if the effect of the latter losses enters into the reckoning. Such losses are : (1) The cylinder and diaphragm gland losses, which vary directly as the pressure ; (2) the disc friction losses, which also vary directly as the pressure ; (3) the windage losses, which increase at a slower rate than the pressure because the blade height is reduced as the pressure increases (in reaction turbines, the dummy and tip clearance losses will take the place of losses 2 and 3) ; (4) the decrease in the hydraulic efficiency of the blading, which accompanies the use of the increased heat drop with pressure ; and (5) the leaving and exhaust losses which change as the square of the steam consumption. The influence of these losses depends upon their absolute value and, therefore, varies with the capacity and speed of the turbine. For the conditions governing Table 10, i.e. an initial

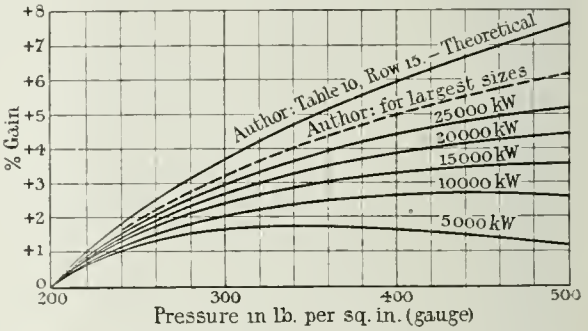


Fig. D.—Percentage gains with varying steam pressures for 1 500-r.p.m. turbines.

temperature of 688° F., vacuum 29·1 inches, and without feed heating, the net gain in heat consumption with increased pressure is shown in Fig. D for turbines of various capacities running at 1 500 r.p.m. The uppermost curve corresponds to line 15 of Table 10. The next lower curve (shown dotted), which represents the net gain for large turbines on page 583 of the paper, approximately coincides with that which would be obtained for a capacity of 40 000 kW. The lowest pressure for maximum net gain in heat consumption is :

340 lb. per sq. in. (gauge) for a turbine of 5 000 kW capacity.
440 lb. per sq. in. (gauge) for a turbine of 10 000 kW capacity.
500 lb. per sq. in. (gauge) for a turbine of 15 000 kW capacity.

The highest pressure which it pays to adopt for a particular rating is, of course, appreciably less than that which is indicated above, and depends on the capitalized value of the net gain and the increase in cost of the high-pressure plant. It will be found that for a 25 000-kW set the net gain is 80 per cent of the

theoretical gain at 300 lb. per sq. in., and 70 per cent at 500 lb. per sq. in. In the larger capacities, and for the higher pressures, slight increases in net gain can be obtained by employing the two-cylinder design. Such additional gains are not as great as might be expected, because the number of glands, and the density and pressure of the steam in the first stages are increased; moreover, there are additional losses in the inter-

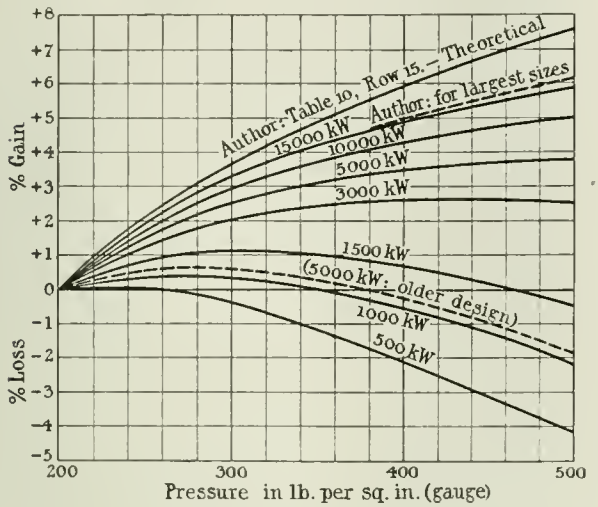


FIG. E.—Percentage-gains with varying steam pressures for 3 000-r.p.m. turbines.

connecting pipes. Further, the increased cost of the two-cylinder machine will be found approximately to cancel the capitalized value of the additional improvement. On the other hand, for the larger capacities, and the higher pressures, the two-cylinder construction may be dictated by mechanical considerations. In Fig. E the position in regard to turbines running at 3 000 r.p.m. is plotted. The author's curve for large turbines approximately coincides with the curve which would be obtained for an output of 16 000 kW. The lowest pressure corresponding to the maximum gain in heat consumption is as follows:

For a 1 000-kW set: 265 lb. per sq. in.
For a 3 000-kW set: 420 lb. per sq. in.
For a 5 000-kW set: 500 lb. per sq. in.

In this case also, the maximum economical pressure is less than these, but it is of interest to note that the lowest pressure for the maximum net gain with a 5 000-kW turbine is 50 per cent higher for a speed of 3 000 r.p.m. than for 1 500 r.p.m. For a rating of 15 000 kW the net gain is 90 per cent of the theoretical gain with steam at 300 lb. per sq. in., and 78 per cent of the theoretical gain with steam at 500 lb. per sq. in. The dotted curve showing the net gains for a 5 000-kW turbine of an older design illustrates the influence of the mean peripheral speed of the blading on the net gain obtainable from increased pressure. Although denominated "older," this turbine is representative of a large number of machines now running which have proved efficient for the pressures of 200 lb. per sq. in. or less at which they operate. The distinctive difference between the two designs is that the mean diameter of the blading of the new design is only about 75 per cent of that of the older design. The later design results in a net gain of 3·1 per cent in heat consumption for a pressure of 350 lb., whereas the net gain with the older design is only one-tenth as great. The two curves plotted in Fig. F show the net gains which can be expected with a steam pressure of 350 lb. per sq. in. as compared with 200 lb. per sq. in. for turbines running at 3 000 r.p.m. and 1 500 r.p.m. It will be noticed that at a given rating between 5 000 and 17 000 kW, to double the speed increases the net gain by approximately 45 per cent. The net gain on a 5 000-kW, 3 000-r.p.m. set is as great as that obtained with a 16 000-kW set running at 1 500 r.p.m. In other words, by doubling the speed the capacity at which a given net gain can be obtained is reduced to one-third. It is for this reason that the use of higher pressures for the moderate- and small-capacity turbines will depend not only upon a reduction in the cost of high-pressure relative to moderate-pressure boilers, but upon the adoption of higher speeds of rotation as the boiler pressure is increased, thereby necessitating the use of gears.

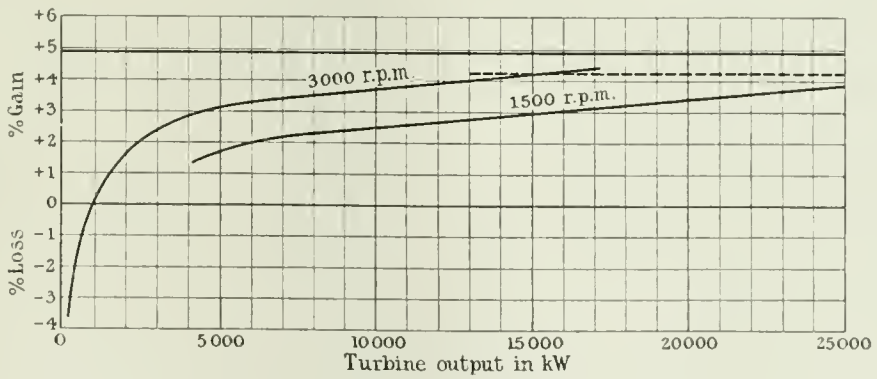


FIG. F.—Net gain in heat consumption between 200 and 350 lb. per sq. in. (g).

Mr. S. E. Fedden: The paper is very fairly written, because it advertises not only the impulse turbine but also the reaction turbine. On page 568 the author mentions gearing. It may interest those present to know that I have had running for the last three years two geared turbines each of 2 500 kVA. The turbines

are running at 5 000 r.p.m. and are geared down to alternators at 1 500 r.p.m. I erected these machines to take the place of two old turbines that were worn out after running for 16 years, and I put them on the top of the old condensers without making any alteration whatever. The guaranteed consumption of these turbines was 15 lb. per kilowatt-hour, and they are running steadily at about half a pound less than that, so that they are well within the guarantee. On page 579 the author mentions fatigue. I should like to ask him how long he reckons the impulse turbine can run without being re-bladed. I presume that after a certain amount of time it would be necessary to take the rotor out and completely re-blade it, in order to be on the safe side and prevent a catastrophe. The author also says that it is quite possible for the disc which runs without vibration under steady load to bump on change of load. I presume that danger would not be so liable to occur in a reaction type of turbine. On page 588 the author mentions the lacing of blading in reaction turbines, and says it is less satisfactory from the point of view of steam flow through the blades, and less reliable. Even if it is less satisfactory with regard to the flow of steam, it seems to me that the reaction type of blading usually produces the best results in steam consumption. On page 609 the author says that a 10 000-kW turbine at 3 000 r.p.m. would be equivalent to a turbine of 20 000 kW at 1 500 r.p.m. If that is so, I think we are hardly right in using these very big turbines until they have been proved very much more than has been the case up to now, because, from the steam-consumption figures which have been mentioned by Mr. Carnegie, it seems to me that a 10 000- or 15 000-kW set at 3 000 r.p.m. is one of the best propositions that can be put down to-day from the point of view of reliability, steam consumption, and coal conservation. I am glad to see the author does not approve of by-passing steam. I think it is most undesirable to pass high-temperature steam into a portion of the turbine that has not been designed for it, more especially, of course, in an impulse turbine. He also mentions that cast iron must not be used because it grows. I have, however, found that cast steel grows very considerably after a few years, so that it also requires watching. On page 612 the author mentions the relationship between steam pressures and superheats of 300 lb. and 300 degrees F., and 200 lb. and 200 degrees F. We have not only to take into consideration the saving in the actual turbine, but also to bear in mind other parts of the plant; and when we remember the enormous wastage there would be in steel economizers, which would have to be used with high pressure, I think we ought very seriously to consider the advisability of using such high pressures. We shall have to take into consideration the capital expenditure as well as the enormous maintenance that may have to be faced if the cast-iron economizer is abandoned and the steel economizer adopted instead. The author's statement on page 616 is, I think, really alarming. The fact that there is a sort of shrapnel shell ready to burst in one's engine-house makes one rather nervous. I happen to have both reaction and impulse turbines, but I notice that it is not only the large turbines that have burst but also the small ones. With a reaction turbine that sort of thing does not occur. There may be a slight stripping of the blades, but that can probably be repaired within a few days; there does not seem to be the liability of such catastrophes occurring as apparently are threatened with the impulse type of turbine. On page 618 the author says there is a great necessity for testing the diaphragm. I should like to know if the diaphragms are tested under the full steam temperature at which they would have to work; otherwise it seems to me that the tests would not be of very much value. I am in hearty agreement with the author in his method of operation. I think that a great deal of the trouble experienced in the past in the running up of turbines has been due to the way in which makers have advised that they should be started up. Some advise that they should be started at full speed, whilst others recommend us to keep steam in them and warm them up standing, which is of course quite wrong. For some years past I have always warmed the turbine up by running it slowly and bringing it gradually up to speed, and I must say that I have had exceedingly good results with my turbines, and very few breakdowns. With regard to large turbines, I think the point is whether we are not going a little too fast. There must be something wrong in the design or the manufacture of these turbines to cause such a number of breakdowns. There is little doubt that turbine makers will put their heads together and learn how to get over these difficulties.

Mr. B. Pochobradsky: In the development of modern turbines two directions are noticeable, one involving a large number of stages of comparatively small diameter, the second a small number of stages of larger diameter. In this country both directions are represented. In the United States turbine manufacturers favour, I believe, a large number of stages of small diameter, while on the Continent the tendency is to reduce very considerably the number of stages and to increase the diameter. The Continental development appears to be based on a long series of tests carried out by various firms and in scientific laboratories, and the main object of such designs is to improve the efficiency. How far that will be possible the near future will show, but the tests already made justify all expectations. The endeavour to improve the heat consumption of the steam plant is well expressed in the continued increase of pressure and temperature. It would not be entirely fair to consider the improvement in economy due to increased pressure and temperature without at the same time taking into account that there are means further to improve the economy, such as heating steam at intermediate stages and using steam from lower-pressure stages for feed-water heating. These additional improvements have been known for many years and they increase with increased steam pressure. Therefore it is not only a question of using high pressure, but also of applying such means as to exhaust more fully the possibilities of improvement. I cannot share the author's opinion that the present development of the steam turbine has reached a critical stage; apparently that opinion is based on a number of unfavourable experiences. In considering the accidents

described, it is advisable to find out whether the cause of the accidents exists only in the modern turbine, whether that cause was known or not known in the past on small or large turbines, and whether it is limited to a type or a certain make of turbine. The troubles described in Table 23 can be divided into four classes : (*a*) general details, (*b*) diaphragms, (*c*) wheels, and (*d*) blades. Items Nos. 1, 5, 8, 13 and 16 would belong to class (*a*). The accidents were due to the omission of provisions for expansion, to failure of oil, and to unsuitable material. The troubles due to the same causes would occur and have actually occurred not only on modern turbines, but on any turbines, in fact would occur on any engine having the same defect in design or material. Items Nos. 4, 9 and 11 refer to troubles with diaphragms. Such troubles are again not limited to modern turbines, although perhaps they are more likely to occur because of higher pressures, higher temperatures, and longer blades. The problem, however, remains fundamentally the same as in the old turbine. The troubles might be avoided by eliminating by-passes leading high-pressure and high-temperature steam to the lower stages of the turbine, and by provision for free expansion of the diaphragm. In large turbines special attention should be given to the bending stresses of the blades, and care should be taken that no undue stresses are introduced in the blades during manufacture. Items Nos. 3, 6, 7, 10, 14, and 15 refer to troubles with discs. Again such troubles were known with old turbines, and the cause lay in too light a design, unsuitable material, and local weakening through balancing holes. Some turbine manufacturers produce turbines with higher peripheral speeds than those mentioned in Table 23, and yet there have been no unfavourable experiences. Such results were made possible by very careful design and thorough testing of material during manufacture. Items Nos. 2 and 12 refer to blade vibration. The occurrence of vibration was known for many years and appeared on old turbines as well. Here again, good design and suitable material eliminate all difficulties. It may be said with confidence that the troubles referred to are not general but are rather limited to certain makes and can be avoided. The steam turbine, speaking generally, has not reached the critical stage of its development, but has reached a stage offering all those advantages which have been expected. I consider that real benefit has been derived from the steam turbine during only the last 10 years.

(*Communicated*) : In Part II of the paper the author describes the influence of pressure, temperature and vacuum on the heat consumption. The factors that are to be taken into account have been discussed on previous occasions and generally agreed upon. One factor has, however, even now been omitted, i.e. the influence on efficiency of higher pressures in the blades. The author based his calculations on the results of tests carried out by his firm with a large number of turbines. These tests have never been disclosed and it is impossible to say whether the deductions drawn from them are correct. As they have been made on a large number of different turbines, it is no doubt extremely difficult to find the exact influence of various factors ; to determine the exact influence it would seem necessary to carry out tests of a much more precise nature. The results of the author's calculations must be therefore considered as approximate and limited to the turbine for which he is responsible. In Part IV he advocates the long blade and moderate mean diameter for the last stage or stages, a design put forward some years ago by the American General Electric Company. Another method to increase the large leaving area was introduced by Fraser and Chalmers' Engineering Works in 1912, the mean diameter of the last stage or stages being very substantially increased and the blade lengths being kept within moderate limits. The reasons that led to this latter design, which has since been adopted by prominent Continental turbine builders, are as follows :—(1) Larger leaving areas are obtainable. In Table 16 the maximum leaving area on a 3 000-r.p.m. machine is given as 11 sq. ft. With the materials and the factors of safety given in the paper, Messrs. Fraser and Chalmers obtained with their design a leaving area of 17·7 sq. ft. on a 3 000-r.p.m. machine. (2) The efficiency of the stage with a large diameter is better. This is obvious from Fig. 27, but the figure itself does not give an entirely fair comparison. If we assume a leaving velocity of 800 ft. per second, which the author mentions as being usual for a good vacuum, it would be impossible to reach with a moderate diameter the most favourable ratio u/c_0, and therefore the efficiency with which the small-diameter stage would work would not reach the maximum ; it would be somewhere on the left-hand side of the curve in Fig. 27. With a large diameter it is possible to work with the most favourable ratio u/c_0, and therefore the large-diameter stage will show an improved efficiency as compared with the small-diameter stage. The blade pitch on a large-diameter stage varies less from the tip to the root of the blade than on a small-diameter wheel with a long blade, and for this reason also the efficiency of the large-diameter stage is better. In addition to the improvement in efficiency it is clear that a shorter blade can be more easily kept out of the range of vibration. To increase the leaving area, the last stage or stages can be easily arranged for double or treble flow. While increasing the leaving area, care should be taken that the advantages obtained through the reduction of leaving losses are not outweighed by the losses due to the splitting up of the steam-flow into two or more parts. I think that the multi-exhaust arrangement shown in Fig. 30 will introduce serious new losses in the endeavour to reduce the leaving losses. It will be noticed that the steam passes through a portion of blading without doing any work, but partial expansions are necessary to do this. The friction losses will be considerably increased for that portion of steam which has to pass through two or three or more sets of blading. Further, there is a very substantial pressure difference between the annular inner space and the outer space of the multiple exhaust. The longitudinal clearances will be in the neighbourhood of 3/16 inch, and there are two such clearances for each additional wheel. As practically the whole pressure-drop of a stage acts across these clearances, considerable leakage must be expected. Unfortunately, however, the leaking steam

flows in a radial direction and will therefore cause disturbances in the steam flow of the working portion and thus further reduce the efficiency. The multiple exhaust substantially increases the length of the machine, the weight of the rotor becomes greater and, if the critical speed of the rotor is to be kept above running speed, the shaft will of necessity have a larger diameter, which in turn will cause increase in leakage losses through the diaphragm glands. These considerations show that the single-flow low-pressure end is to be preferred, and, where it is impossible to supply a single-flow arrangement, simpler means should be applied in order to avoid the losses which are likely to take place in the exhaust shown in Fig. 30. It seems that the efficiency of a multiple exhaust would be so seriously reduced that the advantage of four wheels over three would be nil. Three wheels, according to the author, would increase the leaving area in the ratio of 2·2 to 1. While this ratio appears to be exaggerated, the efficiency of such an arrangement would be so reduced as to make it equivalent to a single-flow arrangement with reduced leaving area. A further disadvantage of the multiple exhaust shown is that only the last stage can be arranged for multiple flow. As the preceding stage is single flow this would mean reduced efficiency under certain circumstances. With the ordinary double-flow or treble-flow arrangement the steam can be split at whatever stage the greatest advantage can be obtained.

Mr. A. Jude: There are several points on which I do not agree with the author. The first is in connection with what he defines as "leaving losses," and which appear to be rather a *bête noire* to him. Last-stage areas, like low-pressure cylinder ports, present troublesome problems which have to be compromised, but the compromise does not by itself kill the big engine. First of all, what is this so-called "leaving loss"? It is the energy corresponding to the leaving absolute velocity and, in big turbines, as the author shows, is some 2 or 3 per cent of the total available energy. A reference to Table 13 will show that the sum of the losses is 7·34 per cent. But the overall efficiency of such a turbine is, say, 80 to 82 per cent. What becomes of the missing 10 or 12 per cent? With the exception of a small local radiation, the whole of it goes down the exhaust pipe, and therefore the whole is the leaving loss. The author is so obsessed with this leaving loss that he invites approval of the very long blades of disc Z (Fig. 27) with and without the multi-exhaust shown in Fig. 30. Is it worth it? Let us take a 1 500-r.p.m., 20 000-kW turbine, at 300 lb. and 650° F., in which the actual speed ratio at the last stage is, say, 0·36 with a mean diameter of 88 inches. Then we have the following table (Table E) of approximate values, the energy drop for the last stage being about 11 per cent of the total available.

TABLE E.

Angle of Nozzle Stream	Length of Nozzles	Theoretical Leaving Velocity (Nozzle Velocity) $= 1 = c_1$	Probable Actual Leaving Velocity, v_4/c_1	v_4^2	$v_4^2 \times 11$ %	Difference from 20°	Difference from 20° at Half Load	Difference from 20°, Day by Day	Value on a 7 200-hour Year (1 % = 5s. per hour)
	in.				per cent	per cent	per cent	per cent	per hour s. d.
20°	24·7	0·445	0·378	0·142	1·57	—	—	—	—
25°	20	0·500	0·425	0·180	1·99	0·42	0·31	0·36	1 10
30°	16·8	0·566	0·481	0·231	2·55	0·98	0·74	0·86	4 4
35°	14·7	0·622	0·528	0·279	3·08	1·51	1·13	1·32	6 8

The cost of a breakdown in the last stage would probably not be less than £5 000, the value (minimum) of the stoppage for the fortnight would be £18 000, and the minimum total about £23 000. The length of moving blades would in this case be only about one inch longer than the nozzles. 17 or 18 inches is a reasonable and manageable length; $25\frac{3}{4}$ inches is not. Against this has to be placed the cost of comparative immunity from breakdown, the insurance premium for which is obtained from the last column above, noting that by the time the value arrives at the feeder panel a very great deal indeed will have been rubbed off it. The author's proposals involve not only long, weak blades, both fixed and moving, but such a variation of pitch that the intended benefit is quickly reduced. The multi-exhaust with its dummy blades is a ludicrous design, at any rate in its present form. The inventor seems quite to have missed the fact that the last stage but one has its leaving losses as well. Does he really suppose that one-half to two-thirds of them will be transmitted as available energy all the way up to the last active blades, right through the obstacles of dummy blades and secondary diaphragms? They certainly will not; rather will toll be taken on the way. I do not like the nozzle box and triple valve arrangement illustrated in Figs. 13, 14 and 33 and submitted as a modern advance. In the first place, the valves as drawn are out of proportion to the size of the steam pipe, although no doubt the author could rejoin that "it works." Next, the two double-seated valves 2 and 3 will be leaking the whole time when shut. I estimate that for a 20 000-kW turbine working at 300 lb. and

650° F., and with an 18-inch pipe, allowing for recovery of energy in the low-pressure stages, the differential expansion would occasion a total loss equivalent to $\frac{1}{2}$ per cent at full load, $1\frac{1}{4}$ per cent at half, and 3 per cent at quarter load, which loss could not be tolerated if a similar or even smaller difference in leaving loss were such a weighty matter. Double-seated valves for this purpose are inherently objectionable unless they are isolated by stop valves, in which case they can be abolished altogether—and why not? Further, the Metropolitan-Vickers Company have evidently, as shown in Figs. 13 and 14, associated this triple nozzle box with the all-Rateau type of turbine—a manifest incongruity. For how can the stream be spread either gradually or suddenly from one-third and two-thirds of the circumference to the whole, without upsetting the proper sequence of things? Fractionalizing under the best conditions is always less efficient than all-round

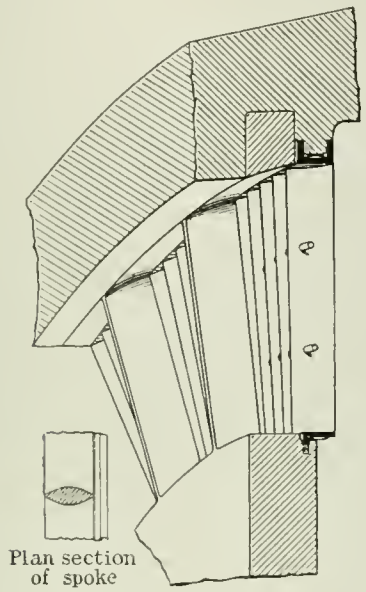


FIG. G.

admission; this design is worse than that. Therefore, a great part of the first-stage leaving losses are doomed to ultimate loss. The enclosed box certainly has points, and it is a pity that the author should spoil it by applying it in an inherently unsuitable way. In regard to diaphragms, numerous failures of which the author for some reason or other has not recorded, it appears that he has followed the old idea that the diaphragm belongs to the family known as "flat plates." It would if it were solid, but it is split and so acts as a beam. The central feather has absolutely no effect on the deflection, at any rate within at least twice the tolerable deflection. The author's difficulty in calculating the deflection was not surprising with the only type of diaphragm considered in the paper—the cast iron or cast steel one with the bent skew blades cast in, serving as spokes—a wretched design which originated in Germany. I submit that the diaphragm shown in Fig. G is a far better one than those described in the paper. This diaphragm has been developed by Messrs. Belliss and Morcom and has now been in use in numerous turbines for several years without any mishap. It is made of rolled steel plate, the windows being cut out of the solid and the spokes finished lozenge-shape. The nozzle blading is portable but very firmly secured without screws or loose fittings, and the advantages are numerous, not the least being the opportunity of making brass blades, and an apparently greater nozzle efficiency than the crude cast-in affair possesses. The deflection of this diaphragm with its symmetrical spokes is calculable within close approximation and is confirmed by actual experiment. Tests were made by applying distributed dead weights (where feasible) or a concentrated load by a lever and crane pulling through a weigher, the plate lying on a ring or resting in its own groove. There was no need to test for deflection in the ordinary course. In regard to wheel rims and blade attachments I consider the straddle blade, both American and Rateau types, to be bad design. With the practical absence of any rim stiffening they both offer opportunities for the wheel to warp and flutter, while the Rateau pattern adopted by the Metropolitan-Vickers Company adds the invitation to crack through the numerous pinholes. The stiff, wide and deep rim is in my opinion the best insurance against wheel trouble, and I agree with the author that the wheel bodies should be as thick, not as thin, as possible. Some of the diagrams in the paper hardly exemplify this doctrine, however. I am not in favour of the proposal to adopt 29 inches vacuum as a standard. The exceptions would be more numerous than the applications; it would be fostering the very troubles the author has been exhibiting, and force an admittedly risky condition on the ratepayers, because big turbines are rather public than private ventures. It ought rather to be the policy of the manufacturer to advise moderation and leave it to the buyer to demand excesses at his own risk. I hope the proposal will be rejected by the British Engineering Standards Association.

Mr. R. J. Kaula: I agree with the author in his cautionary remarks as regards the use of excessively large discs for 1 500-r.p.m. machines, and I appreciate the ingenuity of his multi-flow exhaust design. At the same time I think he will probably agree that all these methods of avoiding large diameters by using multi-flow exhausts and double-flow turbines are in the nature of a passing phase and that sooner or later we shall come back to a simple straightforward design, especially in the case of impulse turbines. In the meantime the main obstacles in the way of the large single-flow design seem to be the difficulty of obtaining a disc of consistently reliable material throughout, coupled with disc vibrations, about which little is known. The latter problem is very much akin to that of critical speeds, which was being attacked in 1912, when the author read his previous paper. That paper contained a valuable contribution towards the solution of critical-speed problems. These are now faced with comparative ease, and I think it is reasonable to expect that the present disc-vibration problems will be solved in a similar manner now that the causes are being sifted

and understood, so that designers will be able to avoid dangerous vibrations. I should like to refer briefly to a case of disc vibrations which I had to investigate with my associates in 1917, with the help of Mr. W. M. Selvey. The case was peculiar in that it occurred on a new machine of a well-tried pattern. Apart from any minor differences which could hardly have any bearing on the subject, the only peculiarity of this turbine lay in the fact that it was coupled to a 25-period alternator by means of a solid coupling, whereas with one other exception all other machines built to this pattern were coupled to 50-period alternators also through solid couplings. The machine that was driving a 25-period cycle alternator, however, was fitted with a flexible coupling. The vibration occurred almost immediately the load was put on and it took the shape of a wave that was stationary in space and had a peripheral length of about 120 degrees. The result was that the blades and the rim of the disc touched the adjacent diaphragm, causing very considerable damage. An examination of the disc material showed that it was inclined to be faulty, and we thought we might overcome the difficulty if we put in a new disc of somewhat different material. A disc of very carefully selected steel was therefore tried. Before the machine was re-started, the adjacent nozzle plate which had suffered on the previous occasion was placed at a greater distance from the disc, so as to avoid a repetition of the damage. At the same time, steps were taken to record the disc vibrations and the nature thereof. When the machine was started up, vibrations of the same order were observed, although the precautions taken prevented further damage, showing that the alteration had not effected a cure. Neither this disc nor the original one took a permanent set. We then decided that we should have to investigate on the lines of resonance. I do not think that had been done before in connection with disc vibrations. We found, as an outcome of our calculations and experiments, that the natural frequency of the disc when corrected for normal speed was 3 000 per minute. Although it was not possible to obtain positive proof of the assumption that the vibrations were actually due to resonance, there seemed every reason to conclude that the coincidence of the number of pole-changes of the alternator —3 000 per minute for a 25-period machine—with the natural period of the disc was in fact producing resonance. It was therefore decided to alter the dimensions of the disc so as to remove its natural period well away from 3 000 per minute. No further trouble was thereafter observed. If further experience proves that various forms of vibrations which may be transmitted to the disc in one form or another are apt to produce resonance vibrations of a dangerous order in the discs, it will be necessary to determine two factors: first, what nature of vibrations causes such effects, and second, what is the period of these vibrations. If these two factors can be predetermined it should be a comparatively simple matter to ensure that the natural periods of all discs are well removed from the periods of the vibrations in question.

Mr. A. B. Field: In the discussion rather undue prominence has, I think, been given to the 1 500-r.p.m. machine. The most marked development in the period referred to by the author appears to me to have been that of the high-speed (3 000-r.p.m.) turbo set, and this has taken place almost entirely in Europe rather than in America. The author's Table 1 of "large machines" will be found to reflect chiefly American practice. Table 2, on the other hand, dealing with large sets comprising 2-pole generators, deals almost entirely with European practice. The reason for this divergence in the trend of development on the two sides is very simple; it is largely due to the difference between the standard frequencies here and in the States—namely 50 periods versus 60 periods. Upon the basis that the author has adopted for comparing the maximum possible outputs at different speeds, this 20 per cent difference in frequency gives European practice nearly a 50 per cent advantage for the 2-pole set; that is to say, if our limit at 3 000 r.p.m. should be set at 20 000 kVA, say, the corresponding 60-period limit would be 14 000 kVA, and so the Americans are forced to use speeds corresponding to 4 poles in order to obtain a reasonable size of unit. Just prior to the war there seemed a possibility of our having a considerable demand for big sets, of individual ratings up to 50 000 kW or more, but I believe that probability has disappeared for the present in Europe on account of the development of the 3 000-r.p m. set. In 1913 the size of sets offered for a speed of 3 000 r.p.m. was limited to about 5 000 kVA maximum continuous rating, and a purchaser of plant for a large station was driven to select a speed of 1 500 r.p.m.; he then naturally grasped all the advantages to be obtained by increasing the size of his unit up to the limit compatible with his total proposed station output. Now, however, as the author points out, more than one firm is quite prepared to offer machines having a continuous rating approaching 20 000 kVA at 3 000 r.p.m. and, as this represents a reasonable unit even for a super-station, the purchaser will hesitate before losing the many advantages of a speed of 3 000 r.p.m. in order merely to increase the size of his unit. I should like more information with regard to the Ljungström turbine; very little is given by the author, and no test-results. He mentions that a 30 000-kW machine of this type is under construction at Schenectady, but I believe the same statement would have been true four or five years ago.

Mr. F. Samuelson (*communicated*): In reviewing the developments of recent years, the author aptly describes the endeavours as a race for maximum output at highest possible speed. The race was undoubtedly started in America a few years ago, where both manufacturers and purchasers were running in keen competition. If Chicago installed a 25 000-kW set, then Detroit would not be satisfied with less than 45 000 kW, and New York would have to cap that feat by ordering a 60 000-kW set. While this competition was going on no time was allowed for matured consideration of the serious problems involved in designing and building such large units, and the inevitable result was trouble of a very serious nature, failures being experienced with turbines in some installations, and with generators in others. The author is apparently encouraging this

race for highest output at the greatest possible speed, and justifies it on account of greater reliability, efficiency, and lower first cost, putting reliability first. I would suggest that this order is not correct, but that lower first cost has received first consideration. I think most engineers will agree with me, that the lower-speed unit will be found to be the safest from an operating point of view. As regards first cost, the author admits that a high-speed alternator is no cheaper, less efficient, and more difficult to design, so that any advantage in the first cost must be sought for in the turbine alone. In Table 14 the cost of the turbine alone is given as 8·9 per cent of the total cost of a power station, therefore a high-speed turbine would have to cost very much less than a low-speed machine to make any appreciable difference in the total cost of generation, and I suggest that a customer would be badly advised to run the least risk of getting less reliability or efficiency in order to get a gain of 2 or possibly 3 per cent in his initial outlay. The reduction in cost of the high-speed turbine alone will not be so great as one would be led to believe from the difference in speed, this being generally in the ratio of 2 : 1. For the same output the high-speed machine requires the same valves and controlling gear, the same size of oil pumps, oil cooler, piping, etc., as the low-speed machine. The exhaust end in the two cases will be very nearly of the same weight, having to deal with the same quantity of steam in both cases. The only items reducing the cost are the size and number of wheels, diaphragms, and main casing. This view is also supported by the author in par. (7) of Part IV. A serious drawback with the 3 000-r.p.m. turbines is that they cannot be used for 25-cycle supply, which is the standard frequency in Glasgow, Birmingham, and other districts. Further, I do not think that turbo-generators of 10 000 to 15 000 kW at 3 000 r.p.m. are so efficient as machines of these ratings running at 1 500 r p.m. The great difficulty of designing and manufacturing satisfactory alternators of upwards of 20 000 kVA at 3 000 r.p.m. should be fully realized by the purchasers of such machines. Very little experience, or knowledge, has been had with such machines, no official steam consumptions are given in the paper, and we have so far heard only of attractive promises. The author states that the demand for turbines of large capacity in this country has been retarded by the war, and that developments have mainly taken place in the United States. While I agree there have been greater developments in the United States than here, is it not a fact that our conditions do not warrant the installation of sets of more than 25 000 kW? Also, the available facilities for fuel and water favour more moderate sizes of plant than is the case in America, where also the limitation by transport, rail and water, is less onerous than in this country. The absence of developments of monster units in this country may therefore be considered as due to natural causes, more than to want of initiative. From a reliability point of view, a power station should not be equipped with less than six generating sets, preferably eight. To exercise proper inspection and maintenance necessitates periodic examination and overhaul, so that one set may be considered to be out of commission at all times, and one to be a stand-by, leaving four or six machines in commission, which tends towards the use of moderate-sized machines.

Under the heading of vibration, the author states that the turbine shafts should run below the critical speed, and in this connection it is interesting to note that the Swiss turbine makers run their turbine shafts through the critical speed, which enables them to build machines at slightly lower cost and having less leakage losses. Personally, I lean towards the same view as the author, but it is possible that the risk we fear of running above the critical speed is exaggerated. Is it not a question of accurate balancing, and the provision of a yielding bearing, in case of trouble, such as the British Thomson-Houston Co. have successfully used on a very large number of their electrical rotors? In my opinion, it is no more difficult to provide against vibration of the turbine discs at 1 500 r.p.m. than at 3 000 r.p.m. In his table of turbine troubles, the author gives several examples of failure of the General Electric Co.'s turbines, owing to vibration of the turbine discs. When these machines were designed, the phenomenon of disc vibration was not understood, and, owing to the large number of machines supplied before troubles began, the failures have been more numerous than would otherwise have been the case. As soon as it was realized that these failures were due to causes not fully understood, the matter was taken in hand by the Research Laboratory in Schenectady, where the engineer, the chemist, the physicist, and the metallurgist all took a hand in studying and providing a cure for the serious wheel-vibration troubles that had developed. In order to ascertain the nature and magnitude of these vibrations, and also to determine the extent to which turbines in operation were liable to such occurrences, it was found necessary to devise special instruments. One of the most useful has been in the application of the oscillograph. Steam-proof electrical exploring coils were designed, so as to be mounted in the diaphragm of any turbine the wheels of which were to be tested and analysed. Any fluttering or vibration of the wheel varies the air-gap between the exploring coil and the wheel, and causes a characteristic variation of the current in the exploring coil, and this current properly amplified accentuates a standard oscillograph; the result is recorded photographically in a permanent form in the usual way. It is possible to determine whether a wheel is vibrating or fluttering, and such records may be taken under all conditions of operation. Extensive surveys have been carried out with this most useful device, and the data thus obtained have been helpful in solving this vibration problem. Another simple method of rendering visible the vibration of a wheel consists in placing the wheel to be tested in a horizontal position, sprinkling the same with sand, and vibrating it by means of an alternating-current solenoid. The sand forms in ridges where the vibration is low, and also indicates the line of highest stresses. It also shows the general character of the vibration. Knowing the frequency of the alternating current, the resonance frequency is thus readily obtained. The problem was also attacked by the construction of model

wheels of various materials, such as steel, plaster, rubber, and paper. These models were rotated at various speeds, and the characteristics studied for comparisons with the phenomena observed in actual turbine wheels. The materials chosen for the construction of these experimental wheels were, in many instances, unsuitable for turbine construction, but the choice was made in order to obtain the greatest ranges of flexibility and elasticity, and with a view to exaggerate or amplify the facts which were being studied. Many wheels of soft and hard rubber, and of various metals, and of other materials such as plaster of Paris have been made and tested. The information obtained from tests of hundreds of such wheels has been of the utmost value. For example, it has been demonstrated that vibrations may be likened to the waves in a flag, and may occur when the relative speeds of the wheel and surrounding vapour exceed a certain amount. It has been shown that a wheel which runs true at certain moderate speeds has a sensitive upper speed upon which it wavers like a flag in an air current. It is possible to observe what effect any change in the shape or dimensions of the wheel would have in changing the speed at which vibrations occur. For example, it was found that a small increase in the dimension of the web, or outer rim of the wheel, raised the critical speed of vibration so high that it could not be made to vibrate even at a relatively excessive velocity. I shall not attempt to explain the exact theory as to the cause of this vibration, but shall say briefly that it is believed to be due to the friction of the air or surrounding vapour on the surface of the wheel, and is found to vary with the pressure of the surrounding medium. Even the thinnest wheels made of Bristol board or paper vibrate or wave with the greatest ease at low speed when surrounded by air, but lose this property as the pressure of the air is reduced, and run perfectly true in a vacuum. The practical turbine involves, as is well known, a great range of pressures surrounding the wheels, from, say, 100 lb. to a fraction of 1 lb. per square inch; thus the possibility of oscillation or transverse vibration in any particular wheel-design must be carefully studied. Oscillograph records have been taken of fluttering rubber wheels, and similar records have been obtained from actual turbine wheels, duplicates of some wheels which had developed cracks in service. The conclusion from all the investigations made is that these cracks are due to fatigue of the metal from repeated bending stresses, and not to excessive centrifugal stresses. Tests with the experimental wheels, as well as with wheels intended for production, have demonstrated that even slight increases in the thickness of the web and outer rim of the wheel raise the critical speed of vibration of such a wheel so high that it cannot be set in vibration even at relatively excessive velocity. Oscillograms taken from actual turbines having modified wheels show no vibration, and this very serious trouble is now fully understood and taken care of in the later designs. The very extensive experimental work involved in this study has enabled one to form a better definite physical conception of what takes place in a turbine structure while the machine is in operation.

With regard to the blade efficiency given in Fig. 27, it would be interesting to know how the author has arrived at the efficiencies at different points in the blade. It does not seem reasonable to expect such a slight difference in efficiency with such a wide difference in pitch as is found with blades having such large ratios of length of blade to mean diameter of wheel as recommended in the paper. With regard to the multi-exhaust design shown on page 606, it will be obvious that several points exist where additional leakage can take place. Losses must also occur in passing the steam through the inner inactive portion of the blades, and through the long nozzle passages. The individual leakage and frictional losses may be small, but the total will be quite perceptible and will offset materially any gain that may be had from the multi-exhaust principle. In the machine shown in Fig. 14 the axial distance between the first of the two exhaust stages and the thrust bearing locating the rotor is about 12 feet. The axial clearance between this stage and the succeeding nozzle diaphragm will probably be about 0·15 inch, to allow for differences in the axial expansion of rotor and casing under varying conditions. This clearance will provide a leakage area from the last stage to the condenser of about 1·5 per cent as a minimum. This loss may be increased if the axial clearances at the leakage point are allowed to get larger, due to axial clearance being improperly adjusted or exceptional temperature-changes taking place. Other losses inherent to this design are double spilling losses at the outer radius of blades, friction losses through extra blades and nozzles, which will bring the total losses to an amount which would compensate for a considerable increase in the leaving loss in a normal design of machine. It is not essential for high efficiency to design for too low leaving losses, as the steam has to be brought from the blade exits to the condenser, and either this is accomplished by the remaining velocity in the steam itself, or such velocity has to be created by difference in pressure between the condenser and the outlet of turbine blades. The weight and cost involved in the additional exhaust stage, or stages, is quite unnecessary for machines operating at 29-in. vacuum, with normal outputs of 25 000 kW at 1 500 r.p.m. and about 10 000 kW at 3 000 r.p.m., as within these limits there is no difficulty in providing straightforward design and full leaving area with ordinary materials. The largest size of unit likely to be found commercially useful to install in this and most other countries is probably 25 000 kW, and as regards 3 000-r.p.m. machines, 10 000 kW is nearly approaching the limit where the alternator can be offered with every confidence in its reliability. The usefulness of the multi-exhaust design is not very obvious, and its practical success has not yet been proved. It is to be regretted that there is no reference in this paper to the new North Tees station, for which the author has designed some very novel and interesting plant. Information as to the value of the reheating factor may be expected from this plant, but I doubt very much if the gains expected by the author will be realized. I understand that in this case the steam on leaving the high-pressure turbine is passed

back to the boiler for reheating before entering the low-pressure turbine, and I expect there will be a considerable loss from drop in pressure in passing the steam through the pipes and reheaters, which loss in pressure is irrecoverable. In designing a new power station some three years ago, I investigated the reheating of the steam, but instead of taking the steam back into the boiler, the reheater was put into the base of the turbine, and combustion gases were taken out of the boiler furnace, passed through the reheater, and returned to a lower temperature zone in the boiler. The minimum loss of pressure is thereby incurred for the steam, and circulation of gases through the heater is easily accomplished by a fan suitably built for high temperatures. Such an arrangement is, in my opinion, more likely to be a success than the alternative method adopted at the Tees station. The gain, however, is so small that I do not think either will prove a commercial success. I am not in agreement with the author's considered opinion that turbine development has arrived at the most critical stage in the history of the steam turbine, which opinion is presumably founded on the troubles experienced during the last two or three years. New problems have certainly cropped up, but they have been successfully overcome, and as long as turbine designers keep to moderate outputs at 1 500 and 3 000 r.p.m. respectively, customers need not apprehend the smallest risk in installing such machines.

Dr. A. Stodola (*communicated*): The chief point of interest in the paper is in connection with the multi-exhaust turbine, and it seems that most of the advantages claimed are well founded. The problem of obtaining the maximum leaving surface is indeed fundamental. The solution put forward by the author is made possible chiefly by the ingenious way in which the rigidity and the necessary area for the steam passage have been safeguarded in the construction of the intermediate blades of the multiple-exhaust stages. A factor which is of great assistance in this connection is the tension due to the centrifugal stresses. The enlargement of the blade at its root and its fastening in the double fork-shaped rim of the disc are so solid that I think the exhaust part would bear still higher peripheral speeds, in which case the capacity should be sufficient with two exhaust ports only for the greatest output likely to be demanded in the near future. The introduction of the exhaust steam in regular stream-lines along the whole length of the condenser, as shown in Fig. 15, is one to which I think no opposition at all will be raised. However, the regularity of the flow does not depend only on good guiding subdivisions. If the steam leaves the turbine with an appreciable circumferential velocity component (generally not equal on the top and at the root of the blade), it carries with it an "impulse moment" which will subsist in each of the partial compartments until it is destroyed by the frictional forces. There are, however, more possibilities of a reduction of the circumferential component in the multi-exhaust turbine, than in other types. One of the best features of the construction is the insulated nozzle boxes shown in Fig. 32. There seems no difficulty in installing four instead of three, in which case only about one-quarter of the total quantity of steam would be subjected to throttling; in other words, the governing would be as good as that obtained by the use of a Curtis wheel as first stage. The present Continental practice is on the lines of greater circumferential velocity, because, as is pointed out in the paper, only large discs can afford the necessary leaving area. By combining this with the free expansion in the triangular end of the nozzle and in the intermediate space between the nozzle and wheel, the number of impulse stages is kept very low. Whether turbines of this type are more economical in steam consumption and in first cost has not yet been ascertained with certainty. The high-speed turbine running at, say, 3 000 r.p.m. will be chiefly affected by this consideration. There is no reason why any impulse turbine should not make use of free expansion later if this will prove more economical. In any case this need be applied only to the high-pressure stages; the multiple-exhaust principle makes it superfluous for the most critical point of the turbine, namely, the last wheel. On the whole, the multiple-exhaust principle appears to rest on a sound foundation, and we may look forward with confidence to the results of the practical performance of the author's turbine. There is one point in connection with the operation and starting up of large turbines which deserves much attention. It is recognized that to warm the turbine while at rest may cause unequal distribution of temperature and consequently distortion. But to start the turbine by passing a puff of steam sufficient to keep the rotor turning slowly also has its dangers. Calculations of the transmission of heat show, indeed, that such a puff will heat up the thin discs in from 20 to 40 seconds almost to the temperature of the steam, while the hub and the shaft remain quite cold. Thus, a large stress is produced in the discs and may cause permanent deformations. A very simple and yet reliable remedy would be to admit the puff for, say, not more than 5 to 10 seconds, according to the thickness of the discs, and then to shut the admission valve for some minutes in order to equalize the heat distribution. Those in charge of large power units should not neglect any possibility of maintaining their plant at the highest level of reliability.

Mr. K. Baumann (*in reply, communicated*): During the discussion more than one speaker raised the same point in several instances. In order to keep my reply within reasonable proportions, therefore, such points will be dealt with in connection with the remarks of the speakers who first raised them. The following remarks must therefore be considered as a reply to the discussion as a whole, rather than as a collection of separate replies to individual speakers.

American experience.—Mr. Patchell's description of the situation which existed in regard to turbines in some of the power stations in the United States in the autumn of 1920 is generally in accordance with the conditions as I found them. I agree with his conclusion that, owing to the rapid development which occurred towards large sizes, some difficulties were bound to be encountered. As a result of the sudden rush of orders for very large turbines, for which, no doubt, the turbine users must take their share of responsibility,

very little time was available for the manufacturers to test the machines thoroughly before putting them into commercial operation. To encounter difficulties is not necessarily blameworthy. It must be realized, of course, that the origin of any trouble must be determined quickly with a view to its removal before serious harm is caused to the industry. It is our duty to analyse difficulties as they arise, so that the industry may ultimately derive benefit from them. I think we ought to be particularly thankful to our friends in the United States for the frankness with which their difficulties have been discussed in the Press and in papers presented to various Institutions, and I am sure that they will not grudge us the benefit of their experience. One object of the paper was to facilitate the study of their difficulties so that their experience may become available to a wider circle of engineers. There is no doubt that there have been difficulties, of such a nature that they would not have been expected 10 years ago to influence turbine development to the extent which they undoubtedly do to-day. This applies particularly to the difficulties experienced in connection with disc and blade vibrations. I believe that the difficulties in the United States in connection with large impulse turbines have been greater than those experienced with large reaction turbines of the two-cylinder type. Engineers who have neither the time nor the opportunity to investigate thoroughly the cause of the troubles might jump to the conclusion that these troubles are inherent to the impulse type of turbine as built in a single casing. That this is not the case, I have clearly shown in the paper. The difficulties are primarily due to the use of large and thin discs running at high peripheral speeds. Such discs are subject to vibrations which in time may cause a failure of the discs. I have endeavoured to make a case for a solution in which the use of such large discs is avoided, and which does not involve the use of the two-cylinder construction or the use of two separate exhausts. Mr. Patchell refers to some troubles experienced with a 60 000-kW maximum continuous-rated turbine at the Interboro' Rapid Transit Co.'s station in New York. His description of the difficulties experienced there shows that troubles with large turbines are by no means limited to impulse turbines, and I agree with him that if a rub occurs in a reaction turbine it may result in very serious damage, which would involve the necessity of replacing the whole cylinder and rotor.

Test-results.—The absence of test-results of large turbines, referred to by Mr. Carnegie, is explained by the fact that, so far as this country is concerned, only a few impulse turbines of large capacity exist, and most of these have been in commercial operation for a short time only. When the paper was written, test-results of only one large reaction turbine were available, and these have been referred to in the paper. I was fully aware of the information given by Mr. Carnegie in the discussion of Mr. I. V. Robinson's paper, but this information consisted merely of test curves in which the actual results were corrected to guarantee conditions. No details of the tests, such as a description of the methods adopted for measuring the water and for calibrating the instruments, were given, nor was any mention made as to who was responsible for the tests. I am glad, therefore, to have the further particulars which Mr. Carnegie has now given, and I have analysed the results as shown in Table F in the manner proposed in the paper, for the purpose of comparison with future tests on new turbines.

TABLE F.

Steam Consumption Test-Results.

1	2	3	4	5	6	7	8	9	10	11	12	13
	Item No.	Reference to Tables	Reference	Manufacturer	Power Station	Year	Load	Speed	Steam Conditions at Stop Valve			Vacuum at Exhaust
									Pressure	Temperature	Superheat	
							kW	r.p.m.	lb./sq.in.(g)	°F.	deg. F.	in. ab. (Hg)
Test-Results	1	A	A. Q. Carnegie	Parsons	Chicago	June 1914	20 060	750	192	539·8	155	1·65
	2	A	A. Q. Carnegie	Parsons	Chicago	Nov. 1918	21 227	750	202·4	548·7	159·8	1·13
	3	C	A. Q. Carnegie	Parsons	Lots Road	June 1916	15 047	1 000	176·5	524·3	146·2	0·97
	4	—	A. Q. Carnegie	Parsons	No. 1 Carville	May 1921	10 000	2 400	240	692	289·2	0·975
	5	B	A. Q. Carnegie	Parsons	No. 2 Carville	Nov. 1919	9 991	2 442	251	687	280·4	0·96
	6	B	A. Q. Carnegie	Parsons	No. 3 Carville	Dec. 1920	9 844	2 414	234	707	306·3	1·04
	7	B	A. Q. Carnegie	Parsons	No. 4 Carville	Jan. 1920	10 053	2 441	241	678	275	1·00
	8	B	A. Q. Carnegie	Parsons	No. 5 Carville	June 1920	10 172	2 440	237	696	294·2	0·96
	9	D	A. Q. Carnegie	Parsons	Dunston	Mar. 1921	11 967	2 400	175·7	607·1	229·2	1·02

The turbine which the Metropolitan-Vickers Co. supplied to the Glasgow Corporation (item 21 of Table I) has not yet been tested officially. It is the only set in operation in the new power station and, as the supply is largely dependent on this unit, the manager

of the Electricity Department is anxious not to disturb the plant for the present. Some preliminary tests were made at the end of last year, which indicated that the consumption is well within the guarantee. This has also been demonstrated by the coal consumption of the plant, which is being checked daily and works out at 1·55 lb. per kWh generated, with coal of a higher calorific value of 11 000 B.Th.U. per lb., and with the load varying from 10 000 to 20 000 kW, the average load for 24 hours being 14 000 kW. This corresponds to a thermal efficiency of just over 20 per cent. Allowing 4 per cent for power required by auxiliaries in the power station, this would correspond to an overall thermal efficiency of 19·2 per cent referred to the higher calorific value. The best results yet published are those of the Detroit Edison Co. at Connor's Creek, where an efficiency of 17·3 per cent for the year ending 30th June, 1916, was obtained with an average load of 14 300 kW. As soon as the set at Glasgow is available, accurate consumption tests will be made and a full report with all essential details will be published as soon as possible. Since the beginning of February the set has continuously carried loads varying from 10 000 to 20 000 kW. There have been no difficulties and no forced shut-downs except those necessitated by the cleaning of the condenser. The total number of hours run during four months was 86 per cent of the maximum possible, and the load factor for this period was 58·6 per cent referred to the maximum continuous rating. During the unfortunate breakdowns in the electricity supply in February, the set was operating under severe conditions. On 9th February, for instance, a load of 20 000 kW was suddenly thrown off and the speed rose from 1 500 to 1 580 r.p.m., afterwards remaining permanently at 1 550 r.p.m. On the previous day the condenser auxiliaries suddenly stopped owing to an accidental operation of the main switch controlling them. The load at the time was 13 000 kW. The set changed over to atmosphere and was kept running for 10 minutes at a load of 6 000 kW without showing any signs of distress.

Mr. Carnegie concludes from the list of troubles given in the paper that the reaction turbine develops only minor defects as compared with the difficulties experienced in impulse turbines. He seems to be content with the position of the reaction turbine remaining such that this type of machine is still liable to stripping of blades, but in comparing it with the impulse turbine he neglects the fact that the troubles on impulse turbines referred to in the paper can be overcome with certainty by the adoption of the multi-exhaust principle and the relatively low peripheral speeds and heavy discs which are associated therewith.

Multi-exhaust principle.—Mr. Carnegie's criticism of this principle on the grounds that steam does not like to be coerced into passing through rows of idle blades without protesting against the treatment, shows that he has not yet properly understood it. In the multi-exhaust construction there are no idle blades and, in fact, the inner portions of blades which are referred to are similar in type to the wing blades which are in common use in the exhaust end of the reaction turbine. The efficiency of these portions of the multi-exhaust blades is relatively high because the velocity ratios are arranged for the highest efficiency and because the carry-over energy is utilized in the succeeding rows of blades. Other speakers also have made statements in connection with the multi-exhaust blading which indicate that they are under a similar misapprehension. Their attention seems to be focused on some constructions that have been described in patent specifications, which may be easily understood but which are not as efficient as the construction actually employed in practice.

TABLE F—*continued.*

Steam Consumption Test-Results.

14	15	16	17	18	19	20	21	22	23	24	25	26
		Overall Thermodynamic Efficiency in per cent										
Heat-Drop	Steam Consumption	Actual	To obtain Corrected Efficiency divide Actual Efficiency by these Correction Factors					Corrected	Heat in Steam less Heat in Condensers	Actual Heat Consumption	Thermal Efficiency	Remarks
			Pressure	Superheat	Vacuum	Output and Speed	Design					
B.Th.U./lb.	lb./kWh	per cent						per cent	B.Th.U./lb.	B.Th.U./kWh	per cent	
385·0	11·7	75·92	1·001	0·993	1·018	0·973	1·02	75·63	1231·4	14407·4	23·74	Installed in 1913
408·0	10·84	77·30	0·999	0·994	1·005	0·980	1·02	77·50	1247·3	13520·7	25·29	Installed in 1913
403·0	11·82	71·80	1·003	0·991	0·999	0·981	1·02	72·26	1241·2	14671·0	23·31	Installed in 1915
461·0	10·3	72·03	0·994	1·010	0·999	0·994	1·02	70·83	1325·9	13656·8	25·04	Installed in 1916
462·4	10·04	73·66	0·993	1·009	0·998	0·995	1·02	72·63	1322·3	13275·9	25·76	Installed in 1917
460·8	10·14	73·20	0·995	1·012	1·002	0·993	1·02	71·66	1331·2	13498·4	25·34	Installed in 1918
456·0	10·28	72·96	0·994	1·009	1·000	0·994	1·02	71·78	1316·9	13537·7	25·26	Installed in 1919
462·2	10·04	73·7	0·995	1·011	0·998	0·995	1·02	72·36	1327·8	13331·1	25·65	Installed in 1920
421·2	10·8	75·20	1·003	1·004	1·001	0·999	1·02	73·23	1283·4	13860·7	24·67	Installed in 1920

I do not propose to enter into the relative costs of turbines with single- and two-cylinder constructions. The statement that in Mr. Carnegie's experience the latter design has not been found too expensive to build as his firm would not otherwise have been able to sell it, does not affect the issue.

Disc vibrations.—Mr. Carnegie's remark that discs for use in reaction turbines are not subject to vibration may apply to some makes, but it does not apply to turbines of the reaction type such as that shown in Fig. 6 of the paper. Having in mind that throughout the paper I have recommended low peripheral speeds and higher factors of safety in large turbines than have hitherto been used in turbines of smaller capacities, there is not much force in Mr. Carnegie's remark that makers of impulse turbines are anxious to make their turbines cheap by the adoption of high peripheral speeds.

Intermediate stays for moving blades.—Mr. Carnegie thinks that, because the velocity of steam with reaction blades averages only about one-third of that through impulse blades, he can afford to take more liberties with the steam without incurring appreciable losses. He neglects the fact that if the velocity is smaller in reaction turbines it can be obtained only by the use of a correspondingly larger number of stages. Consequently those " liberties " have to be taken more often and the total loss would be the same. But Mr. Carnegie omits to state that in reaction turbines the binding strips are usually provided on both moving and stationary blades, whereas in impulse turbines the stationary blades, where the steam velocity is highest, are not provided with these obstructions. The losses resulting from the use of binding strips in reaction turbines would therefore be twice as great. In addition, in actual practice the blades usually provided in reaction turbines are very light and, in order to obtain the required rigidity, it is necessary to employ binding strips on blades of relatively short length, and in the longer blades two or even three such strips are required. In impulse turbines the moving blades are usually much more substantial than in reaction turbines and, consequently, intermediate lashing is necessary only in the last few rows of blades of large turbines. For this reason, the effect of obstructions resulting from the use of intermediate stays in impulse turbines is probably only one-tenth of that existing in reaction turbines.

It may be inferred from Mr. Carnegie's remarks that for a given heat-drop per stage the average steam velocity in the moving blades of reaction turbines is smaller than that in the case of impulse blades. This is, however, clearly not the case, as is shown by the figures given in Table G below.

TABLE G.

Comparative Steam Velocities in Moving Blades of Reaction and Impulse Stages.

Item No.	1	2	3	4	5
			Impulse Stages	Reaction Stages	
1	Heat-drop per stage	B.Th.U. per lb.	20	20	20
2	Blade speed (u)	Ft. per sec.	450	450	550
3	u/c_0	—	0·45	0·45	0·55
4	u/c_1 (c_1 = actual steam velocity in stationary blades)	—	0·473	0·610	0·780
5	Steam velocity at inlet of moving blades	Ft. per sec.	535	345	259
6	Steam velocity at outlet of moving blades	Ft. per sec.	497	738	705
7	Energy corresponding to inlet velocity	B.Th.U. per lb.	5·7	2·37	1·34
8	Energy corresponding to outlet velocity	B.Th.U. per lb.	4·93	10·9	9·9
9	Average energy of steam velocity in moving blades	B.Th.U. per lb.	5·31	6·63	5·62
10	Average velocity in moving blades	Ft. per sec.	517	577	531

For the same heat-drop and blade speed the highest velocity in reaction blading is nearly 40 per cent larger than that in the moving blades of an impulse turbine, and the corresponding energy, which is a measure of relative losses, is nearly twice as great. Even the average velocity (item 10) is higher in reaction blades. The conditions are improved slightly by the use of a higher blade speed in the reaction turbine for the same heat drop, as shown in column 5. For an increase in peripheral speed of 22 per cent the velocities are reduced by roughly 5 per cent, but they are still larger than those which exist in impulse blades. Mr. Carnegie thinks that the effect of the binding strips on the flow of steam is reduced considerably by its insertion at the inlet edge of the blade. I do not agree with this contention. An obstruction immediately in front of the throat of a nozzle causes eddies, resulting in losses, and these losses may exceed those which would occur if the lashing were provided at the outlet side of the blade. The practice of providing an intermediate support at the side of the blade is bad practice from the point of view of strength, because the centrifugal stresses in the weakened blade are increased not only by the reduction in cross-sectional area, but also to a much greater extent by the additional bending stresses

resulting from the unsymmetrical loading. For this reason the method recommended by Mr. Carnegie cannot be applied with safety to blades of even moderate length, and it becomes necessary to provide intermediate strips nearer the centre of the blade unless the method described in the paper be adopted, in which no weakening of the blade occurs and which can be made in such a way that the additional losses are small.

3 000-versus 1 500-r.p.m. turbines.—Mr. Chattock is inclined to lament the direction in which turbine design is progressing and thinks that for large turbines a speed of 1 500 r.p.m. only need be considered; moreover, he seems to be under the impression that a 3 000-r.p.m. turbine is essentially a less reliable machine. In the paper I have endeavoured to state a case for these machines even for fairly large power stations. Their advantages from the point of view of reliability over the 1 500-r.p.m. turbine are quite considerable for the following reasons:

(1) (*a*) Forgings are smaller;
(*b*) The homogeneity of the material can be better secured;
(*c*) Internal forging stresses can more easily be eliminated.

(2) On account of the smaller masses and dimensions the warming up of the turbine is a more simple propo ition in the case of the 3 000-r.p.m. machine, and, consequently, the possibility of difficulties resulting from stresses or distortion due to uneven distribution of temperature is considerably reduced.

Reliability.—I fully agree with Mr. Chattock that reliability is of primary importance and that designers should go to any reasonable cost to attain it. The turbine can be made more reliable by increasing the thicknesses of the discs and diaphragms, and by increasing the clearance between them. Thicker discs and diaphragms increase the cost, not only because they are heavier individually, but also because the length of the turbine is increased, and, consequently, if the critical speed is to be above the running speed, the weights of the shaft and the casting must be increased to a similar extent. The effect of thicker discs and diaphragms is therefore cumulative and causes a relatively large increase in the cost of the turbine, particularly in the case of 1 500-r.p.m. machines. This, however, must not influence the designer if reliability is at stake. The need for reliability outweighs the constructional advantages which may result from the use of thinner discs.

Higher steam pressures.—Higher pressures involve increased wetness in the low-pressure stages of the turbines, but, with the high temperatures usually adopted in connection with higher pressures, I do not consider that increased liability to erosion will be a deciding factor, having in mind that turbines have been running for years with steam considerably wetter than would be obtained under modern conditions. It is well, however, to keep this fact in mind, and to arrange for the proper draining of the water from every stage of the turbine. Further, it is advisable to construct of the material best suited to withstand erosion those stages in which the density of moisture is a maximum. The danger of erosion is considerably decreased if care be taken that no CO_2 is present in the steam, since the acid which this forms becomes active when the steam condenses.

Critical speed.—I agree with Mr. Chattock that the risk involved in running the turbine through the critical speed, and the practical certainty of the gland clearances being increased should the turbine then start to vibrate, outweigh any possible constructional advantages obtained from the flexible shaft. This does not apply with the same force to the turbo-alternator, however, as the clearances are large and there is no possibility of the rotor touching the stator when running through the critical speed. In fact, all large high-speed alternators are designed to run through the critical speed.

Bearing temperatures.—There is much to be said for Mr. Chattock's suggestion as to the measurement of the actual temperature in the bearing metal, but in the case cited, where the trouble was due to bad fitting of the bearing, it is quite possible that only a small part of the bearing metal had heated up before the actual failures occurred, and the measurement suggested would not necessarily have given an earlier warning. The provision of two thrust bearings has the disadvantage that if the first fails it is not quite certain that the second will work satisfactorily when it is called upon to take the load, owing to the difficulty of testing it periodically. Personally, I prefer a thrust bearing of the Michell type of double the standard size and with a duplicate supply of oil. In this case the bearing would always be in operation and it would be possible to ascertain whether it was working satisfactorily or not. In addition, it is not likely that the failure of one part would affect the others. I hardly think that it would be practicable to pass the whole of the oil in circulation through a filtration system on its way to the bearing. It is, of course, established practice to provide strainers in the circulation system, and these, in conjunction with oil filters arranged for a partial filtration system, should be all that is necessary to keep the oil in good condition.

Improvements with higher steam pressures.—In Part II of the paper I have given the improvements in heat consumption possible with higher pressures, making allowance for the variation in the correction for: (A) wetness and (B) the reheat factor. These can be determined accurately on the basis stated in the paper. I have further indicated that for a given turbine the improvement is affected by: (C) the decrease in the velocity ratio u/c_0 with higher pressures, and (D) by increased leakage and disc friction losses with higher pressures. These additional corrections depend to a great extent on the output of the turbine at a given speed, and the figures which have been given in the paper as rough estimates apply only to large turbines, i.e. such turbines as are now being used in modern power-station practice.

Mr. Guy rightly points out that these additional corrections vary considerably with the output at a given speed and that, in fact, the improvement possible with increase in pressure largely depends on the output. He shows that the figures given in the paper apply to outputs of approximately 15 000 kW at 3 000 r.p.m., and approximately 30 000 kW at 1 500 r.p.m. I fully agree with his conclusions in this respect. The information

which he gives in connection with this matter is a valuable contribution to the paper and deserves special consideration.

Reliability of impulse turbines.—Mr. Fedden seems to have come to the conclusion that an impulse turbine is bound to wreck itself sooner or later, and that in order to prevent a catastrophe it will be necessary to have it re-bladed after a certain interval. It is not clear why he should single out the impulse turbine as being particularly addicted to complaints which lead to an untimely end. If blades are liable to vibrate in synchronism with some disturbing force they will break sooner or later, but Mr. Fedden does not seem to realize that it is possible to make impulse blades so strong that they will not vibrate. I have fully explained in the paper how this can be done and, realizing how important it is, I have recommended that it should be done. I have also pointed out that discs must be made so that they will not vibrate under any conditions, not even under change of load. Such rigidity is not difficult to obtain by the use of moderate peripheral speeds, and I have recommended that designers should, for the time being, keep well within safe limits. Mr. Fedden says that the reaction type of blading usually produces the best results in steam consumption, but he does not give any results to substantiate his opinion. He seems to be influenced in this respect by a comparison between the results obtained with some Parsons turbines in his power station, which are designed for the proper steam conditions and rated capacity, and those obtained with an impulse turbine which he bought during the war, designed for an output of 6 000 kW and rated up to 9 000 kW to meet urgent war requirements. This rating-up naturally resulted in a reduction of efficiency at the increased rating.

Mr. Fedden infers that my paper was written with a view to threatening turbine users with catastrophes in connection with impulse turbines. It is disappointing to see my remarks on the present turbine position misconstrued to such an extent, and his remarks make one doubt whether it is advisable to discuss operation difficulties in public. At any rate, they have done much to prevent an open and frank discussion of the kind contemplated in the paper.

Leaving losses.—Mr. Jude expresses surprise at the great amount of attention I have paid to the question of leaving losses. He says that these losses are of the order of only 3 per cent, and he suggests that it would have been better if I had given a little more attention to the other losses. I agree with him that these other losses deserve a good deal of attention but, unfortunately, most of them are hidden in the blading efficiency and, as I have explained in the paper, they are very inaccessible. For this reason, they are of little interest to the engineer purchasing the turbine. The leaving losses, however, are of considerable interest to him because the leaving area largely controls the rating of the turbine and, consequently, its cost per kilowatt. It is a simple matter for the purchaser to compare the different types of turbines on the basis of the leaving area, and he has the satisfaction of knowing that, with a reasonable leaving area, he obtains a reasonable efficiency from the exhaust end of the turbine. The leaving area of the turbine can be compared with the size of the low-pressure cylinder of a quadruple-expansion engine. If the low-pressure cylinder is too small it means that expansion is not complete. The same applies to the leaving area of the turbine, but with this difference: in the steam engine, large low-pressure cylinders reduce the mechanical efficiency and increase the losses due to condensation, whereas in the turbine these losses are practically unaffected by the size of the leaving area, unless it is found necessary to introduce a two-cylinder design. It is largely the inability of the large steam engine to utilize higher vacua that has "killed" it, and it will be the ability of any particular type of turbine to utilize high vacua that will count in future turbine development, in spite of Mr. Jude's remarks, which amount to a recommendation to disregard this fundamental question. Mr. Jude presents some figures for blade heights for a 20 000-kW machine, but he omits to give the most vital information on which his calculations are based, i.e. the vacuum, which cannot be 29 inches although one is led to take for granted that this is the case. His remarks on this subject are therefore not particularly illuminating. I should like to know what he would do if the rating were 25 000 kW or more. His remarks on the multi-exhaust design are based on the idea that there are idle blades and obstructions, and I have already explained that these do not exist.

Steam-chest construction.—Mr. Jude also criticizes the steam-chest construction shown in Fig. 33. He considers that the leakage of the governor valves as shown will be excessive. I should like to emphasize again that, in my opinion, the advantage of nozzle cut-out governing as compared with throttle governing is due not so much to the improved consumption at light loads as to the possibility of avoiding by-passing steam to a lower stage for overloads. I do not consider that the improved consumption at light loads is of great importance, as large turbines will generally be run at the economical load or even above. It is important, however, that the by-passing of steam to a lower stage should be avoided, particularly in view of the tendency to use higher pressures and temperatures. From that point of view, the steam chest shown in Fig. 33 is simply a convenient arrangement for admitting steam to the various nozzle boxes, with the exception of the overload valve which, of course, is essential for admitting steam to an additional set of nozzles, and this valve should be reasonably tight when closed. As this valve is relatively small, having to deal only with the quantity of steam required for the excess load, the possible leakage here is also relatively small. It is relatively much smaller than the possible leakage of a by-pass valve, which has to be made large enough to pass practically the full quantity of steam for the maximum load. From the point of view of steam economy the importance of having an absolutely tight nozzle control valve is therefore not so great as it would be in the case of a by-pass valve. There is, however, one condition which governor valves must fulfil, i.e. they must be able to control the turbine at no load; they must prevent the turbine from running away with the highest

possible vacuum and the highest pressure at no load. It is obvious that if the leakage on valves of the type under discussion was even one-quarter of that which Mr. Jude suggests, the turbine could not meet this condition. Actually, the governor valve shown has exceeded our expectations in this respect, not only when running at light loads, but also when the turbine is running by itself with the alternator disconnected. Careful measurements on steam chests showed the leakage to be very small and within the limits customary with the earlier type of valve. In actual practice, therefore, the fear expressed by Mr. Jude is unfounded. The losses he mentions, i.e. $\frac{1}{2}$ per cent at full load, would correspond in the turbine illustrated in Fig. 14, and with the capacity and steam conditions cited by him, to an actual steam leakage through the relatively small overload valve of about 7 per cent of the total quantity of steam, or 28 per cent of the maximum quantity of steam passing through that valve which is calculated to pass the extra steam for 25 per cent above full load. Actual measurement of the steam leakage through this valve showed a leakage of only 1 000 lb. per hour, which is less than one-tenth of that calculated by Mr. Jude. His other figures are, of course, similarly out of proportion. He says that the size of these valves is out of proportion to that of the steam pipe, but he does not say whether he considers it too small or too large, nor does he give any reasons for his statement. The total area through the three valves, when fully open, is roughly equal to the area of the 18-inch steam pipe, and I contend that this is reasonable.

Nozzle boxes and nozzle governing.—Mr. Jude cannot understand why nozzle-box design and nozzle cut-out governing should be associated with a pure Rateau type of turbine. The explanation is, of course, quite elementary. As Mr. Guy has shown in his paper read before the Manchester Association of Engineers (quoted on p. 577 of the present paper), the leaving loss of the first stage may be so small that it would not pay for full-load conditions to provide a second row of blades, thus converting it into a two-row velocity wheel. There is no more difficulty in spreading the steam uniformly over the whole of the second diaphragm than there would be if a velocity wheel were used as a first stage. Perhaps the following steam-consumption figures will make it clear why in some cases it is preferable to use a single row of blades for the first wheel.

		Turbine with Velocity Wheel	Turbine with Velocity Wheel replaced by three Rateau Stages
		lb. per kWh	lb. per kWh
Full-load consumption ..		10·0	9·85
Half-load consumption	throttle governing	11·0	10·85
	nozzle cut-out governing	10·6	10·6

In the first case, for a turbine with a velocity wheel the full-load consumption works out at 10 lb. per kWh. The half-load consumption, allowing for throttle governing, is 10 per cent higher, but by introducing nozzle cut-out governing it is reduced by about 4 per cent to 10·6 lb. per kWh. On the other hand, it is found that the full-load consumption for the particular steam conditions and capacity can be improved upon by about 1·5 per cent by replacing the velocity wheel by three Rateau stages, the full-load consumption being then 9·85 lb. per kWh. The half-load consumption with throttle governing would again be 10 per cent higher, but by introducing nozzle cut-out governing this can be reduced by $2\frac{1}{2}$ per cent (as compared with 4 per cent in the case of the turbine with a velocity wheel) and the steam consumption at half load is approximately the same as that of the turbine with a velocity wheel. Whilst a pure Rateau turbine will be slightly more expensive, it pays to adopt it in cases where the full-load consumption is of particular importance, in spite of the fact that there is practically no difference in the half-load consumption. In view of the above, it is difficult to understand Mr. Jude's prejudices against the adoption of nozzle cut-out governing even with a pure Rateau type of turbine.

Diaphragms.—I must disclaim responsibility for certain statements which Mr. Jude attributes to me, but which are nowhere to be found in the paper. It is evident that my remarks in connection with diaphragms apply whether the latter are considered as a plate or as a beam construction. There is a good deal to be said for the diaphragm construction shown by Mr. Jude and, in fact, the author's firm use a similar construction for the high-pressure end. A "cast-in" diaphragm is, in my opinion, quite satisfactory for the low-pressure end, and preferable to the more complicated built-up construction which, having in mind that it consists of relatively small blades of long dimensions, is more subject to vibration than a solid diaphragm.

Straddle blades.—Mr. Jude's criticism of the straddle blade is not in accord with actual experience, which is, after all, the best criterion. For long blades the fastening can easily be made sufficiently rigid by the use of the double-fork construction shown in Fig. 27. Mr. Jude forgets that a similar construction is adopted in all boiler work, and his fear that cracks may start from the pinholes is unfounded. On the other hand, the T-groove construction which he recommends involves sharp corners in which the static stresses are usually much higher than those calculated, and experience has shown that this construction is inherently weak.

Standard vacuum.—Mr. Jude misunderstands my recommendation to adopt a standard vacuum of 29 inches when he infers that it is proposed that all large turbines should be designed for such a vacuum. I proposed this figure as a standard for the purpose of comparison of turbine performances, and the vacua given in Table 22 clearly show that 29 inches is adopted in the majority of large turbines. I fully agree with his remarks that large turbines are public rather than private ventures, and it is exactly for this reason that

I have paid so much attention to the development of large and reliable turbines.

Disc vibrations.—Mr. Kaula's experience with disc vibrations appears at first sight to be similar to some experiences I had with turbines in 1910. In these cases, however, the troubles were proved to be due to the action of the steam, which was admitted to the discs in two or four segments and, in all cases, they were cured by changing the distribution of admission without changing the disc. In Mr. Kaula's case it is difficult to see how vibration of the kind indicated should be started and maintained by an impulse acting on the shaft in the direction of its axis. With such a force one would expect a vibration of an umbrella shape and not of the segmental type in space. For these reasons, the explanation which he gives as to the cause of the difficulty does not appear to me to be convincing.

Improvements in efficiency.—I am fully aware of the development referred to in the opening remarks of Mr. Pochobradsky, but I have purposely omitted to mention this in the paper as I believe too much importance is being attached to blading efficiency which, as has been shown, increases slightly with higher steam speeds for a given size of nozzle. There are a great number of other factors which have to be considered before a conclusion can be arrived at with regard to the best efficiency of the whole turbine. I have dealt in great detail with possible improvements in the heat consumption of the turbine by means of feed heating, and Mr. Pochobradsky's remarks on the subject, which seem to infer that I do not appreciate the importance of this point, are not justified, having in mind that even in this paper it has been emphasized more than once.

High peripheral speeds.—My opinion that turbine development has reached a critical stage does not indicate that all the troubles listed in Table 23 are of such a nature that they cannot easily be overcome. In fact, I have clearly shown that all troubles, including those resulting from disc vibrations, can be overcome, and I have indicated the means by which this can be accomplished. My opinion referred to above is based on the present trend of development towards higher peripheral speeds. Such development still persists in many quarters in spite of the danger of disc vibrations with which it is accompanied. The difficulties referred to are not overcome by simply saying that "good design and suitable material eliminate all difficulties." I certainly think that further increases in peripheral speeds are undesirable until the question of disc vibration resulting from the use of these higher peripheral speeds has been fully cleared up.

Blading efficiency with high steam pressures.—Mr. Pochobradsky's remarks with regard to Part II of the paper must have been made under a misapprehension. The data given there are based on the heat-drop, the heat content, the reheat factor, and the wetness correction. Of these, only the last-named is based on test-results of turbines, and is an alternative expression for the superheat correction. This, however, is sufficiently well known and does not apply only to turbines of one make. Rough estimates only were given of the influence of other factors on the thermal efficiency of the turbine. I do not think that the variation in the blading efficiency with higher steam pressure is of the same order as the influence of these other factors which have been dealt with in detail by Mr. Guy.

Large leaving areas with short blades.—Mr. Pochobradsky supports his contention that large leaving areas are possible with relatively short blades, by stating that Messrs. Fraser and Chalmers have obtained with their design a leaving area of 17·7 sq. ft. at 3 000 r.p.m., which is equivalent to 71 sq. ft. at 1 500 r.p.m. Wheels with this leaving area have given considerable trouble in the United States. In view of this, his assurance is not convincing in the absence of any data as to how this leaving area is obtained, or as to running experience. With a ratio of blade length to mean diameter of 1 : 5 this leaving area would involve a mean peripheral speed of 840 ft. per sec. and a tip speed of 1 000 ft. per sec., which is certainly on the large side. The fact that the blade is relatively shorter when using a disc of large diameter has not such a great effect on the reduction of vibration as a rigid support of the blade which is obtained with a relatively small disc in conjunction with relatively long blades would have.

Double-flow versus multi-exhaust turbines.—Mr. Pochobradsky is of the opinion that it is a simple matter to arrange the last stages for double or treble flow, but he does not indicate how this can be done. All the known methods of arranging these various flows are complicated from the point of view of the steam flow, and expensive because they increase the overall length to such an extent as to necessitate the two-cylinder construction. His adverse criticism of the multi-exhaust design is based on the idea that certain of the blades are idle. I have pointed out that this is not the case, and therefore his argument breaks down. He also exaggerates the leakage losses through the axial clearance spaces. Most of his remarks are based on an exaggeration of the relatively small additional losses and additional cost, and on an under-estimation of the advantages of the multi-exhaust design.

Development of high-speed machines.—I agree with Mr. Field that the necessity for the development of large low-speed machines does not apply to this country as much as to the United States, and that the development of the high-speed machine of a moderate capacity has already affected power-station practice in Europe.

Considerations for future turbine development.—I agree with Professor Stodola that the design of blading used in the multi-exhaust turbine is capable of higher peripheral speeds than those indicated in the paper, and turbines with mean peripheral speeds of 200 m per sec. (650 ft. per sec.) are at present running satisfactorily. I believe, however, that for capacities corresponding to an alternator rating of 20 000 kVA at 3 000 r.p.m. it is better to provide more than two exhaust rows for a vacuum of 29 inches, because the cost of an additional exhaust port is relatively small compared with the reduction which it effects in the

extent of the leaving losses. I doubt whether it is advisable to adopt the relatively higher peripheral speeds which have been proposed for the high-pressure stages in impulse turbines on the basis of improved efficiency with steam speeds higher than the velocity of sound. Other considerations, such as a disc friction and increased friction in the nozzles and blades resulting from the smaller steam passages in connection with large diameters, operate in the other direction, and it is possible that the effect of these additional factors will outweigh the improvement possible with a nozzle of given size with increased steam velocities. In addition, experience shows that a small number of wheels of large diameter are more expensive for the same factor of safety than a larger number of wheels of smaller diameter. This is partly due to the necessity for designing the high-speed discs from the point of view of vibration. I agree that in starting the turbine the duration of the puff of steam should be kept as short as possible, and my experience is that there is no difficulty in doing this.

Reliability of 3 000-r.p.m. machines.—Mr. Samuelson insinuates that, in recommending 3 000-r.p.m. turbines for relatively large capacities, first cost had received first consideration. He would be justified in doing this if increased capacity had to be obtained at the expense of higher peripheral speeds and lower factor of safety, and his remarks obviously do not apply to the multi-exhaust turbine, in which, even for relatively large capacities, the peripheral speed is kept moderately low. Mr. Samuelson defends the 1 500-r.p.m. machine for relatively small outputs on the ground that the higher-speed machine has the serious drawback that it cannot be used for 25-period supply, which is the standard frequency in the Glasgow, Birmingham, and other districts. Whilst this is unfortunate for the manufacturer from the point of view of standardization, there is no reason why the user of 50-period machines, who is in the large majority, should suffer for this lack of standardization of electric supply. Mr. Samuelson mentions the great difficulties in designing alternators for 20 000 kVA at 3 000 r.p.m., but our experience shows that the difficulties, if they exist, have been overcome. The arguments which he puts forward in favour of moderate-sized turbines for this country are quite sound, and the factors mentioned operate in the direction of increased application of 3 000-r.p.m. machines, even for fairly large power stations.

Mr. Samuelson is of the opinion that it is no more difficult to provide against vibration of the turbine discs at 1 500 r.p.m. than at 3 000 r.p.m. This requirement could be met if in the 1 500-r.p.m. rotor all the dimensions were double those of a 3 000-r.p.m. rotor. This would mean that the rotor of a low-speed machine would be 8 times heavier, and the cost would be increased in proportion. It is principally the endeavour to keep the rotors of low-speed machines as light as possible that is responsible for increased danger of vibration.

Disc vibrations.—Mr. Samuelson's remarks on the research work done at Schenectady bring out one further point in connection with this vibration, which has not been referred to in the paper, i.e. that it depends on the pressure in the atmosphere surrounding the disc. It is obvious that this fact further complicates the whole problem of disc vibration which, as shown in the paper, is already very complicated. It may be of interest here to summarize the various facts which affect vibration.

(1) The disc may vibrate in various ways:—

(*a*) Umbrella-shaped vibration produced by axial disturbances.
(*b*) Segmental vibration with two or more nodal diameters stationary in space.
(*c*) Segmental vibration with two or more nodal diameters rotating with shaft.

(2) Some of these various forms of vibration may exist at the same time.

(3) The periodicity of vibration of each disc depends:—

(*a*) On the shape of the disc and the weight of the blades.
(*b*) On the speed at which the disc is rotating.
(*c*) On the forging stresses, which it is impossible to determine accurately by means of calculation.
(*d*) On the distribution of temperature over the disc.

(4) The vibration may be started by a small impulse of force or forces in synchronism with the various periodicities.

(5) According to Mr. Samuelson the vibration may be started, in the absence of any external forces, by the friction of the disc on the surrounding atmosphere.

In view of this comprehensive list I do not share Mr. Samuelson's opinion that the difficulties can easily be overcome, and that slight increases in the thickness of the web and outer rim of the wheel would raise the critical speed sufficiently to prevent danger. This may apply to any particular disc of which the critical periodicities of vibration are known, but we are not sure that a similar disc made to the same drawing will have the same characteristics, having in mind that the dimensions may not be exactly the same and that the forging stresses may be different. I still maintain my opinion that the designer will do well to keep within safe limits.

High leaving velocities.—I do not agree with Mr. Samuelson that the high leaving velocity tends to reduce the pressure-drop between the last row of blades and the condenser. Unless the steam is carefully guided, a high leaving velocity creates eddies which cause additional losses, with the result that any possible benefit which may be obtained from the leaving energy is outweighed by the additional eddy losses.

North-Western Centre, at Manchester, 12 April, 1921.

Professor G. Stoney: I think that the author is quite correct in stating that the critical speed of the turbine should be above the running speed. As he remarks, we must consider the increase in steam pressure, temperature and vacuum, as well as the increase in the size of the turbine which has occurred in the past few years. The temperature is going up steadily, and may continue to do so until it is limited by the properties of the material used in the superheater. The vacuum has steadily improved, and we are now coming to the point where the temperature of the cooling water is the limit, and I do not think we shall get a much better vacuum than we do at present. Of course, these mean that the factor which Parsons called K, and which the author calls ϵ, has to be increased, since it has a direct bearing upon the velocity ratio and the British thermal units available. I think the author's rule for the factor of safety, given on page 578, is a very safe one. What is really needed is an easy method of measuring elastic limit. Professor Dalby and others have devised beautiful optical methods, but I am afraid that none of them is really commercial. I quite agree with the author that four-bearing designs are very much more satisfactory than three-bearing designs as regards vibration, but I am in favour of flexible couplings between the turbine and the alternator, although many machines with rigid couplings are running quite satisfactorily. This is more especially the case where there are more than two shafts running in line, e.g. with tandem alternators or tandem steam turbines. The question of the vibration of turbine discs is most difficult, and the mathematical solution of it is not very satisfactory. Experiments on turbine discs run up to speed and above speed are necessary, and for these a bomb-proof chamber would be required. In Table 23 the author mentions two or three failures of discs that have vibrated on four nodal diameters. I have experimented with rubber discs $7\frac{1}{2}$ inches in diameter, and always found that they vibrated on two nodal diameters. The author is quite right in thinking that there is a great future for the 3 000-r.p.m. turbine in large power schemes. As an output of something like 15 000 kW can now be obtained at 3 000 r.p.m., the advantage of adopting the lower speed of 1 500 r.p.m. is negligible. On the other hand, there is the immense advantage of smaller forgings and castings, and it is difficult to get really large forgings that are reliable. High-grade material such as chrome-nickel steel is suitable for such small machines as aeroplane engines and motor-car engines, but its advisability for very large forgings is very doubtful. I have always considered that an impulse turbine would be improved if the shrouding were not only riveted but also brazed on. The amount of damage to the shape of the blade is infinitesimal, and I do not think the brazing could possibly affect the efficiency if it were properly done. The analysis which the author gives of the results of variation of the economical rating under various conditions is most valuable. An interesting result to me is that although the exhaust losses for a given quantity of steam vary approximately inversely as the square of the back pressure, yet when the increased economical rating, which varies as the square root of the back pressure, is taken into account these exhaust losses only increase inversely as the square root of the back pressure. In Table 23 the author gives a most candid and complete account of various accidents. I think that is an exceedingly good policy. If turbine makers had been more candid about various breakdowns that have occurred and their causes it would have greatly increased the rate at which progress has been made. That table shows that too high stresses must not be employed and that the disc must be made stiff enough not to vibrate. In that connection the ratio of blade height to mean diameter is most important, because it is comparatively easy to make a blade to withstand centrifugal force, but it is much more difficult to make a disc to do so. I feel that many troubles might be avoided if two-cylinder designs were more largely adopted for very large sizes, entirely apart from the question of leaving losses. The latter, for the most ordinary sizes, can be quite well dealt with in a single design by the author's ingenious multi-exhaust, which I consider to be one of the most radical improvements in turbine design of late years. Nowadays we have a temperature range from 750° F. to 76° F., so that between the temperature of the steam and that of the condenser there is a difference of from 650 to 700 degrees, which is a great range to have in a single cylinder, especially when it is large. If the cylinder be divided into two parts, with equal power in each part, about two-thirds of that temperature range is confined to the small high-pressure cylinder, and only about one-third is in the low-pressure cylinder, which is large. In that way it is much easier to avoid expansion troubles. One thing which should be mentioned is the method of testing a rotor to see if there is synchronous vibration, by putting it near another machine running at the speed at which it is intended to run and seeing whether vibration occurs. A rotor could be mounted on flexible trestles and made to vibrate by a small motor having balance weights attached to the shaft. I would suggest that this method should be tried not only at the running speed but also at higher speeds, because it is probable that in many cases the vibration impulses on a wheel occur at a multiple of the running speed.

Mr. H. L. Guy: Referring to Fig. 30 it is not clear, from the very brief description which is given of the multi-exhaust construction, what happens in the inner tiers of blades at the low-pressure end of the machine. When the steam passes from the inner portion of the blade—which it does, as the author states, without appreciable expansion—a relatively appreciable expansion takes place in the lower part of the moving blade H. Again, a relatively appreciable expansion takes place in the steam as it passes through the lower part of the moving blade M. This effect is certainly not generally appreciated, and I would suggest that the author in his reply should give a diagram showing

the pressure and the velocity changes which take place in the inner tiers of the multi-exhaust blading. The great advantage which results from the employment of the multi-exhaust construction is that it enables one to use a smaller mean diameter for a given capacity of the exhaust end than can be employed by any alternative construction. This results in a very large number of advantages which not only improve the efficiency of the machine but very materially affect its speed and reliability. I should like to give some indication of the order of the advantages which resulted from the adoption of this method in our own practice. For that purpose we will consider machines rotating at a speed of 3 000 r.p.m. Before the author arrived at this solution of the large-output turbine problem the machines which we were building for 3 000 r.p.m. and outputs up to about 6 000 kW were, I think, typical of the machines which were being produced by other makers, not only in this country but in other parts of the world. The advances in output had been obtained by increasing the mean diameters and, therefore, the peripheral speeds of the turbines, and, as a result, difficulties were experienced which could not very well have been anticipated. We therefore remodelled that line of machines, the principal change consisting of a reduction in the mean diameter of the discs to about three-quarters of that which we had been using up to that time. This was possible with the multi-exhaust, in spite of the fact that the output with which the new machines would deal was increased to three times the output of the old ones. The immediate result of this change, with the consequent changes which have been outlined in the paper, was that the blade stresses were reduced by over 20 per cent as compared with those which had been employed by us for many years. The stresses in the discs were reduced to three-quarters of their old values; those in the diaphragm blades were reduced to two-thirds of the old values; the deflections of the diaphragms—because of their very much smaller diameter—were easily reduced to one-half of what we had commonly adopted before; and it was possible at small cost to increase appreciably the margin between the running speed and the critical speed of the turbine. It was also possible, because of the much smaller dimensions that were obtained, to make at a reasonable cost many of the parts in cast steel which had previously been made in cast iron, which enabled much higher temperatures to be employed than had formerly been the case. The discs also, because of their much smaller diameter, could be made so stiff that the danger of setting up synchronous vibrations was removed. The stresses due to the difference of temperature across a disc are very important in machines having very large discs, particularly when the turbine is being started up, or when a sudden change takes place in its condition, such as loss of vacuum and going over to atmosphere. Such stresses vary with the diameter, so that an increased safety was obtained under this head. Finally, as Professor Stoney and the author have pointed out, additional advantages accrue from the fact that, as smaller forgings are used, better results are obtained than is possible with a large forging. There are nine counts at least upon which the reliability of the machine has been materially improved. On the other hand, the efficiency is immediately improved by the adoption of smaller diameters because the disc friction and windage losses vary as a high power of the diameter, and consequently the percentage of loss under these heads is materially reduced. Although it may not be obvious at first sight, the loss in diaphragm gland leakage and cylinder gland leakage is also reduced by reducing the mean diameter of the machine. I might add that an additional gain is obtained from the fact that, if a machine is designed for the maximum desirable ratio of steam speed to peripheral speed, an improvement in efficiency is obtained by reducing the mean diameter, because the leaving loss is proportional to the peripheral speed, and is therefore reduced when the latter is reduced.

Mr. S. L. Pearce: A good deal of this paper is based upon American experience and, as I had the opportunity last autumn of visiting the States and seeing almost all the large turbine plants that the author has referred to, I can readily associate myself with his remarks in regard to the frankness with which the American engineers have disclosed all their troubles. In connection with very large turbine plants we can learn from the States a great deal of what to avoid and what to adopt. Fig. 1 shows how striking has been the growth, during the past few years, in the size of generating plants. As regards the turbine, one has only to state that between the years 1912 and 1917 the single-cylinder turbine has grown from 15 000 to 45 000 kW. That last figure is perhaps somewhat fictitious, because, as will be understood from Table 16, there has been a great deal of trouble with that machine and indeed with other machines of slightly lower ratings, namely, the 35 000-kW sets in the Chicago station. That trouble has led to a very appreciable lowering of capacity. Generally speaking, one can say that the record of single-cylinder machines in the United States has not been altogether satisfactory. There have been far too many wheel failures, and Table 16 deals with them very fully. I think that in item 20 of Table 1, which refers to the set built for the Manchester Corporation by Messrs. Richardsons Westgarth in conjunction with the Metropolitan-Vickers Company, the maximum continuous rating should be 25 000 kW. I think that, so far as the steam end is concerned, the machine in item 26 will never be built. I should like to ask the author whether he agrees with me that the following figures are a fair summing up of the position as regards the possible size to which single-cylinder machines can be carried, namely, 30 000 kW at 1 800 r.p.m., and 35 000 kW at 1 500 r.p.m. I am fairly confident that these sizes are safe with the particular type of exhaust for which the author is responsible, and I should like to know how much further he considers it is safe to go with the single-cylinder design for high vacua and temperatures before it becomes necessary to adopt two cylinders. From a consideration of Table 23 it seems to me that in practically every case the fundamental cause of breakdown is vibration. I think also it would be true to say, as the author himself states in the earlier part of the paper, that a good deal of the trouble is necessarily the result

of pioneer work and is the outcome of a race for the maximum output and the best economy at the maximum possible speed. It is very important that users and designers should come together and that there should be a frank exchange of opinion on both sides in order that the very best progress may be secured. In the United States there are numerous committees in connection with the National Electric Light Association. Not the least important is the one known as the Prime Movers Committee which regularly publishes a Bulletin. One of these Bulletins which I have seen contains a great deal of most valuable information dealing with the progress of the manufacture and operation of large turbo sets. It seems to me that in the United States nearly all the troubles with these large sets have been due to the amount of vibration set up by inaccuracy, weakness of design, or insufficient clearance. American engineers themselves are strongly of opinion that there is a third contributory cause, namely, faulty material, inasmuch as during the war they were obliged to use a great deal of material such as semi-steel in place of good cast steel. I notice one rather radical departure in the modern General Electric designs, namely, in connection with the shaft packing. Instead of fixing the labyrinth teeth to the stators or diaphragms they are now apparently made longer and mounted on the rotor. I should like to know whether the author agrees with that practice. It is claimed that shaft distortion due to overheating is eradicated, for obvious reasons. I myself formed a very definite opinion that a reaction was setting in in the United States against the single-cylinder impulse turbine for very large sets, on account of the troubles which had been experienced. One of the principal designers there, when discussing the Barton machines with me, said that he would not care to design or manufacture them to have such a large heat drop as they would have in a single cylinder and that it would be necessary to provide for double flow at the low-pressure end. He further stated that he thought the maximum temperature of the steam should never exceed 700° F. The author has referred to the 30 000-kW Ljungström machine now being built by the General Electric Company. I believe it to be the opinion of the engineers of that company that the thermal economies which are expected from that machine will not counterbalance the excessively heavy cost of construction. Tables 7 and 8 are very striking, as showing the small average size of the turbines. I gather that that arises partly from the fact that all turbines have been included, whether for power stations or industrial work. Probably if power stations alone had been taken they would have shown a higher average, although I think the percentage in favour of the impulse as against the reaction type might possibly not be quite so high if some of the smaller impulse turbines were ruled out. The American Westinghouse Company are responsible for a very large number of designs, chiefly of the reaction type, and I think we may say that that company have had less trouble than the General Electric Company. They have built turbines up to 30 000 kW rating in a single cylinder with a sort of semi-double-flow low-pressure end. But the tendency seems to be to adopt the type shown in Fig. 8, which is a purely reaction two-cylinder machine, with a double low-pressure end. For outputs larger than 35 000 kW they have adopted what is known as the triple-cylinder cross-compound type, which is really a very interesting machine. I think Mr. Stott, superintendent of the Interboro' Rapid Transit Company, was mainly responsible, in conjunction with the American Westinghouse Electric Company, for developing the design of that unit. The author gives particulars of that type under items 4 and 6 in Table 5. I suggest that, to make the table complete, he should add the plant which is now being erected at the Colefax station of the Duquesne Electric Lighting Company. That station has been built for six 60 000-kW sets of the triple-cylinder cross-compound type. I believe those turbines will show a slightly superior thermal economy to anything else that I have seen in the United States. The figures are as follows: Steam pressure 300 lb. per square inch, vacuum 29 inches. When these turbines are working together, that is to say, one high-pressure and two low-pressure, there will be an overall steam consumption of $10\frac{1}{4}$ lb.; with one high-pressure and one low-pressure, 11·2 lb.; and with two low-pressures only, 14 lb. There is a good deal to be said both for and against this particular type. I suggest that the points in favour of this triple-compound type are, first, that the separate units enable the high-pressure and low-pressure elements to be designed quite independently and therefore to the best advantage as regards speed and mechanical strength. Secondly, a slightly higher thermal efficiency can be obtained for the combination in comparison with single units of the same aggregate capacity. Thirdly—this is the point in favour of the triple compound as against the tandem—the low-pressure turbines can be run even if the high-pressure end is out of commission. Against it there is the extra capital outlay per kilowatt installed, the extra floor space and the extreme complication of working. So far as we in this country are concerned, I do not think the requirements are likely to call for 60 000-kW units, so probably the point will not arise; but I should be glad if the author in his reply would deal with the question of that particular design, because I know that he has gone into it very carefully. I may mention in passing that the Interboro' Rapid Transit Company, who were responsible for the introduction of this machine, in their latest extension have reverted to the single-shaft single-cylinder turbine. On the question of the feed water the author is optimistic. I think that, so far as the total elimination of erosion is concerned, he has gone a little too far. It is a very difficult thing to get rid of entirely. One must be perfectly certain that there is no acid in the feed water, and the CO_2 and oxygen must be eliminated. I suggest that this cannot be done completely by arranging for the feed water on a closed cycle. There are certain methods offering possibilities for securing that very desirable end which are receiving attention to-day in certain quarters, but with a closed feed-water cycle of operations alone it is not certain that corrosion will be eliminated. The author refers to the use of kerosene. I believe I am right in saying that that arrangement was first tried in this country at Manchester, and was

originally extracted from some German technical production. The preliminary Glasgow results given by the author are extremely valuable, and lead me to believe that with the improved thermal conditions at our new Barton station even better results will be obtained. The broad conclusion I have come to on British and American practice is this: that American manufacturers are not so advanced in the development of turbine engineering on sound lines as British manufacturers, nor, comparing the best plants in each case, can they show such high operating efficiencies as the latter.

Mr. J. M. Newton: I wish to confine my remarks to Part IV of the paper, in which the author shows the very great economical advantages to be gained by running turbines at the highest possible speeds. I feel there is still a certain reluctance amongst engineers, especially power-station engineers, to accept high-speed turbines. It is an old prejudice, at least as old as anything can be in connection with such a modern production as a turbine; but it may be recalled that from 1905 to 1910 the relative advantages of 750 and 1 500 r.p.m. were discussed, and in some cases the former speed was chosen. At that time most of the turbines being made in this country were of the reaction type; the impulse turbine was struggling for a place and was handicapped by inferior thermal efficiency. Shortly afterwards it took a step forward; its efficiency improved and that synchronized with the increase of the speed for a given output. This has the effect of reducing the size of a turbine for a given output, and if the same hydraulic efficiency of the blading can be preserved, the other losses—mechanical, leakage, and disc-friction losses—will be reduced, and consequently the commercial efficiency will be increased. Speaking as one whose experience has lain chiefly in the manufacture of turbines, I would express my strong preference for the high-speed turbine as being the more reliable. Other speakers have already emphasized the difficulty of making large forgings, especially those of alloy steels for high stresses. I would merely add that I have dealt with scores of discs of chrome-nickel steel, with a very slight percentage of trouble up to diameters of about 30 to 36 inches. Larger forgings of the same kind of steel have shown a very great percentage of scrapping in the shops. In one case, out of 10 discs 8 were scrapped. I hope one of the effects of this paper will be to remove the prejudice against running turbines at high speeds. If I were responsible for choosing turbines for a power station, except in the case of the largest stations I would choose 12 000- to 15 000-kW units at 3 000 r.p.m.

Mr. C. Martin: With reference to the question of discs, it is generally known that there are in this country a few test-pits in which each disc, before it is assembled on the shaft, is run up to 30 per cent above the running speed, which approximates to the critical speed of the Rateau turbine. This precaution tends to avoid breakdown, which is often attributed to faulty forgings. The multi-exhaust of the large turbine interests users to perhaps as great an extent as manufacturers. There are quite a number of different types of exhaust arrangements, each manufacturer having his own particular method of efficiently dealing with the large volumes of steam at the exhaust end of the machine. On one of the well-known makes the steam is so divided that the overall length of the blades is reduced to a minimum without affecting the efficiency of the turbine.

Mr. G. A. Juhlin: It is interesting to note from the curves given in Fig. 2, showing the growth in sizes of turbo-alternator sets, that we do not in any sense lag behind in this country. The author mentions that developments in this country were to a great extent checked by the war, and it is a fact that the actual development of a 10 000-kW turbo-alternator at 3 000 r.p.m. was commenced early in 1916, but could not be proceeded with on account of the war. The author rightly points out that no advantages can be obtained on the electrical side by the adoption of high speeds. In regard to efficiency the high-speed machine has no advantages, and in the matter of cost there is no difference between a 1 500-r.p.m. and a 3 000-r.p.m. unit. The development of the alternator has simply taken place in order to enable the turbine makers to take advantage of the lower cost and improved consumption of the turbine. The author emphasizes the advantages to be gained by the high-speed turbine, and particularly with reference to the question of forgings. The same remarks hold good for the alternator. Experience shows that it is easier to obtain sound forgings of small diameters than very large and heavy forgings. Some years ago alternator designers found it difficult to produce machines large enough to absorb the output of the turbine, but to-day the situation is reversed. It is now possible to produce alternators of sufficient capacity for any turbine that can be built, and we may confidently look forward to the manufacture of alternators of at least 25 000-kW capacity at 3 000 r.p.m. I should like to associate myself with the author's recommendation with regard to average factors of safety. This is of particular interest in connection with high-tensile material such as nickel-chrome, vanadium-steel, etc., which is used for the retaining rings on turbo rotors. When it is considered that the elastic limit of this material is about 115 000 lb. and the ultimate strength 145 000 lb., the advantages which may be obtained by adopting the average factor of safety, rather than working on the old systems of the factor of safety on the ultimate strength, will at once be realized. It is certainly more logical.

Mr. K. Baumann (*in reply*): Professor Stoney advocates the use of flexible couplings in preference to the solid couplings used by various makers. I purposely omitted any remarks on this subject in the paper, as I realize that there are a good many points in favour of flexible couplings in spite of the fact that solid couplings are giving good service in most cases. Both solid and flexible couplings require great accuracy of workmanship and of erection if additional vibrations resulting from the coupling up of two rotors are to be avoided. There is a good deal to be said for some sort of flexibility in a coupling which will enable the rotors to run as independent units, but the usual type of flexible coupling does not meet this requirement. The apparent anomaly referred to by Professor Stoney, viz. that the outlet losses of the economic rating only

vary inversely with the square root of the back pressure is easily explained by Table H, which shows in the last column the ratios of the various factors bringing about this result. Professor Stoney recommends the use of two-cylinder turbines for large units with high-temperature ranges. I agree that in the design of the turbine care must be taken to separate the high-pressure cylinder from the large exhaust as much as possible. This can be done effectively in single-cylinder constructions by arranging the exhaust on the far side of the last stage, thus removing the exhaust body as far as possible from the high-pressure cylinder, and by connecting the high-pressure end to the exhaust body in a simple and flexible manner, thus obviating stresses due to uneven distribution of temperature. I should not recommend the use of a small motor for testing turbine rotors for vibration, because it is most difficult to adjust the speed of the motor with sufficient accuracy, the inertia of the rotor body being too small. For this reason a relatively heavy rotor is preferred, the speed of which can be adjusted and held constant more easily. I agree that the turbine rotor should be tested for vibrations at higher speeds than the running speed, but at the same time it must be realized that this method of testing a rotor serves as a rough guide only, as it is impossible to produce in a stationary rotor vibration of the same kind as that which will exist in a rotating body.

To facilitate a better understanding of the multi-exhaust principle, I have prepared, as suggested by Mr. Guy, a diagram of the blading of a duplex exhaust (Fig. H) which contains the velocity diagrams obtaining in the various parts of the blading, and also a diagram (Fig. J) containing further data relating to heat drops, pressures, steam quantities and leakage losses. It will be seen that while the heat-drop through the inner tier of guide blades (G) is small, there is an appreciable expansion through the moving blades (H). The result is a reaction stage with a high percentage of reaction, as stated in the paper. The ratio of the steam velocities and mean blade speed is such that the efficiency of this stage is approximately the maximum possible efficiency. The flow of steam is simple, the angles through which the steam jet is deflected in its passage through the blades is relatively small, and consequently the possibilities of eddies ocurring are remote. As a result of these conditions the hydraulic losses in these stages are relatively small. The principal loss occurs in the moving blades (H); these form a nozzle at the inlet side, which is followed by a parallel straight passage in the intermediate part and a curved passage at the outlet of the blade. On the basis of results obtained in experiments with similar types of nozzles and blades, these losses are estimated to be not more than 10 per cent of the energy corresponding to the highest velocity obtained in these blades. For the calculation of the diagram slightly higher losses are assumed, the velocities shown being calculated on the basis that the losses are 20 per cent of the adiabatic heat drop, the remaining 80 per cent being transformed into kinetic energy. In addition, it has been assumed that 80 per cent of the leaving energy is available for work in the succeeding stage. It will be seen that the efficiency of the inner tier of blades is practically the same as that of the outer tier or that of the last row of blades. The leakage losses at A and B are respectively 1·2 and 0·8 per cent of the total steam quantity; this leakage is based on an axial clearance of 0·080 in. on a diameter of approximately 48 in. This clearance could be reduced by careful adjustment or by providing dummies with radial clearance instead of axial clearance. In the latter case, this clearance could be reduced to half that assumed for these calculations. The loss of work resulting from these leakage losses

TABLE H.

Outlet Losses for Economic Ratings at Back Pressures of 2 in. and 1 in. (Abs.); Steam Pressure 200 lb. per Sq. In. (Gauge); Superheat, 200 deg. F.

1	2	3	4	5
Item	Description	Value for a back pressure of 2 in. (abs.)	Value for a back pressure of 1 in. (abs.)	Col. 3 / Col. 4
1	Economic rating at turbine coupling, kW	41 000	28 900	1·42
2	Steam consumption at turbine coupling, lb. per kWh	11·4	10·78	1·058
3	Total steam consumption, lb. per hour	467 400	311 600	1·500
4	Dryness factor	0·915	0·908	1·008
5	Specific volume of dry steam, cub. ft. per lb. ..	309	585	0·528
6	Actual specific volume, cub. ft. per lb.	283	532	0·532
7	Total steam volume, cub. ft. per hour	$132·3 \times 10^6$	$165·8 \times 10^6$	0·798
8	Leaving velocity, ft. per sec...	622	778	0·799
9	Leaving losses, B.Th.U. per lb.	7·71	12·07	0·640
10	Outlet losses, B.Th.U. per lb.	9·77	15·25	0·640
11	Adiabatic heat-drop, B.Th.U. per lb.	388·7	423·2	0·918
12	Outlet losses in per cent of adiabatic heat-drop ..	2·52	3·60	0·700

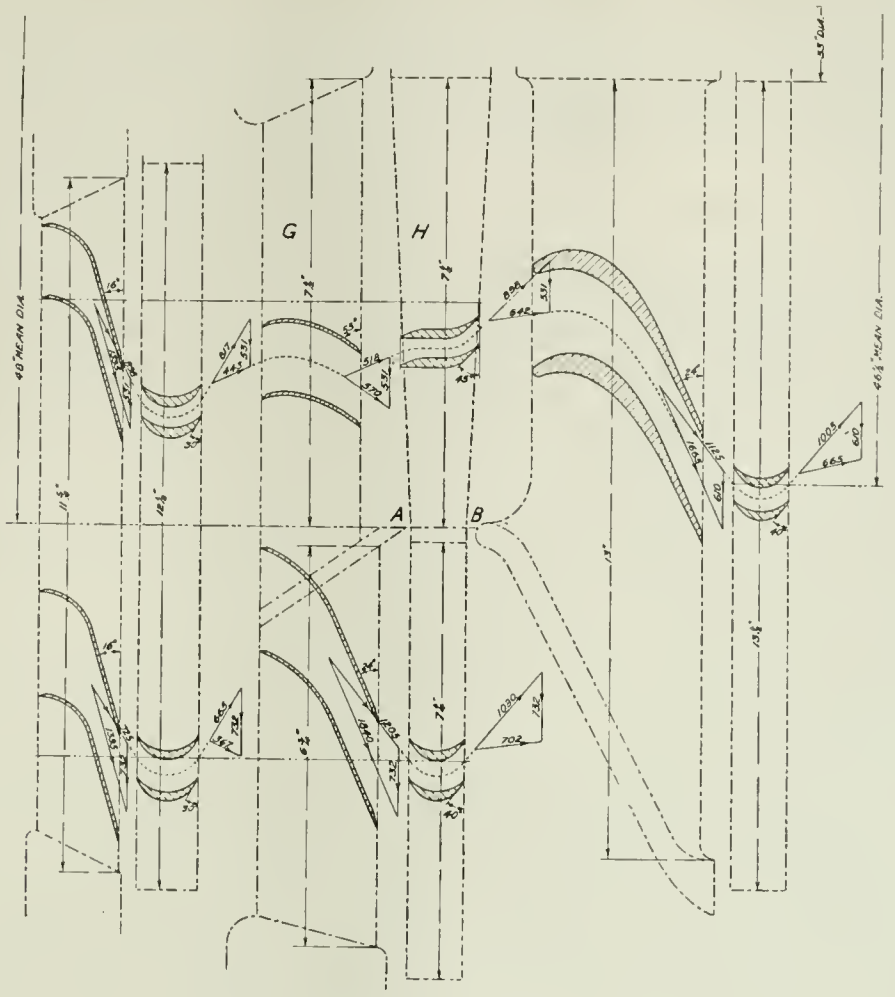


FIG. H.—Blading diagram of a duplex exhaust.

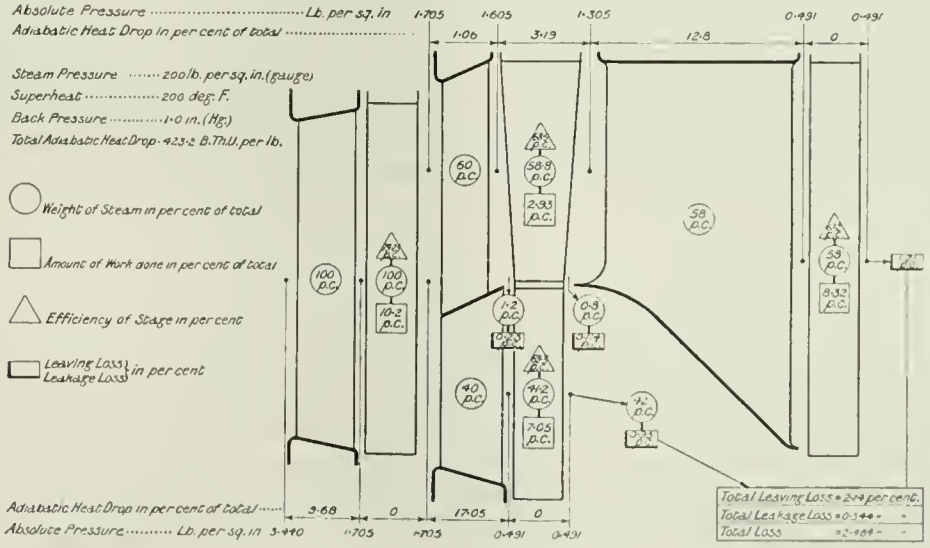


FIG. J.—Diagram of a duplex exhaust.

is 0·23 per cent and 0·114 per cent respectively, the total leakage losses being 0·344 per cent. Adding these leakage losses to the leaving losses, which are 2·14 per cent, we get a total for leaving and leakage losses of 2·484 per cent. If the total steam quantity were passed through the last row of blades, the leaving loss would be roughly:

$$\frac{1\cdot21}{0\cdot58}\left(\frac{1}{0\cdot58}\right)^2 = 2\cdot09 \times 2\cdot97 = 6\cdot23 \text{ per cent.}$$

By introducing the multi-exhaust, the leaving losses are reduced to 40 per cent of those existing in a single-flow turbine for a given rating, thereby improving the steam consumption by 3·75 per cent, which more than justifies the relatively small additional cost of this type of exhaust. On the other hand, for the same outlet losses the rating would be increased by roughly 60 per cent by the use of the duplex exhaust. This does not represent, however, the possible increase in capacity for the same overall efficiency, as with the higher capacity the losses in the high-pressure end are considerably reduced, as pointed out in the paper, and this reduction in losses outweighs the small additional leakage losses resulting from the addition of the multi-exhaust stage. Taking this into account it will be found that the increase in capacity for the same overall efficiency is considerably larger than 60 per cent, which is the figure mentioned in the paper.

I agree with the figures which Mr. Pearce suggests for the possible size to which single-cylinder machines can be carried, but I hesitate to indicate any further development, as a good deal of such development will depend on the experience gained with existing designs. In this matter I am very much in sympathy with the remarks made by Mr. B. A. Behrend in the discussion on "Present Limits of Speed and Power of Single-Shaft Steam Turbines." * These remarks are to the effect that whilst in 1906 he predicted that the power station of the future would contain as many 20 000-kW units as the power station of 1906 contained 5 000-kW units, he ventured now, 12 years later, to place on record that the power station of the future would not contain the 75 000-kW and 100 000-kW units of which they had heard a good deal in the papers presented. I have referred to the special design of dummy strips introduced by the General Electric Co. in Part VII, section (1), of my paper, and I have pointed out there that these strips have the disadvantage that they cannot be replaced without taking the rotor discs off the shaft, which is, in my opinion, a very serious drawback to an otherwise satisfactory solution. The principal reason why the American designer suggested a two-cylinder machine for Barton steam conditions was probably due to the fact that in his opinion a double-flow turbine was necessary, since this in itself would involve the use of a two-cylinder construction. Further, the design introduced by the expert in question consists of wheels of small diameters at the high-pressure end. This design involves by-passing steam for overload conditions, and I quite agree that with such a design a single-cylinder construction is not advisable. The turbines which are now in operation at the Colefax station * are duplicates of those given in Table 1, item 19, and it is for this reason they were not mentioned separately in the list. I think Mr. Pearce has stated the case very clearly for the multi-cylinder machines which have been built in America, and I agree with him that, so far as this country is concerned, the requirements are not likely to call for very large turbines. In view of this it seems hardly worth while to enter into this question more fully.

I agree with Mr. Martin that running discs at 30 per cent overspeed in test-pits tends to avoid breakdown, but it will do this only to a limited extent, as it will not minimize the difficulties experienced as a result of disc vibrations, as I have already pointed out in Part VII, section (3), of the paper. Mr. Martin refers to a make of steam turbine in which the steam is divided in such a manner that the overall length of the blades is reduced to a minimum without affecting the efficiency of the turbine. At the time of reading the paper, particulars of the design to which he refers had not been published, nor does Mr. Martin give a description of it which would enable one to criticize it.

I am glad to have Mr. Juhlin's assurance that to-day the alternator designers are able to produce alternators of a greater capacity than those produced by the turbine designers, and that an alternator of 25 000-kW capacity running at 3 000 r.p.m. is quite a feasible proposition. This statement is rather a striking contrast to the opinion expressed by Mr. Samuelson during the discussion in London, which was to the effect that 10 000 kW at 3 000 r.p.m. is nearly approaching the limit where the alternator can be offered with every confidence in its reliability.

* *Transactions of the American Institute of Electrical Engineers*, 1919 vol. 38, p. 1547.

* *Power*, 1921, vol. 53, p. 537.

North-Eastern Centre, at Newcastle, 9 May, 1921.

Mr. F. W. Gardner: The point of primary interest in the paper is its bearing on the old controversy which still rages round the question of the relative merits of the two main types of turbines—impulse and reaction—and it is worth while taking note of the impression that a perusal of it must have on the reader who approaches this fundamental question with an open mind. In the opening pages we find elaborate tables comparing the total kilowatts of the impulse and reaction turbines constructed in recent years, and intended presumably to suggest a marked preference on the part of engineers for the former type, but when, near the end of the paper, we reach the section dealing with breakdowns, there is no corresponding table setting out the failures of impulse turbines as a percentage of the whole. There is, however, a list of breakdowns which includes a whole catalogue of wrecked impulse turbines—in marked contrast to the few minor failures

recorded against reaction machines, and the author states that the use of alloy steels and high peripheral speeds has received a check. But these are the very weapons by means of which the impulse turbine hoped to win the day. The reason why reaction turbines have not burst is simply that they have been able to hold their own, and more than hold their own, without resorting to the use of such special steels and highly stressed rotors. Development will, of course, go on, and with growing experience the difficulties will eventually be overcome, but the all-important point is this—and it cannot be too strongly emphasized—whatever development may come through experience in the future, the reaction turbine will benefit by it to the full and the inherent high efficiency of the blading will still remain a card in hand. I was glad to hear the author's explanation of why he favours keeping the critical speed of turbine rotors above the running speed—namely, that if the rotor has to pass through the critical speed the fine running clearances between fixed and moving parts will be permanently increased as a result of the vibration that will occur at starting up. This statement finally disposes of the claim so frequently made by advocates of the impulse turbine that this type can be run with large clearances between the blades and diaphragms without loss of efficiency. The truth is that the impulse turbine has no advantage at all over the modern reaction machine in this respect. The next striking point in the paper is the author's dislike for the tandem turbine. At the outset he belittles the performance of the tandems with two-flow exhaust by ruthlessly dividing the output by two, though the fundamental virtue of a tandem machine is its adaptability for the two-flow arrangement. Again, at a later stage, when efficiencies are under consideration, a tandem turbine is made to start off with a handicap of 2 per cent. How is this 2 per cent arrived at, and what is the justification for it? The first question the author leaves unanswered. The greater cost of the turbine is his answer to the second. But when it is desired to increase the output at a given speed beyond the maximum attainable efficiently with a single cylinder, what are the alternatives to a tandem with a two-flow low-pressure element? One alternative is the multi-exhaust described in the paper. But does not this multi-exhaust add appreciably to the length, weight and complexity of the turbine, and therefore to its cost? Undoubtedly it does, and if the tandem is to be thus handicapped in comparison with a simple single-cylinder turbine of half the output, surely a multi-exhaust should be similarly handicapped. Had the author included any multi-exhaust design in Table 22, I trust he would not have omitted to apply such a correction in estimating its comparative performance, but there is no suggestion in the paper that this should be done. Quite apart from this omission, the paper contains no evidence to show that a multi-exhaust can be as effective as a two-flow tandem in keeping down the leaving losses. The blade height that can be used with a given mean diameter and stress is greater in the tandem, as the complication of an abrupt change of section half-way up the blade is not required. Further, the multi-exhaust necessitates the introduction of a labyrinth of idle blades and guide passages, the losses in which must be quite considerable. It is to be regretted that no experimental or calculated data for these losses are given—not only at normal load, but also for other conditions of load on the machine. Their importance in this latter case must not be overlooked, for it appears that when the turbine is working at outputs below the economical, the steam jets from the guide passages will strike the backs of the idle blades shown in Fig. 30 and will be turned through a negative angle, thus involving an additional loss from shock at entry. The multi-exhaust undoubtedly marks an advance upon a single-blade exhaust, but some more concrete evidence of its actual performance is required before we can all share the author's optimism as to its efficiency. There is another reason why a tandem arrangement is advantageous. The practice of reheating the steam at the stage where the initial superheat disappears is now developing and may very soon be widely adopted for large plants. In this case the whole of the steam has to be withdrawn from the turbine and then returned for the remainder of the expansion, and the turbine therefore automatically becomes a two-cylinder design. The same applies in a lesser degree to cases—rapidly becoming the general rule—where steam is bled from the turbine at two or more points for heating the boiler feed-water, as it seems improbable that all the necessary belts for steam extraction and also a multi-exhaust could be crowded into the span of a single shaft, at all events if the exhaust were of one of the more complicated types foreshadowed in the description. The author's claim on page 607 that this design permits of a special standardization of cylinder patterns is difficult to understand. The standardization of successive sizes of both exhaust ends and cylinders is possible with almost any type of turbine, and has long been the recognized practice of manufacturers. The adoption of the multi-exhaust system can hardly lead to a reduction in the total number of stock patterns required to cover the full range of sizes built. The paper shows that a given area through the last row is best obtained by adopting a tall blade on a small disc—that is by making the ratio of the blade height to the diameter as large as possible—and in this respect the reaction turbine has an advantage over the impulse type, because the limiting value of this ratio depends finally on the possibility of providing against blade vibrations, rather than on the theoretical value of the stress in the material. Vibration is most effectively prevented by binding the blades together at frequent intervals along their length, and this can be done without appreciable loss of efficiency in reaction blades, as the velocity of the steam at the inlet edge is comparatively low with this type. Also the spread of the blades at the tips—another very important factor—can be made greater with reaction blading without serious loss of efficiency. Referring to experimental turbines the author says that he prefers to rely on test-results obtained with commercial machines. With this everyone will agree, but it seems rather unfair to dismiss the experimental turbine in this very cursory fashion. Its function is not to afford direct criteria of the overall efficiencies

obtainable in practice, but rather to enable the effect of detail improvements to be predicted. With a small turbine in the maker's works where the conditions are under full control and not subservient to the exigencies of commercial running, it is possible to repeat the conditions exactly for a succession of similar tests, and this is invaluable for such purposes as the comparison of different blade profiles, blade spacings, clearances and innumerable other points of detail. It has always been the policy of the firm with which I am associated to make the fullest use of the experimental turbine for ensuring a constant progress in the details of design, and then to apply the knowledge thus gained to the commercial machines, from the performance of which the final verdict must be derived. If any justification of such a policy is needed, surely it is to be found in the efficiencies that modern Parsons turbines have attained. The section of the paper dealing with the correction factors which have to be applied to reduce the actual steam conditions of a test to the design standards has been very carefully and thoroughly worked out. A comparison of the new curve given for the vacuum correction, with that given in the author's earlier paper, is interesting, and shows clearly how the leaving losses have increased in importance with the increase in the size of turbo-alternators since the earlier paper was written. Such curves have been in use by designers for a long time, but have not, I think, previously been published, and the author's new curve should be of much service for general purposes, though in supplying correction charts for large plants the manufacturer will prefer to treat each case individually, since the curve given is applicable only to plants rated strictly in accordance with the author's definition of economic rating, and it is hardly possible to include accurately in a single curve all the widely differing cases occurring in practice. The author has put an immense amount of work into the portion of the paper dealing with economic rating and into establishing rules for comparison of different machines on the basis of a common standard. The treatment of this part of the subject is interesting and instructive. Such investigations, however, are very limited in their practical application, and based as they necessarily are on somewhat arbitrary assumptions they may, if not properly interpreted, lead to very misleading conclusions. Such laws as that the output varies as the square root of the back pressure, and even the better-known law that the output varies inversely as the square of the speed, are, at best, bold generalizations, and though they can be very fairly substantiated in a theoretical investigation, when we come to actual design other factors are apt to creep in which may necessitate a modification of the results by quite an appreciable percentage. I mention this point because the author states that it is his object to give such data as will enable power-station engineers to study and appreciate the merits of any particular turbine design, and it therefore seems well to warn the power-station engineer that when dealing with the intricacies of turbine design, as with other matters, things are not always what they seem. The test-results on Parsons turbines mentioned by Mr. Carnegie at the meeting in London (page 623) were not carried out by Messrs. Parsons, but in the case of the Carville and Dunston machines by the staff of the Newcastle-on-Tyne Electric Supply Co. I will leave it to Mr. Waite, who was in charge of these tests, to answer the author's question as to the instruments used and the method of water measurement. The other tests given by Mr. Carnegie—Chicago and Lots Road—were similarly carried out with standard instruments accurately checked for the tests, and the water was measured by direct weighing of the condensate in test tanks. Both the tests at Chicago were made by Messrs. Sergeant & Lundy for the Commonwealth Edison Co., while Mr. W. M. Selvey, acting as an independent consulting engineer, carried out the Lots Road tests on behalf of the London Electric Railways Co. and the manufacturers. The electrical instruments, gauges and thermometers for this test were all checked at the National Physical Laboratory before and after the test.

Mr. J. N. Waite: My remarks will be made entirely from a user's point of view. In considering what is the maximum size of a turbo-alternator for a particular undertaking, it has to be borne in mind that, in spite of the utmost technical knowledge and skill of designers and the highest class of workmanship in the shops and during erection, all classes of turbines and alternators are subject to breakdown. In electrical undertakings the first consideration is reliability of supply. This being so, it would appear that the maximum permissible size for an undertaking is the combined overload capacity of the remaining sets if the largest set suddenly broke down during peak load. In multiple-station undertakings the capacity of the interconnecting cable system and the load distribution would also enter into this question. I agree with the author that it is necessary to increase the factor of safety as the size increases, also that an emergency trip speed of 15 per cent above normal speed should be provided for. It is our experience that the usual emergency trip speed of 10 per cent above normal does not provide sufficient margin for security of supply during severe electrical short-circuits. In discussing blading efficiency on page 587 the author states: "The practical engineer will always turn back to one question: 'What overall efficiencies have actually been obtained?'" This question can only be satisfactorily answered by test-results accurately determined. This reflects the attitude not only of the practical engineer, but also of the technical engineer who has to handle practical work. While appreciating the author's suggested rational formula to enable results to be accurately predicted with new steam conditions, the practical engineer will require to see some fairly conclusive experimental evidence on the various factors outlined before he will use any such values. I happen to be in possession of accurate experimental data on the value of superheat on steam consumption, covering the range of 100 to 350 degrees F. This data shows that the size and type of turbine has no practical influence on this value; also the value obtained with 250 lb. gauge pressure appears to be of the same order as that obtained with 190 lb. gauge pressure. The values found are:—

Increase in superheat, deg. F.	100	150	200	250	300—350
Decrease in consumption, per cent ..	4·70	4·20	3·82	3·52	3·26

Using the above data I have worked out the first case in Table 11, but with 250 to 350 degrees F. superheat instead of the 300–400 degrees F. taken in the Table. The decrease in heat consumption for an increase of 100 degrees F. superheat then came out at 3·44 per cent. The Table gives 3·47. This gives a good confirmation of the author's figure. Table 12 gives the theoretical improvement due to reduced back pressure using 2·0 in. (Hg) as a standard. The actual improvement, as the author makes clear elsewhere, will be less than the theoretical. How much less is a matter of design and cost, but I doubt if it is generally realized how seriously the actual improvement falls short of the theoretically possible, as the absolute pressure is decreased—that is at full load. It will be found that at low absolute pressures, the load has a very considerable effect on the value realized in practice. These particular points and the reasons underlying them are of course well known to designers, and the author has devoted considerable space in his paper to showing how a designer can realize an economic compromise. It will probably be of interest to quote a few actual test-results.

6 000-kW Turbine. 190 Lb. Pressure; 160 Deg. F. Superheat.

Decreasing the absolute pressure from 2·0 in. to 1·0 in. (Hg) decreased the steam consumption as follows :—

Half Load, 8·2 per cent. Full Load, 3·8 per cent.

Theoretical, 9·1 per cent (Turbine designed for 2·0 in. Hg).

12 000-kW Turbine. 190 Lb. (Gauge) Pressure; 216 Deg. F. Superheat.

Decreasing the absolute pressure from 1·5 to 1·0 in. (Hg) decreased the consumption as follows :—

Half Load, 4·02 per cent. Full Load, 2·14 per cent.

Theoretical, 4·99 per cent.

Table J illustrates how the actual value falls away from the theoretical as the absolute pressure is decreased.

It will be noted that at half load the actual value followed close to the theoretical down to 0·9 in. (Hg), while at full load the actual value started to fall away at 1·6 in. (Hg). If the efficiency ratio actually realized at 1·6 in. (Hg) had been maintained down to 1·0 in. (Hg), the actual consumption would have been about 3 per cent less than was actually the case. Taking the above values and the actual consumption at full load at 1·0 in. (Hg), the following thermal efficiencies for the turbine are arrived at :—

Absolute pressure, in. (Hg) ..	0·7	0·8	0·9	1·0	1·1	1·2	1·3	1·4	1·5	1·6
Thermal efficiency, per cent..	24·77	24·80	24·85	24·85	24·85	24·81	24·75	24·67	24·57	24·46

showing that the economic limit of absolute pressure in this case at full load is 1·1 in. (Hg). To get the best results out of turbine plant such data as given above, compiled from actual test-results, are essential. Incidentally, it clearly illustrates the value of adequate-sized exhaust ends. One point not touched upon by the author is the influence of air-pump performance upon overall thermal efficiency. If the air pump is able to maintain the partial pressure of the air in the condenser so low that the temperature of the condensate leaving the condenser is practically the same as the temperature of the exhaust steam entering the condenser, the influence of the air-pump performance can be neglected, but if a substantial temperature difference exists, it must be taken into account when comparing overall thermal efficiencies. I am aware that when comparing turbine performance only, on a thermal basis, it does not enter, but as it is the overall thermal efficiency which controls the coal bill, as an operating engineer I am of the opinion that when thermal efficiencies are quoted, it should be stated if the basis is the temperature corresponding to absolute pressure or the temperature of the condensate leaving the condensers. As an example of the effect of this factor on the overall thermal efficiency, I have worked out three comparative cases [the first (*a*) already given at 1·0 in. abs.].

Table J.

12 000-*kW Turbine.* 190 *lb.* (*Gauge*) *Pressure;* 216 *deg. F. Superheat. Percentage Value on Consumption for each* 0·1 *in.* (*Hg*) *Absolute Pressure.*

Absolute Pressure	Percentage Value of Consumption		
	Half Load	Full Load	Theoretical
In. (Hg)	per cent	per cent	per cent
1·7	—	—	—
1·6	0·59	0·60	0·80
1·5	0·63	0·61	0·82
1·4	0·68	0·59	0·85
1·3	0·74	0·51	0·89
1·2	0·76	0·42	0·95
1·1	0·86	0·36	1·02
1·0	0·98	0·26	1·11
0·9	1·10	0·23	1·21
0·8	1·12	0·15	1·32
0·7	1·06	0·14	1·47

(*a*) Condensate temperature equal to vacuum temperature.

(*b*) Condensate temperature 10 deg. F. lower than vacuum temperature.

(*c*) Condensate temperature 20 deg. F. lower than vacuum temperature.

Overall thermal efficiency, 24·85, 24·65, 24·46 per cent.

Heat units per kilowatt-hour, 13 740, 13 850, 13 960 B.Th.U.

The author rightly takes into account the saving on the boilers and condensing plant due to less steam having to be produced and dealt with for a more efficient turbine. For a given performance with given conditions the size of the condensing plant can be

calculated to within very fine limits, and in this connection it can be usefully pointed out that it is no use providing condensing plant capable of producing a low absolute pressure if the turbine cannot efficiently utilize such low absolute pressure. The value in £ s. d. of a given decrease in steam consumption view this is not very important, as with efficient operation the length of time a turbine will operate on overload capacity will be relatively small.

With regard to the test-results of the five Parsons 10 000-kW machines at Carville, and the figures quoted by Mr. Carnegie in the London discussion, the limit

TABLE K.—CARVILLE POWER STATION.

Official Consumption Test-results on Five 10 000-kW Turbo-alternators by Messrs. C. A. Parsons & Co.

Turbo-alternator No.	1	2	3	4	5
Designed economical output, kW	10 000	10 000	10 000	10 000	10 100
Date of test	7/4/21	14/11/19	16/12/20	26/1/20	17/6/20
Speed, r.p.m.	2 440	—	2 414	2 441	2 440
Hours run before test	26 274	12 824	14 501	427	400
Duration of test, minutes	120	120	120	120	120
Steam conditions.					
Pressure at stop valve, lb. per sq. in. (gauge)	240	251	234	241	237
Temperature at stop valve, ° F.	692	687	707	678	696
Superheat at stop valve, deg. F.	290	281	306	275	295
Pressure at H.P. inlet, lb. per sq. in. (gauge)	225	229	219	221	199
Vacuum.					
Barometer, inches (Hg)	30·67	30·07	30·58	29·56	30·13
Vacuum at exhaust flange, inches (Hg)	29·68	29·11	29·54	29·56	29·17
Absolute pressure at exhaust flange, inches (Hg)	0·99	0·96	1·04	1·00	0·96
Load.					
Units generated	20 175	19 981	19 688	20 106	20 343
Average load, kW	10 087	9 990	9 844	10 053	10 172
Average power factor, lag	0·75	0·77	0·75	0·75	0·76
Steam consumption.					
Total steam at weigh tanks, lb.	209 147	201 308	199 847	207 418	205 431
Used by augmentor steam jet, lb.	524	620	228	686	970
Condenser leakage, lb.	54	63	40	64	161
Actual total, lb.	208 569	200 625	199 579	206 668	204 300
Actual, lb. per kWh	10·34	10·04	10·14	10·28	10·04
*Efficiencies.**					
Actual overall thermal	25·17 %	25·96 %	25·55 %	25·46 %	25·87 %
Actual overall on Rankine cycle on stop-valve pressure	72·6 %	74·5 %	74·0 %	73·8 %	74·4 %
Actual overall on Rankine cycle on P1 pressure	73·2 %	75·3 %	74·6 %	74·5 %	76·1 %
Actual turbine only on P1 pressure	76·2 %	78·5 %	77·7 %	77·6 %	79·2 %
Corrected consumption,† lb. per kWh	10·26	9·92	10·12	10·09	10·00

* Thermal efficiencies based on vacuum temperature; alternator efficiency on 10,000 kW at 0·75 power factor estimated as 96 per cent; all heat values taken from Marks & Davis's 1918 steam tables.

† Actual results corrected to 250 lb. (gauge) stop-valve pressure, 300 deg. F. superheat, 1·0 inch (Hg) exhaust pressure, and 10 000 kW load, i.e. average operating conditions.

Correction values used were obtained from tests.

varies directly with the output. Neglecting breakdowns, the operating engineer can usually estimate this output fairly accurately; consequently, if he is in possession of the comparative prices and consumptions of turbo plant, he will be able to settle for himself which is the economic machine. With regard to the relative merits of extra nozzles to obtain overload and by-passing expansions, from an efficiency point on

of error can be guaranteed to be less than 0·5 per cent. These tests, the results of which are given in the accompanying Table K, were made by the Steam Section of the Testing Department of the Newcastle-upon-Tyne Electric Supply Co. to the instructions of Messrs. Merz and McLellan. In all the tests the method was identical. The electrical output of the alternator was measured by a specially calibrated standard watt-

hour meter operated in conjunction with special high ampere-turn current transformers, the whole equipment, with potential transformers, being checked against reference standards. The power factor was measured by a reactive component meter. All pressure gauges were calibrated before the tests on a standard dead-weight testing set. All thermometers were calibrated before the tests to reference standards calibrated by the N.P.L. The stop-valve steam temperature was measured by a Whipple resistance thermometer. The barometer was checked before, during, and after the tests against a meteorological standard. All the auxiliary plant was fed from the station low-tension busbars, and the neutral of the alternator was isolated. The condensate was weighed in the station weigh-tank, which had been previously calibrated. Precautions were taken to make any leakage to or from the pipe between the lift pump and the weigh tank impossible. The salinity of the condensate and the circulating water was tested every 20 minutes during the tests, and any slight leakage indicated was corrected for on a percentage basis. The main condensers have augmentor condensers in series with them, the steam used by the augmentor steam jet being condensed by the condensate so that all the heat of the steam used is returned to the condensate. Normally this steam is bled from the turbine, but for the purpose of the tests, the steam connection from the turbine was blanked off and the jet fed with steam from the boiler side of the stop valve. The amount of steam used by the jet has been calculated by the rise in temperature of the condensate due to the jet. The figure arrived at by this method has been checked by calculating the steam flow through the nozzle from the available heat-drop, the throat of the nozzle being accurately measured. The tests were started and finished by electric light signals between the weigh tank and the meters. The accuracy under consideration is the accuracy of the figure recorded for steam consumption in pounds per kilowatt-hour, therefore the accuracy of the readings affecting this figure are given as a percentage of the consumption. The figures given for the limit of accuracy are the estimated values for the absolute accuracy. The relative accuracy will be much better.

		Per cent
Electrical output	+ or —	0·25
Total steam consumption at weigh tank	+ or —	0·08
Steam used for augmentor jet	+ or —	0·04
Salinity (condenser leakage)	+ or —	0·02
Steam pressure	+ or —	0·02
Steam temperature	+ or —	0·13
Exhaust pressure	+ or —	0·02
Total	+ or —	0·56

The estimated accuracies given for the individual values are the result of long and specialized experience in testing work on power plants, given the necessary standards. For relative values a total accuracy can be obtained of less than 0·1 per cent. It is inconceivable that the inaccuracies of measurement of the various factors would all be operating one way, and therefore it is reasonable to say that the limit of accuracy of the consumption results given in Table K is within + or — 0·5 per cent. The relative accuracy of the figures for the individual machines is estimated at + or — 0·2 per cent.

Mr. R. Dowson : The author states on page 565 that the developments in turbine construction during the period under review (1912–1921) may best be described as "the race for maximum output at the highest possible speed." This may be correct in the case of impulse builders, but it certainly does not represent the policy pursued—at any rate in this country—with the reaction turbine, of which the Parsons Company are the principal exponents. The policy of this firm has always been safety first, steam economy next, with size and speed last, and these practices, together with simplicity of design, have made Parsons turbines immune from the more serious types of disaster tabulated in Table 23, which refers mainly to impulse turbines. Anyone looking at the Parsons design shown in Fig. 7 must be impressed by the simplicity of design ; both shafts are single forgings without built-up construction of any kind, whilst all cylinder castings are of a simple and straightforward character. The whole arrangement is in great contrast to the complicated construction of the Continental designs illustrated in Figs. 6 and 11, and also with the American practice shown in Fig. 8. The turbine depicted in Fig. 13, a modern production of the author's firm, looks a clean, straightforward design, but complications begin when we turn to Fig. 14, which shows the multi-exhaust arrangement with only one extra wheel. Fig. 30 gives some idea of the extra complication involved when two additional wheels are introduced, and the author mentions as many as three on page 606. Even in 1912, at the time when the author's first paper was published, Parsons practice was a long way behind that of most impulse turbine exponents in the matter of high surface speeds and large output per cylinder, and I think that this fact more than anything else misled many engineers into the opinion that reaction turbines had not so great a future as those of the impulse type. When in 1913 Messrs. Fraser and Chalmers put on the market a 5 000-kW turbine at 3 000 r.p.m., Messrs. Parsons had not even produced a 3 000-kW machine at this speed. The 8 500-kW 1 500-r.p.m. Parsons turbine installed at Sheffield in 1916 was, at that time, the largest single-cylinder unit that this firm had ever constructed. In the pioneer large tandem turbine [20 000 kW for Chicago, item (2) in Table 1] which was built in 1913, no one can accuse the designers of taking any unnecessary risks as regards safety factor or possible vibration, the speed being only 750 r.p.m. and the maximum surface speed only 282 feet per second (mean blade speed, 345 ft. per sec.). It was not until 1915, when the 10 000-kW 2 400-r.p.m. Carville machines were designed, that a mean blade speed of 578 ft. per second at rated r.p.m. was permitted, with relatively small forgings. Messrs. Parsons' practice does not exceed 650 ft. per second to-day. I do not think that the author takes quite a fair view of the case for the tandem design. No one denies that the cost per kilowatt is more than with a single-cylinder design,

or that the gain in economy is only small, but the fact remains that a very large proportion of the best orders which are placed with the Parsons Company are obtained with tandem turbines. There must be some justification for this state of affairs, and that justification appears to me to be that lower steam consumptions can be obtained with reaction turbines than with impulse turbines, and this is why, as an offset, impulse turbine builders struggle to pull their cost per kilowatt down, by adhering to the single-cylinder design. The evolution of the Parsons tandem turbine in particular might be touched upon here. In the first example—the 1 000-kW at 1 500 r.p.m. Elberfeld machines — a single-ended low-pressure cylinder was adopted, because at that time the idea was not to increase output, but to increase reliability by limiting the high steam temperature to a small cylinder. Later on, as the demand for larger output arose, the double-ended low-pressure cylinder followed as a natural consequence, for if a tandem turbine was to be used at all, it might as well be double-ended, so that the output could be doubled without affecting the surface speeds. This double-ending, however, was first put into practice for exhaust steam turbines, and it was not until 1913 that the application to large outputs began. This application first found expression in the 20 000-kW 750-r.p.m. Chicago turbine, the 15 000-kW 1 000-r.p.m. Lots Road turbine, and the six 12 000-kW 1 500-r.p.m. turbines for Melbourne, all designed during 1912–1914 (see Table 22, items 1, 3 and 4). The innovation brought with it not merely double the output for the same surface speeds and leaving loss, but also the twin-condenser arrangement, which has the advantage that the condensers can be cleaned—one at a time—whilst the turbine is in commission. On page 485, the author appears to be resigned to an inevitable falling off in efficiency ratio with higher steam conditions owing to the decreasing overall velocity ratio. This reduction in efficiency ratio is certainly unavoidable as long as a single-cylinder design is adhered to, and is one more argument in favour of the tandem design. Table 12 shows the decrease in exhaust temperature required to improve the steam consumption by 1 per cent, and it appears to me that it is in this direction that the author's multi-exhaust principle can best be utilized, assuming its claims to be justified. On page 606, however, the author rather gives the impression that the multi-exhaust was devised mainly as a means of increasing the maximum attainable output from one cylinder. It may, of course, be used for either purpose—increasing the output or utilizing higher vacua—but the first-mentioned application appears to me to be rather discounted by the absence of demand —at any rate in this country—for colossal output from one machine. In like manner, in the whole of the discussion in Part III, he deals admirably with the economic rating of a given turbine frame, but without in the least indicating how that frame is to be chosen. I mention this because on page 578 the author expresses his intention of discussing (in his paper) the situation in such a manner as to enable the power-station engineer to appreciate the merits of any particular turbine design. What the power-station engineer also wants to determine, however, is the best size of units to install in his station. If it can be shown that there is no appreciable advantage from the point of view of steam consumption to be gained by installing a 20 000-kW machine instead of a 10 000-kW machine—whether the multi-exhaust be used or not—then the case for the larger output per turbine in the station must rest upon other considerations, which are unfortunately not discussed in the paper. Mr. Carnegie has published the actual test-results obtained on the 10 000-kW Parsons reaction turbines at Carville, in order to demonstrate what steam economies can be realized in comparatively small machines—which do not involve putting all the eggs into one basket. On page 587 the author says, with regard to the choice of type of blading, that "no predjudices of any kind should enter into the designer's mind when trying to arrive at the best solution." This is certainly an admirable sentiment, but in practice it is rather difficult to adhere to, on account of the patent position. It is of little use for a designer to decide that any particular type of blading —i.e. Curtis, Rateau or Parsons—is the thing for any particular portion of his turbine, if the only really mechanical method of manufacture or fixing of that blading is somebody else's patent. In fact, on page 588 the author rightly says that "the merits of one type of blading over the other do not depend so much on the type of blading used as on the detail construction employed in connection with it." Further, whilst discs are common to all types of turbines as a method of holding or supporting the revolving blades, it must be admitted that diaphragms are a special feature limited to the impulse type, and as such they may be considered to be a part of the impulse blading. I am sure, however, that no one would go so far as to say that diaphragms are a desirable feature; they are rather a source of weakness, so that this must be counted against the impulse blading principle. Turning to the discussion of average overall velocity ratio on page 588, there is, of course, no real difference between the two relations (5) and (6). No doubt, however, the form of expression in (5) may be more used to impulse turbine designers than that in (6), which is the original Parsons "K" coefficient. Professor Stoney * shows that if "K" has the definition given in the paper, and H_A = actual total available heat-drop over the blading in question, then the average overall velocity ratio $= \sqrt{[K/2 \cdot 65 H_A]}$ for pure pressure-compounded impulse turbines. This last expression is the author's Equation (3) in a slightly different form. In Part III, Fig. 20 is most instructive, in analysing the variation in steam consumption with the rating of a turbine. Whilst the derivation of curves B and C is clear, I am not quite sure how the author arrives at curve C, unless he assumes that $l_c \propto K_c^3$, calculates one value of l_c and then draws a suitable parabola through it. On page 590 it is stated that "these (C^2) losses vary *approximately* as the square of the rating," but unless the assumption of the exact relationship be made, it is difficult to see

* *Engineering*, 1921, vol. 111, p. 250.

how Equation (3) on page 591 can be arrived at. As this is the differential equation representing the slope of the curve of steam consumption D (Fig. 20), I think it would be as well to point out that its application is limited to small changes in rating only. That is to say, it cannot be used to calculate the point F from a knowledge of point E′, on curve D. I am much interested in the mathematical expressions arrived at on page 592 with regard to generating costs, and wish that the author could have given more information as to how they are obtained. On page 593, the author puts into a nutshell this method of analysis (viz. "by comparing the decrease in the capital costs resulting from a given increase in rating with the increase in the cost of fuel which results from the same increase in rating"). This seems to me to be an excellent method of tackling the problem, but, in order to ascertain the economic value of any increase or decrease in fuel consumption, it is surely necessary to assume some life period for the plant, and I cannot find this stated anywhere. Obviously, if the fuel consumption per kWh of a plant be increased, the total extra cost of fuel depends on how long the plant runs, that is, upon its life. On page 602, where the author introduces the subject of "specific" or "characteristic" speeds of turbo-machinery, it is stated that the ability to run any machine in question at the highest possible speed "not only decreases the cost of the machine itself, but also that of the driven generator or motor driving it." This does not appear to be in agreement with the statement made on page 565 to the effect that the first cost of the high-speed alternator is about the same as that of a low-speed design of the same output. On pages 609–610 the author discusses the merits of nozzle versus throttle governing, and, I think, rightly lays great emphasis upon the importance of avoiding temperature distortion when meeting overloads, quite apart from any question of which method gives the better steam consumption. From this point of view, nozzle governing would appear to be the best practice for impulse turbines, because if by-passing is used it is necessary to admit high-temperature steam between two diaphragms further down the turbine, whereas nozzle governing means that the high temperature is confined to the primary nozzle boxes. On the other hand, nozzle governing involves partial admission on one or more wheels of the turbine, which may prejudice the best efficiency at the economical load when many nozzles are shut off. In the reaction turbine, the case is rather different. Owing to the fact that the turbine is "drowned" throughout, there is no alternative to by-passing the steam, if greater loads than the "economical" are to be developed. On the other hand, the compact and simple design of the reaction turbine at the steam inlet makes the cylinder far less liable to temperature distortion than is the case with the impulse turbine. In modern reaction turbines the by-pass belt can be placed close up to the primary inlet belt, and in this case it will be evident that, even on the economical load, the temperature of the by-pass belt will be a good deal increased by conduction of heat from the primary, and that on overloads the extra heating up of the by-pass belt and remainder of the high-pressure portion of the cylinder is likely to be quite regular, and therefore free from danger, if the usual simple arrangements for permitting longitudinal expansion have been made.

Mr. K. Baumann (*in reply*): There are a great number of points in the remarks of Messrs. Gardner and Dowson which I have dealt with in my reply to the discussion at London (see page 637). In what follows I have dealt only with new points which have been raised.

With regard to Mr. Gardner's remarks on the list of breakdowns, it was one of my objects in writing the paper to explain the cause of breakdowns of impulse turbines and, in doing so, I endeavoured to get as much data as possible in connection with such breakdowns. This explains why impulse turbine troubles are in the majority as far as the list is concerned, and if Mr. Gardner concludes from this list that there are relatively few troubles with reaction turbines he is mistaken. The weaknesses of the reaction turbine are well known, and it was not necessary to emphasize them in the paper. If I had desired to collect data on the troubles with reaction turbines there would doubtless have been no difficulty in compiling a list equal in size to that published, and the list might have been rather monotonous in respect of the cause of breakdown. I do not understand how Mr. Gardner arrived at the conclusion that my explanation of my preference for a peripheral speed higher than the running speed should dispose of the claim that the impulse turbine can be run with larger clearances between the blades and diaphragms. The relatively small clearances in the impulse turbine which are affected by the whipping of the shaft are those between the diaphragm glands and the shaft, and not between the moving blades and the stationary blades, as is the case in the reaction turbine. It is obvious that the amount of leakage is much smaller in the impulse turbine than in the reaction turbine; first, because the diameter is smaller and consequently the leakage area is smaller in proportion; and secondly because, as a result of the smaller peripheral speed of the rotor at the gland, the clearance can be kept considerably smaller than in the reaction turbine, without affecting the reliability of the turbine. The truth is, therefore, that the impulse turbine has a considerable advantage over the reaction turbine in this respect, and Mr. Gardner's statement on this subject is obviously misleading.

The multi-exhaust turbine has an advantage over the double-flow turbine in that it can be made in a single casing and with a single exhaust, without adding appreciably to the length, weight, and cost. Mr. Gardner criticizes the abrupt change of section half-way up the blade in the double-tier blade. The change in section is very small, as an inspection of the blade section clearly shows, and not only does the additional staying effect neutralize any tendency for additional bending stresses, but in addition the blade is strengthened to an extent much greater than is possible with the binding strips used with reaction blades. The change of velocity at loads below the economical is very small, as at light loads the vacuum is increased. The pressure at which

the steam should be reheated is much higher than that which is usually adopted for the exhaust pressure in two-cylinder machines, and consequently this factor does not operate in favour of the double-flow turbine. It is obvious from his remarks that Mr. Gardner has not understood my remarks in connection with the ease of standardization of multi-exhaust turbines. He has also misinterpreted my remarks on experimental turbines. It was not my intention to belittle research tests which can be made on experimental turbines, and I fully agree with him on the advisability of such tests for the purpose of comparison of blade profiles, blade spacing, etc., but I still adhere to my contention that it is most difficult to compare test-results thus obtained with results possible with large commercial turbines.

Mr. Gardner thinks that the investigations in Part III of the paper are very limited in their practical application, and that as these investigations are based on somewhat arbitrary assumptions they may, if not properly interpreted, lead to very misleading conclusions on the subject. What are these arbitrary assumptions which are the basis of Part III of the paper? I have shown that for given steam and operating conditions the economic rating for a given turbine frame depends on its constant kW losses, the leaving area which controls the leaving losses, and on its cost. It is largely a question of the cost of a turbine per square foot of leaving area. The type of the turbine, or its design, does not enter into the question at all excepting in so far as the type affects the values of the three characteristics referred to, viz. (1) the constant kW losses, (2) the leaving area, and (3) the cost of the turbine. Surely Mr. Gardner does not suggest that the reaction turbine is such a marvellous machine that its leaving area has no effect on the steam consumption or that mechanical losses do not exist, or that its cost is of no consequence, and yet this is what one would conclude from his remarks. It is clear from the calculations given in Part III that the variation of the rating with varying back pressure is practically independent of either the cost of the turbine or the mechanical losses, and consequently this law will apply to any type of turbine, so that Mr. Gardner's warning to power-station engineers is quite unnecessary. In view of the misunderstanding under which Mr. Gardner seems to be labouring it may be advisable to state here the problems with which I have not dealt in Part III.

(*a*) I have not attempted a mathematical solution of the problem as to which is the best type of turbine for any set of steam conditions and rating. Such a solution would be distinctly complicated and no doubt Mr. Gardner has very decided opinions on this subject.

(*b*) I have not attempted to show how the design of a turbine should be changed to make it suitable for utilizing higher vacua for a given rating, and how the cost would be affected.

(*c*) I have not attempted to calculate how the cost of a turbine varies with increased rating for given steam conditions, and a given speed (r.p.m.).

The problems (*b*) and (*c*) are particularly of interest to turbine users. If (*c*) is given, i.e. the variation of the cost with varying rating, it can be shown that by means of the laws evolved in Part III of the paper the information stated under (*b*) can be obtained in a very simple way. To illustrate this, I will assume that for a given speed (r.p.m.) the cost C of a turbine varies as the two-thirds power of the rating R, i.e.

$$C = \text{Constant} \times R^{2/3}$$

According to this law the cost per kW capacity decreases 20 per cent for an increase in the rating of 100 per cent. This variation is represented by curve A in Fig. K for a back pressure of 2 in. absolute. The rating of a given frame may be 10 000 kW and the cost of this turbine may be £20 000 as represented by point B. According to the law obtained in Part III of the paper, the rating of a given frame varies as the square root of the back pressure. This means that for a back pressure of 1 in. abs. the rating of this frame will be 7 100 kW and the cost of the frame will remain

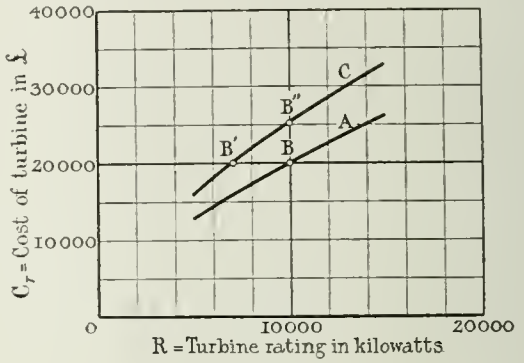


FIG. K.

practically constant. This fact is represented by point B′. Applying the rule stated above, i.e. that the cost varies as the two-thirds power of the rating, we obtain another curve C, representing the cost of turbines for various ratings, and for a back pressure of 1 in. abs. The cost of the turbine for a given rating, i.e. 10 000 kW and 1 in. abs., is given by point B″. It is 1·26 times the cost of the turbine for the same rating at double the back pressure. If the cost of turbines varies as the two-thirds power of the rating, it follows, therefore, that for a given rating the cost varies inversely as the cube root of the back pressure. Whilst this rule is only approximate it is sufficiently accurate to enable one to form an approximate idea of the additional cost involved in designing turbines for high vacua.

The data given by Mr. Waite on the subject of the decrease in steam consumption with increased superheats are of great interest, representing, as they undoubtedly do, the outcome of a great number of tests on commercial turbines of varying capacities. He shows that they check well with the calculations given in the paper on the basis of the reheat and wetness corrections. These corrections are also based on actual test-results, and not, as Mr. Waite seems to suggest, on purely theoretical considerations. It is interesting to compare the figures given by Mr. Waite with the

corrections proposed in my 1912 paper. This comparison is given in Table L, which shows that the difference between the two sets of figures is relatively small. The data given in connection with vacuum corrections are also very interesting, inasmuch as they indicate how important it is to provide the necessary leaving area to enable the turbine to utilize a high vacuum, but to the designer the data are of no value without particulars relating to the leaving area and exhaust area of the particular turbines to which the test-results refer. I do not agree with Mr. Waite on the subject of condenser performance. The importance he attaches to the temperature of the condensate practically disappears with the introduction of modern methods of heating the feed water by means of steam taken from the low-pressure end of the turbine. Whilst without feed heating a difference of 10 deg. F. in the temperature of the condensate may result in a difference in the heat consumption of nearly 1 per cent with modern

TABLE L.

Superheat Varying	Increase in Superheat Necessary to Improve Steam Consumption by 1 per cent	
	1912 paper, deg. F.	Mr. Waite, deg. F.
From 0 to 50 deg. F. ..	10	—
50–100 deg. F.	10	—
100–150 deg. F.	12	10·6
150–200 deg. F.	12	11·9
200–250 deg. F.	14	13·1
250–300 deg. F.	14	14·2
300–350 deg. F.	—	15·3

methods of heating feed water, this difference in the steam consumption becomes only a small fraction of 1 per cent. This is clear from the following consideration :—

In the case of a 10-stage turbine a difference of 10 deg. F. in the temperature of the feed water can be made good by taking 1 per cent of steam from the last stage of the turbine and using it for heating the condensate. The amount of mechanical work lost, which is the measure of the value of the difference of 10 deg. F. in the temperature of the condensate, is only 0·1 per cent, that is, only one-tenth of that suggested by Mr. Waite. Mr. Waite is of the opinion that in comparing the relative merits of extra nozzles to obtain overloads or by-passing steam to an intermediate stage, the difference in efficiency need not be considered, as the length of time a turbine will operate on overload capacity will be relatively small. I agree with him, and it is for this reason that in comparing the two methods I have left economy out of account. Reliability is the only factor which I considered in this connection, and I wish to emphasize again that the fact that overload may be applied only occasionally does not in any way mitigate the objections to by-passing steam to an intermediate stage. I am glad to have the description of the method of the tests of the Carville sets, and I note the great degree of accuracy which Mr. Waite expects to obtain from this method. I feel that in estimating these accuracies he has only taken into account the accuracy of the instruments and that he has omitted the inaccuracy which results from errors in observations during the tests, which may be of a greater order than those resulting from the calibration of the instruments. I certainly think that Mr. Waite is very optimistic when he expects the accuracy of the test-results to be within + or — 0·2 per cent.

Mr. Dowson emphasizes the simplicity of the Parsons design as illustrated in Fig. 7 of the paper and compares it with the design shown in Fig. 14, which he considers to be too complicated. He does not draw attention to the fact that the turbine illustrated in Fig. 14 is provided with feed heaters, and I should like to know the appearance of his design after feed heaters have been added to it. Further, he does not appear to appreciate the advantages which are derived from the use of a single exhaust. He does not point out the fact that the two condensers which are necessary in connection with double-flow turbines complicate the condensing plant considerably and increase its cost. As the two condensers are a good distance apart, it is necessary to provide for considerable movement of one condenser relative to the other. This involves a great number of expansion joints. The low-pressure cylinder shown in Fig. 7 is extremely weak and subject to distortion from the smallest force, which may result from faulty adjustment of the springs supporting the condensers. He considers that the two-condenser arrangement has the advantage that one condenser can be cleaned while the turbine is in commission, but he does not mention that large connecting pipes which combine the two condensers into one complete unit are necessary between the two condensers. It would have been interesting to hear how often turbine users avail themselves of the advantage mentioned, i.e. that one condenser can be cleaned with the turbine in operation.

I note the difficulties Mr. Dowson has in following the mathematical calculations in Part III of the paper, and as I have had inquiries from other sources I am giving at the end of these remarks, in the form of an appendix, the development of the various formulæ used. Mr. Dowson is under a misapprehension when he states that the life period of the plant has not been properly taken care of in the calculations on pages 592 and 593. He has overlooked the fact that this is included in the factor γ which includes the interest, depreciation, and maintenance charges expressed as a percentage of the initial cost of the turbine, as was clearly stated in the paper.

APPENDIX TO PART III OF PAPER.

The Minimum Steam-Consumption Rating (see Page 591).

The output at the coupling K_c is

1. $K_c = \eta_b\left(\frac{Si_0}{3\,420}\right)(1 - l_0) - K_m$

2. $K_c = \left(K_b - \frac{K_m}{1 - l_0}\right)(1 - l_0)$

3. $dK_c = (dK_b)(1 - l_0) - \left(K_b - \frac{K_m}{1 - l_0}\right)dl_0$

4. $\frac{dK_c}{K_c} = \frac{dK_b}{K_b - \frac{K_m}{1 - l_0}} - \frac{dl_0}{1 - l_0}$

5. $\frac{dK_c}{K_c} = \frac{dK_b}{K_b}\,\frac{1}{1 - \frac{K_m}{K_b(1 - l_0)}} - \frac{dl_0}{l_0}\,\frac{l_0}{1 - l_0}$

6. But as $\frac{1}{1 - \frac{K_m}{K_b(1 - l_0)}} = 1 + \frac{K_m}{K_b(1 - l_0)}$

7. and $\frac{dl_0}{l_0} = 2\frac{dK_b}{K_b}$

8. $\frac{dK_c}{K_c} = \frac{dK}{K_b}\left(1 + \frac{K_m}{K_b(1 - l_0)} - \frac{2l_0}{1 - l_0}\right)$

On the other hand, the steam consumption in lb. per kWh at the coupling is

9. $s = S/K_c$, or on differentiation we get

10. $\frac{ds}{dK_c} = \frac{\frac{dS}{dK_c}K_c - S}{K_c^2} = \frac{\frac{dS}{dK_c} - s}{K_c}$

11. As $\frac{dK_b}{K_b} = \frac{dS}{S}$ we get from (8)

12. $\frac{dS}{dK_c} = \frac{S}{K_c}\,\frac{1}{1 + \frac{K_m}{K_b(1 - l_0)} - \frac{2l_0}{1 - l_0}} = \frac{s}{1 + \frac{K_m}{K_b(1 - l_0)} - \frac{2l_0}{1 - l_0}}$

which inserted in (10) gives

13. $\frac{ds}{dK_c} = \frac{s}{K_c}\left(\frac{1}{1 + \frac{K_m}{K_b(1 - l_0)} - \frac{2l_0}{1 - l_0}} - 1\right)$

or having in mind that $\frac{K_m}{K_b(1 - l_0)} - \frac{2l_0}{1 - l_0}$ is small compared with unity we get

14. $\frac{ds}{dK_c} = \frac{s}{K_c}\left(\frac{2l_0}{1 - l_0} - \frac{K_m}{K_b(1 - l_0)}\right)$

or as $K_b(1 - l_0) = K_c + K_m$

15. $\frac{ds}{s} = \frac{dK_c}{K_c}\left(\frac{2l_0}{1 - l_0} - \frac{K_m}{K_c - K_m}\right)$ [see page 591 formula (5)].

The Economic Rating (see Page 592).

16. C_T = Capital cost of a given turbine frame in £.

17. s = Steam consumption in lb. per kWh generated.

18. e = Evaporation of boiler in lb. per lb. of coal.

19. K_c = Rating of turbine at coupling in kW.

20. K = Corresponding rating of set at generator terminals in kW = $K_c\eta_g$.

21. λ = Load factor referred to the turbine rating.

22. ϕ = Cost of coal in shillings per ton.

23. γ = Interest, depreciation and maintenance in per cent of initial cost of turbine.

24. f = Fuel costs per kWh in pence = $\frac{12\phi s}{2\,240e}$

25. c_T = Capital charges due to turbine per kWh in pence = $\frac{C_T\gamma 240}{8\,760K\lambda} = \frac{1}{36{\cdot}5}\cdot\frac{C_T\gamma}{K\lambda}$

26. u = Total cost per kWh in pence = $f + c_T$

27. u becomes a minimum for $\frac{du}{ds} = 0$, ϕ, e, c_T, γ, λ, being constant for a given case.

28. Therefore $\frac{12\phi}{2\,240e} = \frac{C_T\gamma}{\lambda}\cdot\frac{240}{8\,760}\cdot\frac{dK}{ds}\cdot\frac{1}{K^2}$

29. Or $\frac{ds}{s} = \frac{dK}{K}\left(\frac{2\,240e}{12\phi s}\,\frac{C_T}{K}\,\frac{\gamma 240}{\lambda 8\,760}\right) = \frac{dK}{K}\cdot\frac{c_T}{f}$

30. But as $\frac{ds}{s} = \frac{dK}{K}\left(\frac{2l_0}{1 - l_0} - \frac{K_m}{K_c + K_m}\right)$ we get

31. $\frac{2l_0}{1 - l_0} - \left(\frac{K_m}{K_c + K_m}\right) = \left(\frac{c_T}{f}\right) = \frac{\text{Capital charges due to turbine per kWh in pence}}{\text{Fuel cost per kWh in pence}}$ [See page 592, formula (8)].

32. Where $\frac{c_T}{f} = 5{\cdot}11\,\frac{e\gamma}{\phi\lambda}\,\frac{C_T}{Ks}$

33. $\frac{c_T}{f} = m\frac{C_T}{K_c s}$

34. $m = 5{\cdot}11\frac{e\gamma}{\phi\lambda}$

The outlet losses l_0 for the economic turbine rating are obtained therefore from

35. $\frac{2l_0}{1 - l_0} = \frac{K_m}{K_c + K_m} + m\frac{C_T}{Ks}$ [see page 592, formula (9)].

The Overall Economic Rating (see Page 593).

The capital cost of the actual power station is

36. $C_{st} = b_e K + b_s K_s + C_T$ [see page 594, formula (10)].

The capital charges per unit generated are

37. $c_{st} = c_e + c_s + c_T = \frac{1}{36{\cdot}5}\cdot\frac{C_{st}\gamma}{K_c\lambda} = \frac{1}{36{\cdot}5}\frac{\gamma}{\lambda}\left(b_e + b_s s + \frac{C_T}{K}\right)$

The fuel costs are

38. $f = \dfrac{12\phi s}{2\,240e} = as$

The total costs per unit generated are

39. $u = f + c_{st} = \left(a + \dfrac{1}{36{\cdot}5}\dfrac{\gamma}{\lambda}b_s\right)s + \dfrac{1}{36{\cdot}5}\dfrac{\gamma}{\lambda}b_e + \dfrac{1}{36{\cdot}5}\dfrac{\gamma C_T}{\lambda K}$

This becomes a minimum for $\dfrac{du}{ds} = 0$, i.e.

40. $a + \dfrac{1}{3\,65}\dfrac{\gamma}{\lambda}b_s = \dfrac{1}{36{\cdot}5}\dfrac{\gamma}{\lambda}\dfrac{C_T}{K^2}\dfrac{dK}{ds}$

41. Or $\dfrac{ds}{s}\left(as + \dfrac{1}{36{\cdot}5}\dfrac{\gamma}{\lambda}b_s s\right) = \dfrac{1}{36{\cdot}5}\dfrac{\gamma}{\lambda}\dfrac{C_T}{K}\dfrac{dK}{K}$

42. Or $\dfrac{ds}{s}(f + c_s) = c_T\dfrac{dK}{K}$. From (42) and (15) follows as $\dfrac{dK}{K} = \dfrac{dK_c}{K_c}$

43. $\dfrac{2l_0}{1 - l_0} = \dfrac{K_m}{K_c + K_m} + \dfrac{c_T}{f + c_s}$ [see page 595, formula (11)] or with (34) $m = 5{\cdot}11\dfrac{e\gamma}{\phi\lambda}$ we get

44. $\dfrac{2l_0}{1 - l_0} = \dfrac{K_m}{K_c + K_m} + \dfrac{1}{\dfrac{1}{m} + b_s}\dfrac{C_T}{K_s}$ [see page 595, formula (12)].

DISCUSSION ON

"SOME THERMAL CHARACTERISTICS OF ELECTRIC OVENS AND HOT-PLATES."*

North-Eastern Centre, at Newcastle, 25 April, 1921.

Mr. F. H. Downie: Fig. 3 shows the importance of good thermal contact between the hot-plate and the article heated. A gap of 0·08 inch increases the time taken by the disc to reach its final temperature to nearly twice the original value. It is therefore necessary to use cooking utensils with strong, machined bases. The light aluminium pans of thin metal used for cooking on gas are not suitable for use on hot-plates if economical results are required, since the bottoms are apt to warp and buckle. The effect of the condition of the surfaces is shown in Fig. 2. Oxidizing the surface of the disc increases the rate of temperature-rise. Possibly a black surface would be better. The tests on the ovens show that the light type of oven with air insulation is better from the point of view of quick heating. In many baking operations the oven is used for only 20 to 30 minutes after it has been heated. The smaller consumption during the heating period would be more economical.

Professor H. Stroud: It seems to me that electrical engineers are attempting to copy other methods of constructing ovens, instead of designing one specially suited to the electrical method of heating. An oven with a small capacity for heat, and well polished in addition to being lagged, should be the aim. Probably this would be best achieved by a double-walled dome-shaped oven with heating elements in the dome, which might be of square section. To inspect the oven the dome might be balanced and thus easily raised. The ordinary hot-plate is not satisfactory, except for very slow operations. Possibly the radiant type with reflector beneath, and arranged with rings so that the utensil completely covers the aperture, will be successful for ordinary use.

Mr. W. F. T. Pinkney: The greatest room for improvement in cookers is in connection with the hot-plate, and what we demand of the manufacturers are quick-heating hot-plates, reasonably efficient ovens and, above all, reliability throughout. The greatest trouble we have to cope with is maintenance and upkeep, and the manufacturer must give greater attention to the reduction of maintenance. It would be interesting to know to what extent chrome steel could be utilized to reduce some of the losses indicated in the paper. Cooking utensils constructed of this material, with polished exteriors and machined bottoms, having a similar surface to cast iron, might perhaps be produced. The question of suitable utensils for use with electric cookers has not received sufficient attention from manufacturers. Much more can probably be done on the lines of immersion heaters of suitable capacity and size for boiling liquids, instead of using hot-plates for this purpose. In regard to the construction of ovens, the light oven, although more suitable for quicker heating, generally lacks mechanical strength and frequently gives trouble for this reason. This paper shows the first serious attempt to go deeply into the research side of electric cooking, and I hope it will be followed by further experiments, the results of which will be given to us.

Mr. P. Ward: The authors have given us a set of tests taken under standardized conditions, which are very useful from a manufacturer's and from an engineer's point of view, but tests taken under working conditions would possibly have proved more valuable in the long run, as the results could be put before actual or potential users of cookers, whereas the present results would not convey to them any useful information. A hot-plate of the open type will do away with the necessity for utensils with ground bottoms, but from

* Paper by Dr. E. Griffiths and Mr. F. H. Schofield (see pages 361 and 373).

Table 2 it does not appear that any great reduction is effected on the time required to boil the contents of the utensil. Loss of efficiency is not a serious matter if quick boiling can be obtained for small quantities of liquid. Open-type plates can be loaded up without trouble, but if a solid-type plate is loaded up to 25 watts per square inch of surface, maintenance costs become excessive and the consumer regards the plate as unreliable. Even at such a loading the boiling time is still too long. The distribution of temperature inside the oven during the heating-up period is not very important, as no cooking operation is in progress during that time, but the maintenance of even temperature when the elements are on "medium" or "low" heats is to be desired. The time taken for the outer casing of the oven to reach a steady temperature is interesting. I had formed the opinion from observation in actual practice that this would be about 60 minutes. This time, however, will doubtless vary greatly with the quantity and nature of the lagging. With regard to the general design of cookers it would certainly be an advantage to depart from the "gas stove" model. Some makers have a cooker on the market at the present time in which the oven and hot-plate are separate and can be mounted side by side at any convenient height. There is also the "dome" type of oven, which appears to be very efficient, but the question of efficiently supporting the heating element needs further attention. In one oven of this type which I saw recently the element was supported at 192 points, at any of which a breakdown might occur.

[The reply of Dr. Griffiths and Mr. Schofield to this discussion will be found on page 665].

Scottish Centre, at Dundee, 29 April, 1921.

Mr. J. A. Omand: About 10 years ago I made practically the same tests as those given in the paper, and obtained almost identical curves. I tested 20 different irons at that time. I placed the thermometer 1 inch from the sole of the iron, and put it in a box to keep off the draught. I was always under the impression that the boiling-water test was the most practical way to measure efficiency. In the case of one kettle the efficiency was 72 per cent. I found, in boiling the ordinary kettle with the sole in direct contact with the hot-plate, that the efficiency was about 53 per cent. I agree with the authors that a good thermal contact is essential. The ordinary cast-iron pot placed on a hot-plate seldom gives good results, because it is usually curved. About 8 to 12 per cent of heat is lost by not having the pot ground or turned up. Fig. 12 should be of great assistance to manufacturers of cooking and heating utensils. I should like to draw attention to the loss of weight occurring when meat is cooked in an oven. In the case of one test the loss was 18·4 per cent. The energy consumption per lb. cooked weight was 0·49 unit, raw weight 0·33 unit per lb. When pork was roasted the consumption was again 0·33 unit per lb. raw weight, but 0·42 unit per lb. cooked weight. The loss in weight in the latter case was nearly 30 per cent. I find the question of heat distribution in ovens a very difficult one. Some cooks hold that it is of no use to have the heat at the bottom of the oven; that the heat must be at the top to bake pastry, while for cooking roasts it should be all round the oven. Electric cooking is an important subject, and one that requires a great deal more attention than is now paid to it. I was astonished at the low efficiencies obtained by the authors. In Table 2 these are apparently much better than in the first test. In the thermal tests which I took, the gas efficiency was about 8 per cent when water was boiled in kettles.

Mr. R. D. Archibald: I do not think the disc fairly represents the case of heating water, the circulation of which would cause the temperature to be more uniform than in the case of a disc. For a given average temperature the bottom of the water vessel would be lower than that of the disc, and the rate of transfer of heat greater. For the comparison of different heaters, discs of the same thermal capacity would appear to be best, but the variation in the diameter of the plates involves the use of discs of different thicknesses and shapes. It would be interesting to know how far this affects the question. Have the authors made any tests on other than electrical ovens, and, if so, how do the conditions compare? Would it not be a good plan to compare ovens when in actual service? The circulation of the air must be quite different in these circumstances. The authors bring out several points in connection with heat radiation and convection. There appears to be much general ignorance of these phenomena. For example, one finds steam- or water-heated radiators often coated with gold or aluminium paint, which provides the worst surface for efficient radiation. I have even seen totally-enclosed motors treated in this way. Fig. 12 shows what a large effect this can have on the emission of heat at quite low temperatures.

Mr. D. H. Bishop: It is obviously very difficult to devise tests to represent accurately the conditions under which the apparatus will be used in practice. Too much importance can hardly be attached to this, in view of the influence on future development. Apparatus may be specially devised to fulfil the test without being an improvement from the cook's point of view. How do blue planished steel plates compare with bright metal as regards radiating power? I am alluding to the steel sheets used for lagging turbines and steam pipes, and not so much to the cooking apparatus. Is it worth while fixing them on steam pipes, in addition to the ordinary lagging?

Mr. J. S. Thomson: Some time ago I went into the whole problem in my own house, and for over two years everything has been done electrically. The results have been very satisfactory from the reliability point of view. I think designers of cooking apparatus and manufacturers should be compelled to do all the cooking in their own homes electrically. In a few years' time we should then, I think, have very reliable apparatus. In my own case I find the consumption to

be about 1 unit per day per person for cooking for a household of 8 persons. The annual consumption is 3 000 units for cooking, and 200 units for lighting.

Mr. H. Richardson: We shall never be able to make the fullest use of the supply until we have two things, first, a very cheap supply of electricity; and secondly, thoroughly reliable and satisfactory apparatus. This is improving rapidly, but I should like to suggest to manufacturers that, in order to obtain an ideal apparatus in future, they should fit up their own houses completely. In regard to the type of oven, I would suggest that the frame of the oven should, first of all, be standardized in six different sizes, and then the elements in those ovens should be standardized, in so far as their overall dimensions and their contacts are concerned. When we once get such an apparatus, the elements, if broken, can easily be replaced from the nearest ironmonger's shop. There is sufficient scope for competition and originality between manufacturers in the endeavour to perfect and sell the best types of elements.

Mr. G. Fowler: The tests shown in Table 1 bear out the chief complaint received from consumers, i.e. that the speed of boiling liquids is much too low, and it is surely not beyond the bounds of possibility for manufacturers to increase the loading on these hot-plates. The time for boiling a 2- to 3-pint kettle should be 8 to 10 minutes. The present hot-plate takes 15 to 20 minutes, and it must be admitted that this is a very poor performance compared with that of the gas-ring. The radiant open-type appears to give better results. Referring to the oven, if we take the last three examples shown in Table 4, the average time taken for raising the temperature to 205° C. is roughly 30 minutes. I think it will be agreed that unless we can get an oven to reach that temperature in half that time, it should not be put on the market at all. The question of standardization is certainly being taken up, but the British Engineering Standards Association actually specify circular hot-plates. In the case of ovens, I do not consider this to be a good policy, and I see no reason why rectangular hot-plates should not be more generally used, as it would tend to standardize the size and type of elements throughout the oven. In regard to thermal insulation, in a recent paper a well-known authority on electric cooking made the somewhat startling statement that, in cooking a joint of meat of average weight in an oven, only about 20 per cent of the heat was absorbed in the joint, the rest being wasted, and he thought the thermal insulation of the electric oven was a rather important point which should receive the attention of manufacturers, with a view to utilizing the waste heat.

Dr. E. Griffiths and **Mr. F. H. Schofield** (*in reply to the Newcastle and Dundee discussions*): In reply to Mr. Ward, we regret to say that a dome-type oven was not included in the series sent us for test, and we are interested to hear that an oven of this type is being manufactured commercially.

As regards Mr. Archibald's remarks, we would point out that the discs left nothing to be desired in respect of uniformity of temperature. Exploration by means of thermo-couples attached to various points proved that the inequalities were of negligible magnitude.

In reply to Mr. Bishop's inquiry concerning planished steel plates, it may be assumed that if the thickness of insulation is adequate, then the nature of the surface has very little influence on the heat loss. The temperature gradient takes place through the lagging, and the temperature of the surface should be very little above that of the surrounding atmosphere.

PROCEEDINGS OF THE INSTITUTION.

656TH ORDINARY MEETING, 28 APRIL, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 14th April, 1921, were taken as read, and were confirmed and signed.

The President announced that the Institution of Civil Engineers had invited the members of this Institution to attend the presentation by the Right Hon. A. J. Balfour, P.C., F.R.S., O.M., of the Kelvin Gold Medal to Dr. W. C. Unwin, on Wednesday, 4th May.

Messrs. F. B. O. Hawes, P. M. Baker, and E. Nash were appointed scrutineers of the ballot for the election of new Members of Council.

Professor J. T. MacGregor-Morris and Mr. R. O. Kapp were appointed scrutineers of the ballot for the election and transfer of members and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Braid, George Gladman.
Carter, William Gilbert, Major, M.C.
Denton, Francis Medforth.
Henshilwood, George.
Medlyn, William John.
Polyblank, William Joseph, Lt.-Col., O.B.E.
Ramsay, Magnus.
Rickard, Charles Ernest, O.B.E.

As Associate Members.

Alexander, Douglas John, B.Sc. (Eng.).
Aspinall, Henry Oswald.
Barradell, David.
Bewsher, Harold William.
Billingham, George William.
Boak, Claude William.
Chambers, Henry Montague.
Chick, James Henry.
Clark, Edwin Fowler, B.A., B.Sc. (Eng.).
Cobbold, Hazen Nevill H., B.Sc.
Cresswell, Frank Pridgeon.

As Associate Members—continued.

Harrison, William Lorrieux.
Hetherington, Thomas.
Holroyde, Arthur Gordon C.
Hunter, George.
Joyce, Thomas Charles.
Ogilvie, William.
Powning, William Henwood.
Reddrop, William Howard.
Rothwell, Arthur.
Smith, Joseph Irvin.
Teggins, Albert William.
Wilson, Sidney Smith.
Wright, Archibald.

As Graduates.

Baxter, Frank Sladdin.
Bennett, Arthur William.
Coles, Arthur William H.
Cree, John Cherry.
De Cousinery, Edouard.
Dennis, William Hugh.
Desborough, John William H.
Ferro, Thomas Edward.
Greensmith, Albert Cecil.
Gwynn, Percy Harold.
Hay, Gordon Clemens.
Howell, Arthur Hendrick L.
Kalapesi, Mino J.
Pender, Daniel.
Randall, Samuel Gibson.
Rogers, Harold Belton.
Smith, Clarence Herbert.
Thomas, Hubert Jasper S.
Venkataram, Visvanatha.
Wilford, George McLean.

As Students.

Abell, Robert Henry.
Adams, Percy Rickman.
Bearcroft, Hubert Percival.
Beebee, Arthur Thomas S.
Benham, Kenneth Gurnett.
Blackman, George William.
Brereton, Allen Sadleir.
Brown, William.
Brown, William Charles J.
Brown, William Cuthbert.
Cash, Harry Hutchinson.
Childe-Freeman, Edwyn Ralph.
Colebrook, Hector Frank.
Collier, Percy Clifford.
Crocker, Walter Albert.
Curtis, Herbert Francis.
Davies, Bertram Jones.
Dickinson, James.
Dove, John Harrison.
Driscoll, Leo Frederick A.
Drew, Edward Dundonald.
Eadie, John Christian.
Elliott, Harold John.
Fleming, John Gavin.
Forster, Harold Oswald.
Gradwell, George Alfred.
Greer, Frederick John.
Harper, William.
Henderson, George Parker.
Henman, Harold.
Hopkins, Howard Roy.
Howard, Douglas Alan.
Hunter, James Ambrose.
Jeffs, Leslie.
Johnson, Rowland Alfred.
Jones, George Frederick.
Jul, Julio.
Keene, Leonard William.
Lamb, Leslie Herbert.
Lloyd, Harry.
Lunt, John Winstanley.
McDonald, Robert Edgar W.
Mooney, Allan McLeod.
Mullane, Antony Joseph.
Oades, Wilfred.
O'Dell, Hubert John S.
Paley, Frederick Raymond.
Pike, Cyril William.
Postlethwaite, Henry Victor.
Pope, John Wilson.
Price, Henry James.
Ratcliff, Reginald Pickworth.
Robson, Charles William.
Rochford, Sidney William.
Rowbottom, John Radcliffe.
Rowland, Frank Ernest.

As Students—continued.

Schofield, Tom Graham.
Sims, Charles Noel.
Smith, Harold Streeter.
Stokes, James Armitage.
Swan, Pearl Bathgate (Miss)
Swinney, John.
Terry, Robert Saint George.
Voigt, Paul Gustavus A.
Walker, Harry Parker.
Wardell, Percy Stewart.
Weston, Noel Cecil.
Willis, Arthur.
Wiseman, Tom.
Wood, Robert Henery.
Wood, Thomas.
Youngman, Robert Talbot.

As Associate.

Hudson, Noel, Lt.-Col., D.S.O.

TRANSFERS.

Associate Member to Member.

Coxon, James.
Cummins, Charles Philip C.
Eames, Alfred James.
Elliott, John Robert M.
Fletcher, John Francis.
France, William Masterman.
Gomersall, Edward, O.B.E.
Handcock, Ernest Charles.
Herbert, Thomas Ernest.
Hinings, Francis Sydney G.
Mackness, Cyril Francis.
Mallinson, Arthur Benjamin.
Murray, James King.
Pennington, William.
Smart, Edwin Victor.
Stretche, Thomas Edward P.
Turner, Edward.

Associate to Member.

De Lattre, Alfred Leopold.

Graduate to Associate Member.

Lintott, Harry Eugene.
Parikh, Jekisondas Mohanlal.
Philpott, Henry Edwin R.
Smith, Herbert.

Student to Associate Member.

Martin, Geoffrey Edward.

Associate to Associate Member.

Field, John Edward.
Gittins, George Edward.

Student to Graduate.

Best, Joseph Albert A.
Carr, Conway Buxton.
Jones, Victor Arnold.
Miller, Frederick McKenzie.
Shepherd, James Ernest.
Sherlock, John Richard.

After introductory papers had been read by Mr. J. R. Blaikie, Member, entitled "Electric Supply: Present Conditions and the Hopkinson Principles" (see page 701), and Mr. J. W. Beauchamp, Member, entitled "Multipart Tariffs for Domestic Electricity Supply" (see page 714) a discussion took place on the subject of "Tariffs" (see page 719). The discussion was adjourned at 7.45 p.m. to Thursday, 5th May, 1921.

667TH ORDINARY MEETING, 5 MAY, 1921.

(Held at the Institution of Civil Engineers.)

Mr. Ll. B. Atkinson, President, took the chair at 6 p.m.

The minutes of the Ordinary Meeting of the 28th April, 1921, were taken as read, and were confirmed and signed.

The adjourned discussion on "Tariffs" (see page 719) was continued and the meeting terminated at 7.45 p.m.

ADDRESS TO THE WESTERN CENTRE*

By Llewelyn B. Atkinson, President.

FOUR GREAT BRITISH ELECTRIC LIGHTS: FARADAY—THE DISCOVERER; MAXWELL—THE INTERPRETER; KELVIN—THE MEASURER; SILVANUS THOMPSON—THE TEACHER.

(*Address delivered at* Bristol, 2 *May*, 1921.)

In a science like our own which has sprung from almost entire ignorance within a space of less than 100 years, it is not amiss, when a certain phase has terminated, to try to review in some perspective the great figures which have taken part in that phase and to compare the parts which each of them has taken in it. In selecting the above four figures, I do not wish for one moment to suggest that they alone have contributed to bringing our science to the state of development it had reached some 10 or 15 years back; nevertheless, they do represent certain distinct parts of the whole, and the individuals' names were, in themselves, those of such outstanding characters that it is a fair generalization to state that they almost entirely made possible the advancement of electrical science which took place between, say, 1830 and 1910.

Michael Faraday—The Discoverer.

Michael Faraday was born in 1791 at Newington in Surrey, his father being a Yorkshire blacksmith who had moved from Ingleton, near Settle in Yorkshire, to the South of England. Michael Faraday appears to have had very little education in the ordinary sense, but he was eventually apprenticed in London to a bookbinder of the name of Riebau, and, when binding books that were brought to his master, he availed himself of the opportunity to dip into some of them, and in this way he was evidently able to absorb his first knowledge of scientific subjects. His mind thus inspired with the idea, he attended lectures on natural philosophy, and on one occasion one of his master's customers, named Dance, took Faraday to hear four lectures delivered by Sir Humphry Davy at the Royal Institution. He made copious notes of these lectures, which he transcribed, and then bound up and sent to Sir Humphry Davy, asking him to interest himself in obtaining for him some employment in scientific work. At the time nothing came of it, but a year later, Sir Humphry Davy's laboratory assistant leaving the Royal Institution, the post was offered to Faraday and accepted, at the magnificent salary of 25s. per week and two rooms in the Royal Institution.

* For the historical facts given in this Address the author has made free use of, and desires to acknowledge his indebtedness to, the following works:—
S. P. Thompson: "Michael Faraday, his Life and Work"; and "The Life of William Thompson, Baron Kelvin of Largs."
R. Routledge: "A Popular History of Science."
L. Campbell and W. Garnett: "The Life of James Clerk Maxwell."
J. S. Thompson and H. G. Thompson: "Silvanus Thompson, his Life and Letters."
Encyclopædia Britannica, Miscellaneous Articles.

The following year he accompanied Sir Humphry Davy, in the capacity of secretary, on a tour through France, Italy and Switzerland, meeting on this journey, which lasted nearly two years, nearly all the principal scientific men then in Europe. On his return he again took up his work at the laboratory, of which in 1825, on Sir Humphry Davy's retirement, he was appointed Director.

In 1833 Faraday was appointed Fullerian Professor of Chemistry at the Royal Institution, a life appointment without obligation to deliver lectures.

This is not the place in which to recount the whole of Faraday's work, because I wish to lay stress on his part as an electrical discoverer. His scientific researches on electrical work fall broadly into three periods. The first of these, lasting from 1816 to 1830, was mostly occupied with chemical work. The second, lasting from 1831 to 1839, covers the period of the experimental researches in electricity and magnetism which are indeed the foundation of our science. The third period, from 1860 to 1862, also covers further electrical researches, immensely important in themselves, but not of the importance that this second period covered. Between 1839 and 1844 Faraday had been obliged to suspend serious work owing to a breakdown of his health, and the last series of his investigations came to an end for the same reason.

Let us imagine ourselves at the starting-point of Faraday's electrical work. In 1820 Oersted had discovered, it is generally believed, by an accident in the course of one of his lectures, that a compass needle was acted upon by an electric current in a wire. But there is no doubt that Oersted had been looking for some connection between the electric current and magnetism, although up to this date he had not found any.

The announcement of this discovery, in Faraday's own words, "burst open the gates of a domain in science, dark till then, and filled it with a flood of light."

Ampère, Laplace and Davy added very rapidly to a full knowledge of the reaction between magnets and electric circuits, and between electric circuits themselves.

Dr. Wollaston, having suggested that there ought to be a tendency when a magnetic pole was presented to a conducting wire for that conducting wire to revolve around its own axis, went to the Royal Institution in April 1821 to make an experiment. He did not realize

the result he expected, but Faraday's attention having been thus called to this point, he went to work and discovered that a wire included in an electric circuit hanging vertically over a magnetic pole with the lower end dipping into a pool of quicksilver would rotate around the pole of the magnet, or, conversely, if the wire were fixed and the magnetic pole free to move, the latter would rotate around the former. The various modifications of this experiment were subsequently contrived. In this device we have the first electric motor, being what we should now call of the homopolar type.

This was the only electrical discovery of importance in what has been termed the first period of research, but doubtless his mind had been occupied from the year 1823 onwards in pondering on electrical matters, and in the meantime Sturgeon had invented the soft-iron electromagnet in which an iron core surrounded by a copper wire is magnetized when a current passes. Faraday was firmly convinced that a reciprocal action, that is to say, the conversion of magnetism into electricity, could be effected, and even in 1822, following his habit of putting down in his notebook thoughts which crossed his mind and which he proposed later to experiment on, he had written these words: "Convert magnetism into electricity."

We have to bear in mind that at this date the general doctrine of the conservation of energy had not been formulated in any rigid manner, much less proved, and, consequently, these ideas arose from certain intuitions in Faraday's mind that such a result, whilst to be expected, was not a necessary consequence, though to-day, given the one result, the other follows as a mathematical certainty.

Many were the experiments which Faraday recounted of his efforts to produce electric currents from magnets or from coils forming electromagnets, and from 1824 to 1828 he was still relentlessly attacking this problem. In 1831, for the fifth time, he made a frontal attack on the problem, this time with a ring magnet wound with two sets of coils, the one set connected to a battery, the other to a galvanometer, and with this apparatus success was at last achieved, but not in the way he expected. What he and others had been looking for was a steady current produced by the action of magnetism on a coil, just as magnetism is produced and maintained in soft iron by a steady electric current. What he found was, as we now know, the current set up in the second coil when the current in the first coil was started or stopped. Once having achieved this result, he was able to produce the same effect by magnetizing the soft iron core inside the coil by permanent magnets, and a few days later, a copper disc forming part of an electric circuit, the axis and periphery of the disc connected with the galvanometer being placed between the poles of the great horseshoe magnet of the Royal Society, on being revolved gave a steady current which deflected the galvanometer, and thus the magneto-electric generator or, ultimately, the dynamo-electric generator, was born. This last frontal attack with all its splendid results lasted only 10 days. The story of it is given in his "Experimental Researches" and forms a monument to his immense perseverance and, moreover, to his extraordinary intellectual insight.

Following these results and arising out of them, we come to Faraday's discovery of the mutual induction between electric currents in one circuit and those in another circuit, and that the starting, stopping and variation of a current in one wire give rise to transient currents in a neighbouring wire. It was at this time that he first used that idea which in his hands and others was to prove so fruitful, i.e. the idea of the conductor cutting the magnetic curves. He explains: "By magnetic curves I mean lines of magnetic forces which would be depicted by iron filings." These lines of force have been a powerful aid to our understanding of electric and magnetic phenomena.

They may be shown mapped out by iron filings on magnets, and a wire carrying an electric current attracts iron filings on the card threaded on the wire—facts discovered by Davy. The circles show the lines of magnetic force, whilst a circular conductor in section illustrates how these lines form circles around each conductor and through the centre of a ring or coil.

All the results which Faraday discovered as to the induction of circuits and of magnets, whether by their mutual motions or by changes in the currents, he was eventually able to summarize in that idea which has never been replaced by any better generalization: the idea that an electromotive force is set up in a circuit and a current produced if the circuit is closed, whenever the circuit cuts the lines of force of a magnet existing in the neighbourhood of a circuit carrying a current.

In the series of discoveries which I have outlined, starting from the original discoveries of Oersted and Ampère, Faraday had completed our knowledge of electro-dynamics.

In 1833 Faraday published another series of researches on electro-conduction, giving an extraordinary collection of observations on the conducting power of liquids, solids, fused salts and other bodies, interesting even at this date in themselves, but important in particular as leading to the study of what we now know as electrolytic conduction, and here again Faraday achieved results of the most far-reaching consequences. He discovered that the amount of decomposition of electrolytic bodies with definite electric quantities was proportional to the electro-chemical equivalents of the substances being acted upon, or direct multiples of these equivalents, and he was led to the following statement:

"According to it [this theory] the equivalent weights of bodies are simply those quantities of them which contain equal quantities of electricity or have naturally equal electric powers, it being the electricity which determines the equivalent number because it determines the combining forces. Or if we adopt the atomic theory, or phraseology, then the atoms of body which are equivalents to each other in their ordinary chemical action have equal quantities of electricity naturally associated with them."

There, in fact, is stated the doctrine of the atomic charge, or that "electron" which all the investigations since that date have established as the unit of electric

action, and which, we may certainly say to-day, is the foundation of the atom.

In 1835 Faraday considered the question of whether electricity resides upon the surface of the conductor or upon the surface of the dielectric in contact with it, and he arrived at this conclusion :—

" Electricity appears to exist only in polarity as in air, glass, electrolytes, etc. Now metals being conductors cannot take up that polar state of their own power, or rather retain it, and hence probably cannot retain developed electric forces."

Here again, Faraday had reached, in the course of a few weeks' experimental work, the great truth to be developed by those who followed him, i.e. that all the phenomena of electrostatics lie in conditions set up in non-conducting or dielectric materials and not as some emanation from the boundaries of the conductor.

Omitting many important researches, I will now come to the third period of Faraday's activity when, after a period of rest, during which fresh ideas were doubtless being organized in his mind, he attacked the problem of whether there was any action or connection between electricity or magnetism and light. Ordinary light consists of undulations in the ether in all planes along the direction of the light ray, but, previous to Faraday's time, the phenomenon of polarized light, or light vibrating in one plane only, had been discovered, and moreover it had been found that such light when passed through substances having a crystalline structure, or in which by applied stresses strains are set up, has the property of rotating the plane of the vibration. It was with polarized light that Faraday commenced his experiments. He worked first with liquid electrolytes through which the electric current was made to pass, but without result. He tried numerous solid dielectrics under electric stress to see if they would give him any result. In every possible way were electric stresses set up in these materials, and pages of his notebook are filled with negative results. Then he substituted magnetic for electric forces and again worked upon air, flint, glass, rock crystal, calcareous spar, etc., but with no result. At last he tried a piece of " heavy glass " (boro-silicate of lead), a glass which he himself, after four years of work in his early scientific career, had developed for optical purposes, and at last, using a powerful electromagnet so arranged that the lines of force passed along this glass in the same direction as the polarized light, a movement of the plane of polarization was observed. Experiments here show that the result was only obtained in so far as the light ray passed along in the same direction as the lines of magnetic force. Having achieved this result, he obtained a more powerful electromagnet and was then able to produce the effect, even with some of the substances with which he had hitherto failed. Thus he established that there is a definite connection between magnetic phenomena and light, a connection which we shall see later became so fruitful in the hands of Maxwell, but more than that, arising out of these experiments and out of the use of the bar of heavy glass already mentioned, and of the powerful magnet which he had borrowed for the purpose of the foregoing experiment, he made a further great discovery, that of diamagnetism, a property of many materials by which, instead of being attracted like iron filings, they are repelled from a single pole of a magnet or, if in the form of a bar or needle, set themselves across the lines of magnetic force instead of along the lines of magnetic force as in the case of iron.

Among all these experimental discoveries we find in Faraday's work his inability to accept the ideas, then current, of action at a distance, that is to say, the idea that forces could exist between the material substance without any intervening substance through which the action was to be transmitted, and in this relation he constantly expressed his results in terms of lines of magnetic force, viewing them as something physically existent but acting he knew not how, and of which he used the following phrase :

" Such an action may be a function of the ether, for it is not at all unlikely that if there be an ether it should have other uses than simply the conveyance of radiation."

The outstanding feature of all Faraday's work lies in the power which he possessed of visualizing, in some way that seemed peculiar to himself, possible actions and interactions which he put to the test of experiment. That these visualizations produced in his mind great certainty as to the results, is clear from the way in which he persistently followed a given line of thought, returning again and again by new experiments until he had arrived at the result which he foresaw.

Faraday, as will be shown later, was not only no mathematician in the ordinary sense, but was practically ignorant of mathematical methods. The forecasting of new results of mathematical processes and their subsequent examination experimentally is a procedure to which scientific men have long become accustomed, but Faraday substituted for that mathematical process some mental process of his own with a power of insight which has probably never been equalled by any other true experimenter, before or since.

Let us summarize some of his discoveries in electrical science :

(1) The possibility of an electric motor, or continuous movement arising from electric current and magnetic fields.

(2) The induction of currents between conductors across an intervening space.

(3) The production of electromotive forces and electric currents by the mutual movement of electric conductors and magnetic fields.

The electric motor, the transformer and the induction coil, and the dynamo, lie in these discoveries.

(4) The electric valency of substances in electrolysis, and the realization of the electric unit in which much of our present electrolytic knowledge and our insight into the structure of the atom and the first suggestion of the electron were contained.

(5) The action of the magnetic field in rotating the plane of polarized light, in which were the germs of the electromagnetic theory of radiation and all that is therein involved.

(6) Diamagnetism, i.e. the repulsion of bodies under magnetic force, which leads to the strong presumption

of the truth of Ampère's hypothesis of the cause of magnetism as lying in the molecular electric currents circulating in the compound atoms or, as we now view it, in the rotation of the electron in the atom of which it forms a part.

Truly, indeed, are we entitled to regard Faraday as the Discoverer.

Maxwell—The Interpreter.

James Clerk Maxwell was born on 31st January, 1831, at the period when Faraday was commencing his second series of researches, his father being John Clerk Maxwell of Penncuick, Midlothian, a lawyer owning the estate of Middlebie, and it was on this estate to a large extent that Maxwell was trained. In very early youth he showed signs of great ability and, his mother having died when he was nine years old, he was encouraged in early nature studies by his father, who himself appears to have had a good understanding of scientific subjects.

When Maxwell was 15 years of age he had already commenced original studies on geometrical problems, and the results were communicated to the Royal Society of Edinburgh and appeared in the Proceedings of that Society as a communication from Professor Forbes of a paper by Mr. Clerk Maxwell, Junior.

In 1847 at 16 years of age he entered the University of Edinburgh, having already done much original work in mathematics and mathematical physics, and by 1849, when he was 18 years of age, he had already carried out original experimental work on polarized light, properties of crystals, etc.

At the age of 19 he entered Trinity College, Cambridge, and in 1854 disappointed his tutors by becoming second wrangler, Routh gaining the honour they had hoped for him, of being the senior wrangler. The two were declared equal in the examination for the Smith mathematical prize.

In 1855 he wrote "I am reading electricity," and again, "I am getting on with my electrical calculations every now and then and working out anything that seems to help the understanding thereof."

In 1857 we find him in correspondence with Faraday.

From 1860 to 1865 Maxwell was at King's College, London, as Professor of Physics, a great deal of his work there being optical, but nevertheless notable for the experimental measurements in which he took part with Balfour Stewart and Fleeming Jenkin in the determination of the British Association standard ohm.

From 1866 to 1870 he carried out experimental work in his country home in Scotland, and it was during this time that much of his "Treatise on Electricity and Magnetism" was written. It was not until 1871 that he was appointed the first Cavendish Professor in the University of Cambridge, his first task being the creation of the experimental Cavendish Laboratory where he and others have made such notable discoveries.

By 1860 Maxwell had already commenced planning in his mind the great work of electricity and magnetism which was to revolutionize electrical theory.

In 1861 he wrote to one of his correspondents:

"I am trying to form an exact mathematical expression for all that is known about electromagnetism, without the aid of hypothesis, and also if the variations of Ampère's formulæ are possible without contradicting his expressions." In the first of these tasks he was successful, but as to the second he arrived at the result that Ampère's formulæ could not be varied without contradicting the experimental results.

His ideas on this subject may well be quoted from the preface to the first edition of his "Treatise." He says "the general complexion of the 'Treatise' differs considerably from that of several excellent works published, most of them in Germany, and it may appear that scant justice is done to the speculations of several eminent electricians and mathematicians. One reason of this is that before I began the study of electricity I resolved to read no mathematics on the subject until I had first read through Faraday's experimental researches on electricity. I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that neither he nor they were satisfied with each other's language. I had also the conviction that this discrepancy did not arise from either party being wrong. I was first convinced of this by Sir William Thomson, to whose advice and assistance, as well as to his public papers, I owe most of what I have learned on the subject. As I proceeded with the study of Faraday I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols. I also found that these methods were capable of being expressed in the ordinary mathematical forms and thus compared with those of the professed mathematician. For instance, Faraday in his mind's eye saw lines of force traversing all space, where the mathematician saw centres of force attracting at a distance. Faraday saw a medium, where they saw nothing but distance. Faraday sought the seat of the phenomena in real actions going on in the medium; they were satisfied that they had found it in a power of action at a distance impressed on the electric fluids. When I had translated what I considered to be Faraday's ideas into a mathematical form, I found that in general the results of the two methods coincided, so that the same phenomena were accounted for and the same laws of action deduced by both methods, but that Faraday's methods resemble those in which we begin with the whole and arrive at the parts by analysis, while the ordinary mathematical methods were founded on the principle of beginning with the parts and building up the whole by synthesis. I also found that several of the most fertile methods of research discovered by mathematicians could be expressed much better in terms of ideas derived from Faraday than in their original form." He continues: "I have confined myself almost entirely to the mathematical treatment of the subject, but I would recommend the student after he has learned, experimentally if possible, what are the phenomena to be observed, to read carefully Faraday's experimental researches in electricity. He will there find a strictly contemporary historical account of some of the greatest electrical discoveries and in-

vestigations carried on in an order and succession which could hardly have been improved if the results had been known from the first, and expressed in the language of a man who devoted much of his attention to the methods of accurately describing scientific operations and their results."

Such then was the task which Maxwell set himself.

The translation of Faraday's experimental researches into a mathematical form led to the discovery that, having done so, the resulting statement of electrical phenomena is substantially identical with the statement of a mechanical and dynamical science. Maxwell utilized the fact to reduce the science to one of measurement and prediction, and the calculation thereby of the conditions arising in electrical and magnetic phenomena in regions where no experimental work had hitherto been done. This again led to the discovery of the definite relations between electric and magnetic forces and, finally, the representation in concrete form of a mechanism of the ether and a mechanism of dielectrics which complied with all that Faraday and others had shown experimentally in respect of the lines of force and stress of the dielectric medium. This led finally and as a culminating triumph to the electromagnetic theory of light, forming the basis on which the whole question of electromagnetic radiation and, in a practical way, wireless telegraphy has been built up.

It is interesting in relation to Maxwell's exposition of Faraday's work to read what Faraday himself wrote to Maxwell in 1857.

"There is one thing I would be glad to ask you. When a mathematician engaged in investigating physical actions and results has arrived at his conclusions, may they not be expressed in common language as fully, clearly and definitely as in mathematical formulæ? If so, would it not be a great boon to such as I to express them so? Translating them out of their hieroglyphics that we also might work upon them by experiment. I think it must be so, because I have always found that you could convey to me a perfectly clear idea of your conclusions which, though they may give me no full understanding of the steps of your process, give me the results, neither above nor below the truth and so clear in character that I can think and work from them. If this be possible, would it not be a good thing if mathematicians working on these subjects were to give us the results in this popular, useful working state as well as in that which is their own and proper to them?"

Faraday asked a question which has been asked many times. I think the answer may be found in Maxwell's own work. It is true that most of the investigations in his "Treatise" are mathematical, but nevertheless he sets out as the work proceeds both the experimental data on which he proposes to operate mathematically and the results obtained, and in this sense indeed not only was Maxwell a mathematical operator on the experimental results of Faraday, but he clearly expounded the further results which inevitably followed those discoveries.

Faraday's work on the relation of electromagnetic phenomena and light was doubtless the starting-point of Maxwell's investigations therein, and it was by working from these experimental results that Maxwell was led to the conclusion that light and other radiations such as heat are, in fact, electromagnetic disturbances in the ether, and that the velocity of the propagation of an electromagnetic disturbance should be the same as that of light. He showed that the velocity of transmission of electromagnetic disturbances varies inversely as the square root of the specific inductive capacity, and also inversely as the square root of the magnetic permeability of the dielectric. The proof then of the truth of his theory would lie in comparing the refractive indices of substances for light, which depend on the velocity of transmission of light, with the specific inductive capacities of the same substances, and many experimenters set to work on this question. In the result it was established, with some notable exceptions, that for light of infinite wave-length the specific inductive capacity of a dielectric is equal to the square of its refractive index, and there seems no reason any longer to doubt the truth of Maxwell's proposition. Definite mechanical actions in dielectrics and in the all-pervading medium which would explain all the observed facts of electrical and magnetic phenomena were put forward by Maxwell, with the result that it was no longer necessary to entertain the theory of action at a distance, and the point of view of Faraday was definitely established.

The views of Maxwell and his treatment of electromagnetic theory found for some years but little acceptance by the scientists of other nations. The English school of electrical theory indeed was a thing apart, and it was not until the experimental work of Hertz from 1885 to 1887, when there was placed absolutely beyond any further question the identity in the method of propagation of electromagnetic and light waves, that Maxwell's theories and the English school had triumphed.

Thus had Maxwell interpreted Faraday, had completed and crowned as Interpreter the great work of Faraday as Discoverer.

Kelvin—The Measurer.

William Thomson (afterwards Lord Kelvin) was born in June, 1824. His father James Thomson was of Scotch descent, but his family for 200 years had occupied a small farm at Ballyn-a-huish, and his father was brought up on the land as a farm labourer, receiving from his parents the rudiments of his education. But with the Scotch instinct for education he studied himself, and among other things the art of "dialling," or making sundials. It is related of him that when he was 11 or 12 years old he discovered how to make dials suitable for any latitude. He subsequently studied under the Presbyterian minister near his home, and later became a school teacher at his native place, spending six months of the year at Glasgow University. In 1814 he became lecturer on mathematics at the Royal Belfast Academy, and in 1815 was made Professor of Mathematics in the college.

Such was the father of William Thomson, who was the fourth child. If there be anything in heredity, surely it was likely that the son of such a father would exhibit exceptional gifts. When he was 8 years old

his father became Professor of Mathematics in Glasgow University, his mother having died two years earlier. Just as in the case of Maxwell, who lost his mother at an early age, the effect of this was a closer contact between father and son than might otherwise have been the case, and undoubtedly this fact had a great bearing on his early training.

In 1834, at 10 years of age, he matriculated in Glasgow University. It is almost unbelievable that a child could undertake the tasks which he performed. As a holiday task for which he received a prize he translated from the Greek Lucien's "Dialogues of the Gods," with full parsing of the first three dialogues. At 16 he received a prize in astronomy and a university medal for an essay entitled "On the Figure of the Earth." As time does not permit me to expand the theme of the intellectual brilliance of this youth, let us see how his attention was turned to electricity and magnetism. There was with him at the university one David Thomson, a cousin of Faraday, and it was by him, Thomson himself stated, that he was inoculated with Faraday's fire. He preached to William Thomson, Faraday's doctrine of action through a medium along lines of force as against the older mathematical doctrine of action at a distance, and finally William Thomson was converted. In 1841 Thomson, now in his 17th year, entered Peterhouse, Cambridge. At 18 years of age he had written and published original mathematical investigations on the flow of heat and distribution of electricity. The results, although it was eventually found that they had been anticipated, had been arrived at by different methods by three of the foremost mathematicians in Europe. But when the examinations arrived he, like Maxwell, failed to become first wrangler, being second and, again like Maxwell, he obtained the Smith mathematical prize.

In 1846 Thomson was appointed Professor of Natural Philosophy in Glasgow University, a post he held until his death in 1907, a period of 61 years. To catalogue the mathematical and physical investigations of this mental giant, the greatest since Newton, would take more than a lecture; problems and discoveries in heat, light, electricity, magnetism, Atlantic cables, cooling of the earth, etc.; there is no end to them; Silvanus Thompson in his biography of Kelvin gives a bibliography of 661 original papers and communications. It is stupendous. From the time he was 10 or 12 until he left us at 73 that colossal brain never stopped working. In 1866 on the completion of the Atlantic cable he received the honour of knighthood, and in 1892 was created Lord Kelvin.

But it is of Kelvin as measurer that I wish to speak. In the session of 1882–3 I attended a course of lectures at the Institution of Electrical Engineers on "The Practical Applications of Electricity," and the last of the course was by Sir William Thomson on "Electrical Units of Measurement." He commenced: "In physical science a first essential step in the direction of learning any subject is to find principles of numerical reckoning, and methods for practicably measuring some quality connected with it. I often say that when you can measure what you are speaking about and express it in numbers you know something about it; when you cannot express it in numbers your knowledge is of a meagre and unsatisfactory kind. It may be the beginnings of knowledge, but you have scarcely in your thoughts advanced to the stage of science whatever the matter may be." Then he recited the case of the hardness of materials expressible only in an empirical scale beginning with the diamond and ending—I forget with what. Then he went on to say that 10 years previously (in 1872) electrical measurement was little better. That in 1858 a practical beginning of definite electrical measurement had been made—standard resistances and condensers in absolute measure had been used on his own initiative in telegraphic and cable work, but these had hardly reached even the laboratories in 1872. He recited how, as early as 1871, he had commenced using the absolute system in the reckoning of electromotive forces of voltaic cells, and the electric resistances of conductors in electromagnetic absolute units, and in 1861 he obtained the appointment of a Committee of the British Association on electrical standards.

Earlier on I have spoken of Maxwell's work at King's College on the experimental determination of the ohm; all this work was initiated by Kelvin and carried out on lines designed by him, and after 10 years' work the absolute system known as the "C.G.S." was launched for general use.

Kelvin did not invent the absolute system; the idea had been put forward by Gauss, a German mathematician and physicist. I should like here to say a few words on the subject of absolute measurement, because a clear understanding is essential to engineers, but engineers are to-day in the position of having such a complete equipment of direct-reading instruments put into their hands that they have ceased to inquire or know how they have been arrived at. But the last generation of electrical engineers, to which Kelvin belonged, had none of these things and were very much occupied in trying to get them. The measurements and calculations of mechanics and dynamics, that is, of matter at rest and matter in motion, can all be compassed by three fundamental units, a unit of length, a unit of mass or quantity of matter, and a unit of time. There is no relation between them and they are all purely arbitrary, and the advantage or disadvantage of any particular units for these quantities lies only in the extent to which they are universally used, with some other advantages or disadvantages arising from the method of connecting them with one another in the compound units. The discussions on the comparative advantages of the British and metric units lie in these latter considerations; both start with purely arbitrary units. The British units are:—of length the distance between two marks on a platinum bar at a certain temperature—we call it the yard; the quantity of matter is the matter in a piece of platinum; the weight or force with which it is attracted to the earth is called a pound; and the unit of time is the 1/86 400th part of the mean time of the rotation of the earth (mean solar day)—we call it a second.

The metric system has a different bar and marks and the distance is called a metre, a different piece of platinum the weight of which in Paris is called the

weight of a kilogram, and the same unit of time. But before Kelvin's time, although the dynamical units were understood, scientific and engineering measurements were made in arbitrary units. Thus, forces were measured by the attractions in London and Paris respectively for the aforesaid pieces of platinum, work done by the number of times such a piece of platinum would be raised one unit of length against gravity in a second, and so on. Electrical units were more arbitrary. Electromotive force was specified as being equal to so many Daniell or other cells. Resistances were specified in lengths of copper wire of unspecified properties of a specified size, and current in degrees of deflection of the owner's own galvanometer. It was from this state of chaos that first Gauss and then Kelvin extricated us. Starting with units of length, mass, and time, the unit force was defined as that which, acting on unit mass for unit time gave it unit velocity, that is, an absolute measure independent of place. With these preliminaries Kelvin showed how to build up a complete system of units and measures for all electrical quantities though one or two natural physical quantities intervene. It is true that the apparent number of fundamental units in electrical work is three, as in mechanical work, but there are really four, three arbitrary and one implied. Kelvin himself reminded us that Gauss had already pointed out that only two fundamental units are necessary for mechanical measurement if we imply one. Thus, if we define the unit of force as the force with which at unit distance a particular mass attracts another equal mass, we are bringing in a special property of matter, i.e. its attractive property. If we define the unit of force as that necessary to give a certain unit mass a unit velocity in a unit time, we bring in another property inherent in matter, its inertia, so in each case we really involve something besides the apparently arbitrary quantities of the units. So in electrical units, the existence of electrical states at rest (statical) is known only by the mechanical forces which exist. The existence of electricity in motion (electric current) is shown either by forces exerted or by heating and chemical effects. If we decide to measure by heating and chemical effects we must choose some material substance, the action not being the same on all. Consequently, even in the case of electric currents we have chosen to select mechanical effects which can if necessary be measured *in vacuo*, by which means special substances can be eliminated.

But we must decide whether we are to start by measuring forces arising from electrostatic attractions, or the forces arising from electrodynamic attractions. By choosing the former we arrive at a series of units we call electrostatic units; by choosing the latter we obtain a series we call electromagnetic units.

In the first our primary unit is the quantity of electricity which at unit distance in air attracts an equal quantity of electricity with unit force. Concentrating this quantity on a sphere of unit radius at a distance from surrounding bodies in air, we raise it to unit potential above the surrounding bodies. From these we derive all the other units.

In the electromagnetic system we start with unit force between two magnetic poles at unit distance, and unit current is that which flowing in a unit length at unit distance exerts unit force on a unit pole. The unit electromotive force is that which produces such a current in a unit resistance, and so on. It was the measurement of this resistance which occupied so much of the attention of the Electrical Standards Committee.

It is not difficult to understand the principle. If we have a coil rotated in a magnetic field an electromotive force results; if the coil is a closed circuit a current is produced alternating in direction in the coil, but giving a rising and falling current always in the same direction in space.

If in the centre of the coil we hang a small magnet it is deflected by the current. It is held to its place by the earth's field, but the electromotive force and current also depend on the earth's field, so that we are able finally to express the resistance of the coil as a quantity dependent on the velocity of rotation of the coil. Various corrections for the field of the small magnet, and so on, have to be made. Thus is the unit of resistance determined. The rest follows.

These two sets of units, the electrostatic and the electromagnetic, being based on two different properties of space and matter, the first on the potential energy existing in the dielectric space between charged bodies, and the second on the kinetic energy of a magnetic field, are not related to one another by a constant multiplier, but it was shown by Maxwell that they are related to one another by some power, positive or negative, of a number which is the ratio of the square root of the elasticity to the square root of the density of a medium. But this ratio is the velocity with which a transverse vibration is transmitted in the medium, that is, it is the velocity of light—a deduction which has since been experimentally proved.

Kelvin not only started this work, but he it was who inaugurated the British Association Committee; he it was also who finally suggested the three primary units, the centimetre, the gramme, and the second, and it was largely his influence which obtained international sanction for this absolute system. The electromagnetic system was adopted for general use, but the units were of inconvenient size. The unit of current was about right, but the units of electromotive force and resistance were far too small. So other units were adopted as follows: A unit of electromotive force equal to 10^8 C.G.S. units; a unit of resistance equal to 10^9 C.G.S. units; and a unit of current equal to 10^{-1} C.G.S. units. The series was completed and forms a complete and consistent whole in which the unit of length is 10 million metres, the unit of mass one hundred-thousand millionth of a gramme, and the unit of time one second.

Kelvin himself was the author of a series of very beautiful instruments for making measurements. The absolute electrometer is one, in which the actual attraction is weighed. The quadrant electrometer is another, in which a needle is drawn between quadrants connected to the potential terminals to be measured, and deriving great sensitiveness from the fact that the needle can be charged from an independent source to a known

and high difference of potential from the earth. The mirror galvanometer, the ampere balance for the direct weighing of current attractions, and many others all attest to his intense practical interest in carrying out the measurements he had devised. It is the simplicity and extreme accuracy of measurement which has differentiated electric science from almost every other. The way was pointed by Kelvin.

In the 1883 lecture with which I commenced, Kelvin pointed out that there are, in addition to those natural constants, gravitational attractions, dielectric elasticity and electromagnetic force, to which I have alluded, other natural constants which might well be used for determining absolute physical units, and it may be that such will eventually be the units. For example, the second is by no means an ideal unit; it is becoming longer as the time of rotation of the earth increases under the influence of tidal friction; it is conceivable that an air raid might destroy the physical metre and kilogram on which we rely. But the mass of an atom of hydrogen, the length of a wave of light, say of one of the sodium lines, the velocity of light *in vacuo* giving us the time of oscillation of a sodium atom, with the electron as the unit of electric quantity; these are all material and recoverable and unchangeable quantities, and are those in which our present units will be recorded never to be lost or destroyed, but even if they were it would not undo all that has been given to us by Kelvin the Measurer.

Silvanus P. Thompson—The Teacher.

It must, I think, have been in 1884 when first I saw Silvanus P. Thompson. I was a student in the evening classes of Finsbury College at that time and I remember one evening in the laboratory Professor Ayrton conducting round what struck me then as a small and precise gentleman who asked many questions, and whom I afterwards learned was Silvanus Thompson, then Professor of Electricity in Bristol University. That was his first visit to the College of which he afterwards became the Principal, and of which he was Principal when he died. But I am plunging into the middle. The ancestors of Silvanus Thompson were a Westmorland family of Morland, and his grandfather was, first, grocer and later banker of Appleby. It is interesting to observe that there is good evidence for thinking, from an observation of the family crests, that Silvanus Thompson and William Thomson were descended from a common ancestor. For some generations the Thompsons were Quakers. His father Silvanus Thompson, born at Appleby, unable because of his Quaker belief to enter Oxford or Cambridge, studied at University College, London, and became Master of the Quaker School for Boys in York in 1841 and in 1848 married a Tatham of Settle. Silvanus Thompson was the second child and was born in 1851. He early developed a taste for scientific experiments and his younger sister Maria was the dog on whom he experimented. Besides contriving cranes in which she was lifted—and, it may be added, not infrequently dropped—he had heard or read in a book of travels of the process of tattooing and in the nurse's absence had tattooed a star on his sister's wrist, rubbing in ashes from the fire-place. The result could be seen 40 years later.

In 1858 Silvanus Thompson entered Bootham School, a Quaker school, where he became a contributor to the school magazine, one of his first contributions being an article on electricity. At the age of 16 he sat for the London matriculation examination and in the same year entered the Quaker Training College at Pontefract, and in 1869 he obtained the B.A. degree of London University. In 1870 he took a post as junior master at Bootham School and thus began his teaching career. In 1875 he left this school and came to London, having gained a bursary at the Royal School of Mines, and during this time he attended lectures at the Royal Institution and other learned Societies. In 1876 he conducted with Dr. Guthrie some electrical researches which were communicated to the Royal Society in that year. It was in 1876 that, after he had applied for the post of Professor of Chemistry at Bristol, which he did not obtain, he applied for the lectureship on Physics and was appointed to the post, and in 1878, when a Chair of Physics was established, he was elected to the professorship and in the same year took his D.Sc. degree in London. It was now that he, having made a reputation as an investigator and as a lecturer, began his studies in the subject of technical education. He devoted several vacations to studying on the spot the technical and scientific educational systems of France, Germany and Switzerland, and his first public contribution to the subject was at the Social Science Congress at Cheltenham in 1878 on "Technical Education; where it should be given." He worked tremendously at this time on this subject, whilst at the Bristol University he was leading a most difficult and distasteful life. The funds of the college did not allow any real outlay on scientific apparatus. In 1880 he wrote: "My opportunities for working in physics are terribly circumscribed. My lecture room is used for all sorts of other lectures. My only laboratory is a damp cellar 11 ft. by 9 ft., etc." In 1880 he gave the inaugural lecture at the Mechanics' Institution, Nottingham, afterwards published as a pamphlet entitled "The Apprenticeship of the Past or the Future, or Trade Education for the Working Men of Nottingham." It was in 1880 that he began writing his book on Electricity and Magnetism, and in 1881 he married. In 1882 the University College, Bristol, came under new management and at last proper laboratories were fitted up under Thompson's guidance. Nevertheless, the opportunities for teaching science in the way he had conceived it should be taught were very limited, and it is not surprising that he was looking for much more extended opportunities for developing his ideas. He had been consulted by the City and Guilds Institute on the subject of a scheme for the Central Technical College, many of the ideas he put forward being embodied in the College which was opened in 1884. In 1885 when Professor Ayrton, then the Principal of Finsbury College, removed to the Central College Thompson was invited with others to apply for the post. This he did, and in 1885 at the age of 34 he became the Principal. He remained and worked there for 31 years. It was not a light task which he undertook. He was

Principal and Professor, Administrator and Teacher, often giving 10 lectures a week in the busy months. Finsbury College has held a unique position in education. The College did not prepare students for any degree or examination, but at the end of the courses gave certificates of proficiency, and these soon became a passport for good posts in the engineering world. "The laboratory, the workshop, and the drawing office take up the main portion of the student's time; for every hour in which the student is being talked to in the lecture room there are two hours in which he is instructing himself by actual work." These are his own words.

But I ought to have referred earlier to his first great electrical educational work—his "Elementary Lessons on Electricity and Magnetism." This he wrote in 1881, the year of the great Electrical Exhibition in Paris, the year when the electric light burst upon an astonished Europe. The book was an instantaneous success. In 1882 a second edition was called for, within two months his publishers warned him to prepare for a reprint, and in 12 years 16 reprints were necessary. This book was the cause of my first personal touch with Thompson. I had read in it an account of the effect of an iron case in shielding the space inside from magnetic effects. I was then 19 and was making many electrical and magnetic experiments. I suspended a needle and mirror inside a mustard tin with a narrow slot through which the light beam passed. I found very little shielding effect, so I wrote to Professor Thompson, at the same time pointing out three errata in the elementary lessons. The reply is from the Isle of Arran, and is as follows:

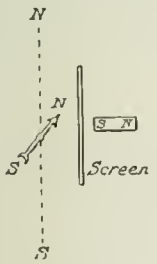
North Bank, Whiting Bay,
Isle of Arran, N.B.
7th August, 1882.

Dear Sir,

In my "lessons" I have certainly said that a sheet (not a "thin sheet" as you say) of iron will act as a magnetic screen, and it is quite true, though the thickness has a good deal to do with the perfectness of the action.

Sir W. Thomson used hollow iron bomb-shells to screen his marine galvanometers from external action on board cable-laying ships. Quarter-inch sheet iron I find to answer very well.

Common sheet tin acts as an *imperfect* screen but sufficiently perfect to get some curious results. For example, set a compass needle in the meridian. Then place a big bar magnet at one side so as to attract the needle into an oblique position. Then interpose a sheet-iron screen. You will see the deflection decrease (with two magnets and two screens, one on each side, the effect is more marked); and if you arrange things carefully you can get your needle to *rotate* by pulling in the screen at the right moment and pulling it out at the right moment. This I have often done with sheet tin.



For your errata pray accept my thanks. Two of the three were already marked for correction in the forthcoming new edition. You will, I believe, always find the authors of scientific books *most grateful* for information as to errata which, whatever pains one may take, *will* creep in.

I am, my dear Sir,
Yours faithfully,
Silvs. P. Thompson.

Ll. B. Atkinson, Esq.

That letter was a lesson which I have endeavoured to absorb and work to all my life. I was young and absolutely unknown to him, and yet on his holiday he took the trouble to spend his time in replying fully and helpfully. I have learned since that this was only one of countless similar cases.

In 1883 he delivered a course of Cantor lectures at the Society of Arts on "Dynamo-Electric Machinery." They created the greatest enthusiasm. I attended one of them and of course when published read the rest. It was the interest and demand for republication that was the starting-point of Thompson's "Dynamo-Electric Machinery." It has been written: "If Professor Thompson had done nothing else, this invaluable work would serve as his enduring monument." The book contained much original work, but its great success was due to his way of discussing with every inventor, designer and constructor his designs and ideas, and then by his great analytical and synthetic power rebuilding the whole structure, fitting each of these independent elements into its place as a whole. This work revolutionized dynamo design; it shortened by a decade the time taken to reach a definite point, and spread throughout the whole world in a simple way a knowledge of a subject until then wrapped in much obscurity to all except a few minds.

In the Silvanus P. Thompson memorial lecture in 1918, Sir Ernest Rutherford said:

"I gained my first knowledge of electricity from 'Elementary Lessons,' that remarkable and perennial book which has served to interest and instruct scientific youth and even middle age in all parts of the world.

"This work is marked by that clearness, simplicity and charm which is so characteristic of all his writings and lectures. If I was suckled, so to speak, on 'Elementary Lessons,' I cut my first teeth on 'Dynamo-Electric Machinery,' and I can well recall the strong impression left on me by the exceedingly clear, simple and logical statement on the essentials of a complex subject."

Of his other published works and notably of his Cantor lectures, and later his book on the electromagnet, I can only just speak. His lectures all over the world, his some 20 printed works, his personal teaching of the thousands of students who passed through his college and laboratory courses, always bringing not only his own knowledge, but the work of others, in a style, a lucidity, and a clarity that made the most difficult subject easy, these are what I stress as entitling him among our electrical men to the name of the great teacher, and alone and beyond that the great lesson which is mentioned on the memorial card issued by his family after he was gone, reminding all his past friends

and students of "That best portion of a good man's life, his little nameless and unnumbered acts of kindness and of love."

And now I shall go back a little to the four great men whose work I have briefly sketched. What had they in common; in what did they differ?

Faraday the discoverer came of Yorkshire stock, and Thompson of Westmorland and Yorkshire stock. Kelvin and Maxwell were of Scottish stock. It seems as if south of the Tweed these philosophers are not born. Is the reason climatic? Is it something in the harder life of these northern parts, or the longer winter evenings which tend to books and studies, or is it that the races are different, the northern race having more insight into things unseen?

Again, we have seen that Faraday did not develop any special scientific bent of mind until his early youth, but in the case of Kelvin, of Maxwell, and of Thompson there were very early signs of their great scientific and mathematical ability.

Another point on which Maxwell and Kelvin had a common feeling of certainty was the finite age of the universe.

First, we shall take Maxwell's view. In a British Association Address in 1873 he said: "Each molecule therefore throughout the universe bears impressed upon it the stamp of a metric system as distinctly as does the metre of the Archives of Paris, or the double royal cubit of the Temple of Karnal. No theory of evolution can be formed to account for the evolution of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction. None of the processes of Nature since the time when Nature began has produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecule or the identity of their properties to any of the causes which we call natural. On the other hand, the exact equality of each molecule to all others of the same kind give it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self existent. . . . But in tracing back the history of matter, science is arrested when she assures herself on the one hand that the molecule has been made, and on the other that it has not been made by any of the processes we call natural. Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created."

Now how was Kelvin guided to this point?

One of his earliest enthusiasms in study was the work of Fourier, the mathematical physicist, who had shown how, given a certain distribution of heat in a material system, to calculate the flow of heat by conduction, and the consequent heat condition at any determined subsequent time. He had proved that, whilst the theory of Fourier enabled one to predict the future distribution from some existing distribution, it is not equally true to say that one can work backwards from any given distribution to a pre-existing distribution. In fact, we usually start with an arbitrary distribution which cannot have resulted by heat flow from any preceding distribution. Reasoning on these lines he came to the conclusion that the existing order of events could be traced back, if one had the necessary data, to a time zero, but not beyond, and, accepting the permanence of the atom, he concluded these must have had a beginning. In 1906 he was still opposing the views of the new chemistry and physics on the causes of radio-activity attributed to the breaking up of atoms and the production of new elements. But we may well learn to keep an open mind in scientific matters like this, for we now know of certainty that, contrary to Maxwell's views, there is evolution of the elements and of the atoms, and presently we may learn of a process of continuous creation.

Of the relation of these men to our Institution I may say a last word. Faraday died before our Institution was founded. Maxwell died while it was still the Society of Telegraph Engineers. Kelvin was President in 1874, and again in 1889, the year after its reconstruction as the Institution of Electrical Engineers, and for the third time in 1907, the year of his death, although he gave no Address nor took any part in the work of the session. Thompson was President in 1899; his Address was devoted to the subject of the future work and development of the Institution and, writing in later years, Dr. Alexander Russell said "much of the advice given in it has been adopted by our Institution."

Now I reach the end of this brief address, in which I have endeavoured in some way to co-ordinate those parts of the work of these great lights which relate to electrical work. If I have succeeded in any way in bringing to your minds what we owe to the work of these four men, perhaps to the point that you yourselves may pursue it further, I shall have achieved what I desired.

DISCUSSION ON "LONG-DISTANCE WIRELESS TRANSMISSION."

(*Before the* WIRELESS SECTION OF THE INSTITUTION, 25 *May*, 1921.)

Introductory Notes, by C. F. ELWELL, Member.

In planning to provide a radio-telegraphic service over a distance of the order of several thousands of miles, the wireless engineer must first decide upon the value of the energy which will be required in the receiving aerial, or coil, in order to operate satisfactorily the receiving devices which he proposes to use. The value selected must be obtainable under the worst circumstances; that is, it must represent the minimum value for the year. At night and during the winter the selected value will be exceeded. For example, measurements made throughout a whole year between Arlington and Darien * stations showed a minimum audibility of 200, whereas signals of audibility 50 000 were sometimes obtained.

There is a considerable difference of opinion as to the value necessary to ensure good, reliable communication through all but the most violent atmospheric disturbances. Dr. L. W. Austin considers that a minimum of 6×10^{-10} watt is a necessity to ensure good all-the-year communication in the region of Washington. The Nauen station produces signals of this strength in American receiving aerials, whilst Bordeaux with its 1 000-kW arc transmitter and 250-metre towers produces 20×10^{-10} watt. Professor Howe † has worked out a figure which corresponds to 400×10^{-10} watt as a suitable figure for good receiving conditions. That these figures represent a fair factor of safety in comparison with readable and measurable signals may be gathered from the fact that Professor Vallauri,‡ in his measurements made in 1919, at Leghorn, Italy, of the field strength of signals from Annapolis, measured energies of from $0{\cdot}1 \times 10^{-10}$ watt to 1×10^{-10} watt.

In any case, having decided upon the energy required at the receiver, there is lacking a transmission formula which will enable the engineer to specify with any degree of certainty the amount of power required at the transmitting station several thousand miles away.

For transmission to short distances of the order of 25 to 50 miles a formula is available, based upon mathematical analysis, which would enable him to estimate very closely the transmitting power required, or conversely, the received current for any aerial or coil. Although the formula is known, it is stated here for the purpose of clearness:

$$h_s I_s = \frac{\lambda d R I_r}{377 h_r} \quad . \quad . \quad (1) \qquad \text{where}$$

I_s = current in transmitting aerial in amperes.
I_r = current in receiving aerial in amperes.
h_s = effective height of transmitting aerial in kilometres.
h_r = effective height of receiving aerial in kilometres.
λ = wave-length in kilometres.
d = distance in kilometres.
R = total resistance of the receiving circuit in ohms.

* L. W. AUSTIN: *Proceedings of the Institute of Radio Engineers*, 1916, vol. 4, p. 257.
† G. W. O. HOWE: *Radio Review*, 1920, vol. 1, p. 599.
‡ *Proceedings of the Institute of Radio Engineers*, 1920, vol. 8 p. 286.

For the purpose of evaluating the effective height, or the aerial current, of either the transmitter or the receiver, this formula is sufficiently accurate provided that the distance be as short as stated. At greater distances than 50 miles the magnetic field would be less than that given by this formula, on account of the absorption of the energy of the wave as it travels along. To allow for this decrease, a factor must be added to the right-hand term. The best known of the several factors which have been put forward is as follows:

$$\sqrt{\left(\frac{\sin\theta}{\theta}\right)}\epsilon^{0{\cdot}0015d/\sqrt{\lambda}}$$

When formula (1) is multiplied by this factor it becomes

$$h_s I_s = \frac{\lambda d R I_r}{377 h_r}\sqrt{\left(\frac{\sin\theta}{\theta}\right)}\epsilon^{0{\cdot}0015d/\sqrt{\lambda}}$$

and is known as the Austin–Cohen transmission formula.* The factor $\sqrt{(\sin\theta/\theta)}$ takes care of the curvature of the earth, in that θ is the angle subtended at the earth's centre by the sector representing the distance between the two stations. For short distances this factor reduces to 1. The factor $0{\cdot}0015$ is sometimes represented by the letter alpha and it will now be referred to under this name. The value of α employed in the Austin–Cohen formula was obtained empirically using damped-wave transmitters. Several other values for α have been proposed and Dr. Fuller † has made measurements between two arc stations, i.e. using continuous waves, from which he determined a value of α of $0{\cdot}0045$, but he at the same time changed the term $\sqrt{\lambda}$ to $\lambda^{1{\cdot}4}$. Other values for α have been proposed from time to time, and although they give results even more unreliable than either the Austin–Cohen or the Fuller formula, some of them are mentioned for reference purposes.

In the Sommerfeld purely theoretical formula the exponential factor is

$$\epsilon^{0{\cdot}0019d/\sqrt[3]{\lambda}}$$

Dr. Eccles suggested for night transmission ‡ a value of α of $0{\cdot}0002$.

Experiments made by Mr. Hogan between Arlington and the U.S.S. "Salem" seem to fit a value of α of $0{\cdot}000247$.‡

When we say more unreliable, it is because neither the Austin–Cohen nor the Fuller formula can be relied upon in serious design work, and recent measurements will be quoted to support this statement.

Professor Howe in a recent article upon the power required for long-distance transmission § calculated a

* L. W. AUSTIN: *Bulletin of the Bureau of Standards*, 1914, vol. 11, p. 69.
† L. F. FULLER: *Transactions of the American Institute of Electrical Engineers*, 1915, vol. 34, part 1, p. 809.
‡ W. H. ECCLES: "Wireless Telegraphy and Telephony," p. 154.
§ G. W. O. HOWE: *Radio Review*, 1920, vol. 1, p. 598.

number of tables purporting to give the power required at the transmitter for various distances up to 6 000 miles in order to give the minimum strength of signal which he estimates is necessary, and which we have already mentioned. The Austin-Cohen formula was used and the number of kilowatts given by the formula was in some cases quite unreasonable. In a subsequent article * he re-made these calculations employing the Fuller formula, and the results are at least much more reasonable, although it would be a brave wireless engineer who would seriously propose to employ some of the figures suggested by the calculations. As an additional check on the formulæ Professor Vallauri, after his measurements of the field strength of Annapolis as received at Leghorn,† attempted to calculate the value of $h_s I_s$ for Annapolis and obtained, with the Austin-Cohen formula, values of from 170 to 530, and with the Fuller formula values of from 24 to 75. When the real value of $h_s I_s$ for Annapolis is known to be $0{\cdot}107 \times 280$ or about 30, it will be seen that the Austin–Cohen formula does not give as good results as the Fuller formula, over this distance, viz. over 4 000 miles. This may be due to the fact that the Austin–Cohen formula was obtained with damped waves and the Fuller formula with continuous waves. The large amounts of power called for when the Austin–Cohen formula is employed might lend some colour to this theory. In any case, from consideration of the methods employed to obtain the Fuller formula, which involved the use of so unsatisfactory a detector as the rotary ticker, it is clear that great faith cannot be placed in it.

The measurements recently made on board the "Aldebaran,"‡ sent out by the French Minister of the Marine to measure the strength of signals emitted by Nantes and Lyons all the way out to the Antipodes, also confirm the general opinion that the Austin–Cohen formula is not reliable for distances greater that 2 200 miles.

As it is fairly apparent that there is no transmission formula upon which a design engineer can rely, the time seems to be ripe for co-ordinated international measurements of the strength of signals emitted by both high- and low-power continuous-wave transmitting stations, with a view to a new determination of α. In this connection I would suggest that a single term will probably not be found which will cover such variables as we know transmission over land and sea to be and, for example, the Pacific as opposed to the Atlantic ocean. It is possible that for $\alpha d/\sqrt{\lambda}$ we shall have to substitute a factor of the form of $(\alpha d+\beta d')/\lambda^n$, where d represents distance over sea, and d' distance over land. Only a large quantity of data carefully taken in all parts of the world will permit of a proper determination of α or its equivalent.

Fortunately the apparatus necessary for the measurement of I_r, the received current, is not of an expensive nature. All amateurs possessing modern continuous-wave receiving apparatus can, with a few additions to their apparatus and a study of the excellent articles by Professor Vallauri * and by Commander Pession, commence work on this highly interesting subject. High-power continuous-wave stations are numerous and they can commence on those close at hand, leaving the measurement of the smaller I_r to be obtained from American stations until after they have perfected their methods of measurement.

The Radio Research Board might be induced to co-ordinate all these data, as the measurements will not be of much value if not accurately dated and timed, and the logs of transmitting stations will also need to be confided to some central authority, such as the Radio Research Board, or a sub-committee of this Wireless Section.

A standard method of carrying out the measurements might be developed and printed forms even distributed to those desirous of obtaining data. One of the first steps will be for the owners of all stations to measure the effective height of their aerials. The triangular method of Commander Pession is very simple. In order to avoid the use of the expensive thermo-galvanometer, a method of estimating the strength of the received signals based upon audibilities might be worked up. It would be inexpensive and measurements of many sets of triangles could be made in order to cross-check the results. It is quite possible that the measurement of the value of $h_s I_s$ for any given transmitting stations will become as common in the future as the measurement of its wave-length is to-day, and this ability to control the $h_s I_s$ of any given station may furnish means of control of the abuse of power, i.e. employment of much more than the necessary amount to cover a certain distance at any period. The crowding of the ether will mean legislation against abuse of power, as well as limitations as to the wave-lengths to be used.

It is quite possible, after all, that no formula can be evolved either mathematically or empirically, because there is no reason why the distribution of the field due to a given transmission should be symmetrical, that is, follow a fixed transmission law, when the earth itself is far from being homogeneous. There are three factors tending towards dissymmetry—land, sea, and the Heaviside layer.

A formula that is satisfactory for calculation of the current received in the United States might probably not be correct for transmission in the other direction; therefore it is essential that measurements between any two points be made in both directions. In this connection Mr. Hogan's measurements † of the signals from Darien at Honolulu throw a little light.

If no universal transmission formula can be evolved it may be necessary to trace polar diagrams around certain wireless centres, or certain countries, not only by means of measurements taken close up to the transmitting station, as is customary when such observations are made at the present time, but also at greater distances from the station. A series of polar curves could be plotted out for each large transmitting station, representing the intensity of radiation in various directions at different distances from the radiating aerial. It would seem by no means certain that such curves

* G. W. O. Howe: *Radio Review*, 1920, vol. 1, p. 652.
† *Radio Review*, 1921, vol. 2, p. 77, p. 138, and p. 179.
‡ *Bulletin de la Société Française des Électriciens*, 1920, vol. 10, p. 247.

* *Radio Review*, 1921, vol. 2, p. 77, p. 138, and p. 179.
† L. F. Fuller: *Proceedings of the Institute of Radio Engineers*, 1916, vol. 4, p. 262.

would retain the same shape, as the distance from the aerial is increased.

The following debatable aspects of the question are suggested:

(1) Mathematical treatment.
(2) Alternative methods of measurement of received field strength.
(3) Methods of determination of the field strength necessary to ensure a reliable service.
(4) Ways and means of obtaining and co-ordinating data from actual measurements.

Admiral Sir Henry Jackson: Mr. Elwell suggests in his paper that the Radio Research Board might well undertake the work of co-ordination of results of radiation measured simultaneously at various stations, and, speaking as Chairman of that Board, I welcome this discussion as we are much interested in this particular subject, and are actually organizing such measurements as he suggests, considering they must be of great value in the design of our long-distance stations. Thus we are ready to help in this matter, but, before undertaking to collate and analyse results, it would be necessary to see that the recording arrangements were standardized and that the results obtained by them would be acceptable to those responsible for the design of the stations. Perhaps we are, at the beginning, aiming at too great a degree of accuracy, but, if so, it is a fault on the right side. It must not be forgotten that although the human ear is very sensitive, it is not acceptable as a recording instrument of precision, even in one single individual, and when many observers are engaged at different stations to measure the energy by the intensity of the sound in the telephone, large sources of personal error must creep into the results and render the final one open to doubt. This, of course, will be much reduced if equality of signal strength between two sources simultaneously emitting radiation is the measurement to be taken, and not the comparative strengths of one signal at different times. Thus, if a local source of radiation be used as a standard, and if a number of these can be calibrated and also depended upon to remain constant, the problem is much simplified, and probably very useful and reliable results would be obtained. The expense is greater than that indicated by Mr. Elwell, but this would be justifiable as producing a result free from personal errors due to different observers, or at least greatly reducing them. This is the system we hope to begin with very shortly. We have been delayed from the want of an observer, as we must first prepare and test such standards. The Austin–Cohen formula is now being criticized, but I think that during the past few years radio engineers have been only too glad to use it as a groundwork for their calculations. There is, however, one factor in it which appears to me to carry more weight than it is entitled to when applied to very long distances, and that is the effective height of the transmitting aerial. It is now generally accepted that the waves radiated from the aerial network at the transmitting station are reflected by the Heaviside layer in the upper atmosphere before they give up their energy to the receiving aerial. If this reflecting layer is not perfectly concave, there must be more or less distortion or refraction of the waves, and thus it is difficult to accept the assumption that the reflected image of the transmitting aerial will retain its exact proportions a few thousand miles away. The phenomenon appears to me to resemble that of reflection in a mirror. If the mirror is perfect the reflected image will retain its proper proportions; if the mirror is rough the image may be reduced to a blur, not even consistent in outline at different times and places, and thus optical calculations of its proportions may be rendered impossible. Possibly the author has something of this sort in his mind when he mentions that the Heaviside layer is one of the causes of dissymmetry. It is one of the reasons which make me think that much useful information (even if it be only in a negative sense) can be obtained from a lengthy series of systematic observations such as the author proposes, and it falls under (1) of his debatable points. Possibly the mathematicians await the results of the practical measurements which fall under items (2), (3) and (4). Item (3) appears to be a question for the practical radio engineer and for operators. Items (2) and (4) are matters in which the Radio Research Board will, I feel sure, assist by all the means in their power.

Mr. A. Gray: Some 18 or 19 years ago the Marconi Company adopted an empirical formula,* which was used for many years, based on the cube root of the power and the effective height. Dr. Eccles recently † gave an explanation why the cube root of the power was permissible, namely, because the wave-length took care of itself owing to the aerial increasing with the range of the station. Mr. Elwell refers to the continuous day and night range. There is, however, another useful range, i.e. the occasional range, a range of so many hours a day. In many of the ultra-long-distance stations projected at the present time that is the kind of range to be expected with existing means. The line along which improvement can be looked for is in so improving the receiving apparatus that, with the existing powers which may be economically used, better results can be obtained. My colleagues in the Marconi Company have been doing much work in this direction and may be able to give some information about it when the time comes.

Mr. H. J. Round: Mr. Elwell states in the first few lines of his paper that at night and during the winter the selected value of signal will be exceeded. Our general experience in the past few years has been that not only is the night-time becoming more troublesome than the daytime in long-distance transmission, but winter is also becoming more troublesome in the daytime. This, of course, is due not to actual night conditions getting worse, but to the fact that we have, to some extent, overcome the daylight difficulties and not the night difficulties. Some recent measurements made by Mr. Eckersley on the transatlantic signals

* The formula was $P = K(D/H)^3$
where P = power to wireless apparatus in kW,
K = a constant varied according to the nature of the intervening surface,
H = effective height of aerial, i.e. height of centre of capacity in feet,
D = range or distance to receiving station (assumed to employ the same size of aerial) in nautical miles.

† *Electrician*, 1920, vol. 84, p. 207.

indicate that at present, during this part of the year, the received American signals at night as measured near London are, for several hours on a large number of nights, only one-fifth or one-sixth as strong as the average signal in the daytime, and usually the strength of the atmospheric is considerably greater at night. In winter, at our small transatlantic stations we usually have considerably more trouble in daytime reception during that period than in the summer time, owing to the extreme weakness and variability of signals. The best time of the year seems to be the half-way period between summer and winter. A rough explanation of this curious condition may be as follows: Marconi pointed out that a weak-signal period occurred when the sunset or sunrise line was between the two stations, and this is an almost invariable rule. In the summer these two weak periods would be near together about midnight and in the winter about noon, and consequently in the summer true night would not last long all the way across, whereas daylight would not last long in the winter. If true day and night conditions could be assumed to mean good signal conditions, the result would be that in summer we should get good day and bad night conditions, and in the winter good night and bad day conditions. If this idea is followed out, it is easily seen that the further stations are from one another East and West, the more erratic will be general working conditions; but if the stations are North and South, and the same distance apart as before, the conditions will be more amenable to calculation. I should like to mention another point made on the first page, with reference to the measurement in watts of signals. The size of the receiving aerial and other conditions would come in, in connection with such measurements, and perhaps it would be as well if we could arrive at some better way of stating values. I suggest that some term like "microvolts per metre," that is, a metre of vertical aerial or a metre of ether, should be used as a measurement of strength received. It would be a much more definite measurement and, incidentally, the value of strength of atmospherics can, and should be, measured and indicated in the same way. With regard to atmospherics, the number of these arriving at a definite voltage in a certain time could be stated as an additional measure of the atmospherics. Mr. Elwell appears to indicate that there will be a great difference between damped and continuous-wave transmission, but I think that when we actually come to measurements we shall find this difference to be only small. I do not mean to say by this that the continuous wave is not much more readable; it is simply a question of the measurement of the actual strength. Referring to the organization of measurement of signals and Mr. Elwell's suggestion that amateurs might be employed for that purpose, mentioned on page 678, I am afraid that an enormous mass of unreliable data would thereby be obtained. After watching the work of Mr. Eckersley and Mr. Lunnon during the last few months in connection with measurement, I have come to the conclusion that it is a much more difficult job than appears on the surface to make certain that results are obtained with, say, a 10 per cent or 20 per cent accuracy. If amateurs are employed I would suggest that the Wireless Section of the National Physical Laboratory should give some data on the value of the effective height of such aerials as amateurs would use. These effective heights are not liable to vary a great deal, but the difficulty to an amateur of making these measurements is very great, especially if he attempts to use any method such as an audibility method. The three general methods that we have used in the measurement of signals consist: (1) in using an ammeter of a more or less sensitive nature; (2) a method which we have called the slide-back method, in which we use a valve for ascertaining the maximum voltage of the signal (incidentally the Americans have recently used a similar method); and (3) the comparison method. The ammeter is used for very short distances; the slide-back method is used for moderate distances, say 100 miles; the comparison method is used for extremely weak signals. Ordinary audibility methods are, I am sure, very dangerous for accurate measurement. We propose in future to make a number of accurate measurements at different places and on mobile stations. I hope we shall be able to continue this work throughout the next two years and publish the results as soon as they are obtained.

Professor C. L. Fortescue: The variety of different formulæ which have been proposed at different times suggests the necessity of critically examining them in order to see which of them appears to be preferable. The starting-point in all cases is formula (1) of the paper, which is preferably written

$$X_d = I_r R/h_r = 377 I_s h_s/(\lambda d)$$

where X_d is the R.M.S. electric field strength at distance d from the transmitter, in volts per cm. This formula applies to the case of a half-Hertzian oscillator projecting from a perfectly conducting plane surface, and each term has a definite physical meaning. The $I_s h_s$ term gives the electric moment of the half oscillator; the $1/\lambda$ term is to be expected when it is remembered that the electric field is proportional to the rate of change of the magnetic lines, and therefore to the number of waves of a given amplitude passing per second; and the $1/d$ term indicates that the flow of energy is hemispherical. This formula is purely theoretical, but for short distances it has been proved by Duddell and Taylor, and by Austin, to hold experimentally. For most practical purposes, however, corrections have to be made. The actual aerial is not truly a half-Hertzian oscillator, and a correction must be made for the equivalent height. This can be calculated or determined experimentally, and errors exceeding 30 per cent are unlikely. Then, again, allowance must be made for the imperfect conductivity of the surface of the earth. Calculations show that there is some energy absorption, but that it is small. Far more important for long distances is the curvature of the surface of the earth. The problem is difficult, but there is now a fair consensus of opinion that if the earth were surrounded by a homogeneous dielectric having substantially the same dielectric properties as the air on the surface, then the range of our large transmitting stations would be relatively small. A great mass of evidence is now available supporting the theory of a conducting

"Heaviside layer" which acts practically as an outer spherical mirror at which the electromagnetic waves are reflected with some scattering and other wastage of energy. Assuming that the energy flow is thus confined to a belt and neglecting the losses in the first instance, a factor $\sqrt{(2\pi d^2/2\pi aH \sin\theta)}$ must be applied to the formula for X_d; a being the radius of the earth and H the effective height of the Heaviside layer. This gives

$$X_d = 377 I_s h_s \left(\frac{1}{\lambda}\right) \frac{1}{\sqrt{(aH \sin\theta)}}$$

To allow for the losses, an exponential term of the form $\epsilon^{-\alpha d f(\lambda)}$ is the natural one. The term $f(\lambda)$ in the index has been a bone of contention amongst both experimentalists and mathematicians. Cohen found $f(\lambda) = \lambda^{-\frac{1}{2}}$ and Austin used this form. Fuller found $f(\lambda) = \lambda^{-1.4}$. The diffraction theorists proposed $f(\lambda) = \lambda^{-\frac{1}{3}}$. G. N. Watson appears to find that, even if the Heaviside layer be a perfect reflector, $f(\lambda) = \lambda^{-\frac{1}{2}}$, from which it is apparently to be concluded that the term $\lambda^{-\frac{1}{2}}$ is characteristic of reflection between concentric spherical surfaces. Assuming this to be correct, the X_d formula becomes

$$X_d = 377 I_s h_s \left(\frac{1}{\lambda}\right) \frac{1}{\sqrt{(aH \sin\theta)}} \epsilon^{-\alpha d \lambda^{-\frac{1}{2}}}$$
$$= A I_s h_s \left(\frac{1}{\lambda}\right) \frac{1}{\sqrt{(a \sin\theta)}} \epsilon^{-\alpha d \lambda^{-\frac{1}{2}}}$$

The introduction of the $1/\sqrt{(a \sin\theta)}$ term instead of the $1/d$ of the Austin formula certainly gives a correction in the right direction in the case of the "Aldebaran" results. Possibly the formulæ are equally useful for practical purposes if suitable values of A and α are taken. For any given line of communication these values can only be found by actual trial, and the true function of the formulæ is to enable suitable values of $I_s h_s$ to be calculated for any desired conditions when two or more actual observations are available.

Mr. L. B. Turner: The opening sentence in Mr. Elwell's paper runs: "In planning to provide a radio-telegraphic service . . . the wireless engineer must first decide upon the value of the energy which will be required in the receiving aerial, or coil, in order to operate satisfactorily the receiving devices which he proposes to use." A few years ago, before the modern amplifier had come into being, that statement might have been accepted as a truism. It is not so now; in my opinion it is not even true; at any rate it is very incomplete. I should like to introduce what I have to say by an inversion of that statement, which I would modify as follows: "In planning to provide a radio-telegraphic service . . . the wireless engineer is *not* directly concerned with the power picked up by the receiving aerial; his concern is rather to arrange for a sufficiently large ratio between the power picked up from the sending station and the power picked up in the form of disturbing signals, particularly atmospherics." With our present-day means of amplifying we can increase practically without limit the power of any signals received, so that we can render any signal, however weak, competent to operate the receiving apparatus. It is almost a matter of indifference to us now whether we use a large high antenna at the receiver or a small antenna. As the size of the antenna, and therefore the power picked up from both desired and undesired signals, are decreased, the old conditions are more or less perfectly restored merely by adding another triode or so to the amplifier. The trouble is that the disturbing signals also are effective. Of vital importance is the *ratio* between the power received from the signal and the power received from atmospherics, and it is to make this ratio large that powerful transmitters have been and will be demanded. I wish to say something about the choice of wave-lengths from that point of view. Our knowledge of even the arrival forms of atmospherics is of the scantiest, but it is a common experience that the longer the wave-length of a receiver the more violent are the disturbances received from atmospherics; so that the consideration of atmospherics must lead to the selection of an optimum wave-length shorter than the optimum in the absence of atmospherics. Such a course may tend towards profound alterations in the choice of wave-length. I have recently made some calculations along these lines, and I give here a table showing in a particular case, and on certain assumptions, how (*a*) the maximum signal power receivable, and (*b*) the atmospheric power at the receiver, vary as the wave-length of the transmitter is changed. The Austin–Cohen exponential factor is assumed; and the atmospherics are taken to be rapidly damped pulses of the form $F\epsilon^{-rt}$, r^2 being much greater than the corresponding damping coefficient of the receiving circuit, but much less than $(2\pi n)^2$, where n is the signal frequency. The wave-length of the transmitter is supposed to be varied without alteration of antenna height or current. The units of columns (*a*) and (*b*) are arbitrarily chosen to make the values in those columns unity at the wave-length (2·2 km) giving the maximum ratio $(a)/(b)$.

RANGE: 5 000 km = 3 100 miles.

Wave-length	(*a*) Signal Power	(*b*) Atmospheric Power	Ratio: $(a)/(b)$
km			
1	0·014	0·035	0·39
2	0·63	0·63	0·99
2·2	1·00	1·00	1·00 (max.)
3	3·7	4·3	0·85
4	12	18	0·67
6	35	88	0·40
8	103	501	0·20
10	178	1 600	0·11

From those figures it will be seen that, as the wave-length increases, the received power from the signal goes up. On the other hand, the received power from the atmospheric goes up more rapidly, and the ratio between the two reaches a maximum value at a certain wave-length, 2·2 km. According to the ratio criterion for choice of wave-length, therefore, the best wave-length would be 2·2 km. It is true that vastly less signal power is obtained at the receiver with this wave-length than with, say, 10 km, i.e. 178 times less—but

I suggest that with a modern amplifier this is a matter of unconcern. We can multiply it by 178, and instead of having 1 600 of atmospheric we then have only 178. The whole question is, of course, not so simple as this. The assumptions on which I have calculated these figures have to be supported, and various other factors on which a choice of wave-length depends must be brought into account; but I think this factor is very important, and its significance is at least illustrated by the table.

Professor E. W. Marchant: Mr. Turner has referred to the minimum measurable signal strength mentioned on the first page of Mr. Elwell's paper. That figure, as he says, is of very little importance nowadays because we are able to increase the sensitiveness of receivers to such an enormous degree. The actual figure quoted of 6 times 10^{-10} watt is, I think, a high one; as Mr. Elwell says, later on in his Note, Prof. Vallauri has got down to one-tenth of 10^{-10} watt. In measurements we made some time ago we found it easy to measure signals of a strength of 6 times 10^{-10} watt by using crystal detectors, and of course with valves it is possible to go to much smaller figures. With regard to the remark made about the station at Bordeaux having a 1 000-kW arc transmitter, I hope that in all this work care will be taken to state exactly what the aerial current is, in any particular case, because that is the controlling factor, and I do not think the power which is put into the aerial at Bordeaux is anything approaching 1 000 kW. I should like to say a word or two on the method of measurement that might be adopted for tests of signal strength. Mr. Elwell has referred to the method of carrying out measurements by means of a shunted telephone. That is a method which has been tried experimentally by Austin and many others, but I think most people who have had experience with it are of opinion that it is not very accurate. It gives only a very rough and ready test. I have tried the method myself, but I have never been able to get any results that I could regard as really satisfactory. I hope, if a Committee is appointed to discuss the best method of getting these results, that this question will be discussed, because I think it is a very important subject indeed. If we are to have measurements of this kind made it is very necessary that some method which can be thoroughly depended upon should be used. In the tests which I made I used an Einthoven galvanometer, and calibrated the measuring circuit by a buzzer. It is, however, difficult to manipulate this galvanometer satisfactorily. I have tried to use it in connection with valves, but, so far, the results have not been very successful, although there was plenty of power to work the instrument. I should like to refer to the general question which Mr. Elwell has raised as to the desirability of making a scientific and systematic series of observations on long-distance transmissions. I should like to call attention to the fact that, in 1914, a small Commission met in Brussels which started a discussion of this kind with a view to undertaking a research which should cover European stations. The valve has altered all that; instead of discussing transmission over distances of 500 to 600 miles, which was the range we were discussing at that time, the possible distances now have become very much greater. I hope it may be possible to obtain co-operation between experimenters in this country and those on the other side of the Atlantic, so that a really exhaustive set of observations may be made, and that we may have full details of the signals sent out from America, the antenna currents, atmospheric conditions and so forth, and obtain results which will be of real value in the elucidation of this very difficult subject. The investigations now being organized by the Radio Research Board should be of very great value for this purpose.

Captain R. C. Trench: For the engineer the first essential to the solution of the problem is to see it broadly in its simplest form. Taking formula (1) of the paper

$$h_s I_s = \frac{\lambda d R I_r}{377 h_r}$$

a useful simplification can be arrived at immediately by writing it

$$\frac{h_s I_s}{\lambda} = \frac{d}{377 h_r} R I_r$$

Now $h_s I_s/\lambda$ is a measure of the power radiated, and RI_r of the power received, so that the equation can be rewritten $P_s = K(d/h_r)P_r$, where K is a constant. This is our first formula for a perfectly conducting plane surface. Mr. Turner points out that, however small P_r is, it can be rendered audible by modern reception; all that is required is that it should be strong enough to carry us through unavoidable disturbance, and he apparently deprecates allotting it a value in the search for a formula. Nevertheless, some power is wanted in the receiving aerial, and it still remains for the engineer to say how much; it is more on the longer waves on account of atmospherics, more in congested areas, less if it is not necessary to work all the 24 hours, etc. Now that unit audibility for so many types of receiver is given, it should not be too difficult to allot to P_r a working value for any set of conditions, based on so many times audibility. We next come to the question of attenuation under practical conditions, represented by an exponential factor of which the index varies with different authorities, but which takes the form $\epsilon^{\alpha d/\lambda^n}$. Our equation then becomes

$$P_s = K \frac{d}{h_r} P_r \epsilon^{\alpha d/\lambda^n}$$

The Austin–Cohen exponential factor is wonderfully satisfactory up to a range which is usually given as about 2 200 miles (it was tested by its authors in 1910 only up to 1 100 miles), and beyond this the Fuller exponential factor may be followed. Having then decided what values he will adopt for α and n, the engineer has his working formula complete. He must then note the general normal conditions, intervening surface, type of apparatus, etc., under which this formula applies, and must look for modifying factors for different conditions. These factors are of two kinds; first and simplest is a modification of K to allow for greater or less efficiency at the receiver, or more or less interference. The next and far more difficult allowance is

that for different rates of attenuation over different surfaces, entailing modifications of the exponential index. I have ventured on a suggested modification in the form $Md^{\frac{1}{2}}/\lambda$ to be added to the exponential index $\alpha d/\lambda^n$, where M varies from 0 to about 0·25, as the surface varies in difficulty from the Atlantic ocean to mountainous country. It is here particularly that research is so much needed. Mr. Elwell refers to the ease with which the amateur can measure his received current I_r, but I do not see how this is of assistance unless he can also measure the value of R for each reading of I_r. In a given field strength the value of I_r depends on R, which may, as Mr. Turner points out, be reduced almost indefinitely by retro-active amplification, so that the mere measuring of I_r, when a small adjustment of the receiving circuit will throw out the whole receiving resistance, does not appear to carry us much farther.

Mr. T. L. Eckersley: Mr. Round has mentioned the fact that we have made a large number of measurements on various stations, and I should like to show, in Figs. A, B and C, the actual results we have obtained in three 24-hour tests on the transatlantic stations New Brunswick, Marion and Tuckerton respectively. These results were obtained with apparatus which will be described in more detail by my colleague, Mr. Lunnon. Every precaution was taken that the results should be accurate, and we can probably rely on the accuracy to about 5 per cent. The tests shown here were taken throughout a period of 24 hours, and the ordinates of the curves represent the actual voltages induced in the aerial at Chelmsford by the signals sent out by the stations New Brunswick, Marion and Tuckerton. These results form some of the first really accurate data for the comparison of the existing formulæ with actual results. The Austin–Cohen formula has the sanction of use, and has been tested by Austin over a wide range of wave-lengths and distances, but the shunted-telephone method of measurement which he used may be liable to considerable errors. It is therefore of great importance to have some absolute measurement which can be relied on within known limits of experimental error. The results of the comparison with Austin's formula are shown in the figures. Take the case of New Brunswick (Fig. A). The mean daylight value of the electromotive force induced in the aerial is 750 microvolts. The effective height of the receiving aerial is 16 metres. The effective height of the transmitting aerial is assumed to be 0·6 of the maximum height, i.e. 240 ft., or about 75 m. The distance and wave-length are both known, and the current in the transmitting aerial was 550 amperes. Inserting these values in the formula

$$V = \frac{377 h_1 h_2}{\lambda d} \sqrt{\left(\frac{\theta}{\sin\theta}\right)} \epsilon^{-0{\cdot}0015 d/\sqrt{\lambda}}$$

we arrive at the result $V = 397$, so that the ratio 750/397 is 1·89. The received signal is therefore greater than that given by Austin's formula, and in this respect agrees with some results of Prof. Vallauri. The ratio of observed to calculated results is practically the same in the case of Marion (Fig. B). The actual mean value of the electromotive force received at Broomfield was 517 microvolts. The calculated value was 284 microvolts. The ratio is therefore 1·82. The fact that the ratio is the same for both these stations suggests that the exponential part of Austin's formula is correct, but that the remaining factor gives values of received electromotive force which are rather too low. Taking the results in respect of New Brunswick and Marion, and actually eliminating the first factor of the formula, we can arrive at the coefficient of the exponential factor; this is 0·002, which agrees closely with the Austin–Cohen coefficient 0·0015. Comparison may be made with other formulæ, i.e. those calculated theoretically by G. N. Watson.* In the former paper the author gives a formula for the electromotive force induced on the pure diffraction theory. This formula is

$$V_{\text{rec.}} = 0{\cdot}5368 \frac{1}{\sqrt{(\sin\theta)}} \cdot \frac{h_1 h_2}{\lambda^{7/6}} \epsilon^{-23{\cdot}9/\lambda^{1/3}}$$

where θ is the zenith angle between the two stations. It gives values which are only 1 per cent of the observed values. Pure diffraction, as Prof. Fortescue pointed out, is entirely insufficient to account for the facts. It is therefore necessary to consider the effect due to some reflecting or refracting layer which confines the energy to the surface of the earth. In his second paper G. N. Watson takes account of the reflecting layer (i.e. the Heaviside layer) and shows that an exponential factor of the Austin–Cohen form results, and in this respect the actual results agree with the theory. As an illustration of the reflection theory considered by G. N. Watson, take the simple case of a transmitter confined between two perfectly conducting planes (Fig. D). The transmitter is situated on the lower conducting surface, and the upper conducting surface is supposed to be at a distance of about 80 to 100 km above the lower. It is not difficult to see that the effect of reflection at the upper layer is equivalent to the effect of an image of the transmitter placed at a height $2h$ above the original, and that this again on reflection generates another image at a distance $2h$ below, and so on, and that finally the effect in the space between the two surfaces is the same as that due to the series of images arranged at equal distances $2h$ along the axis OX. At distances comparable with h the main contribution is from the transmitter O, and the electric force varies nearly in accordance with the ordinary formula $377 h_1 h_2/(\lambda d)$. At distances large compared with h an ever-increasing number of the images contributes to the value of E, and this quantity approaches more and more nearly the effect due to samples picked out from a continuous distribution of images along the axis OX, which is known to give a value of E varying inversely as the square root of r, and not inversely as r. This, of course, is obvious from energy considerations, for since this is confined to the space between the two planes it must vary as $1/r$, and the electric force as $1/\sqrt{r}$. The case of waves transmitted in the space between two concentric spheres is obviously akin and the same main characteristics will occur. At distances short compared with h the sphere is approximately a plane and the signals will vary in proportion to $1/r$,

* *Proceedings of the Royal Society*, A, 1918–1919 vol. 95, pp. 83 and 546.

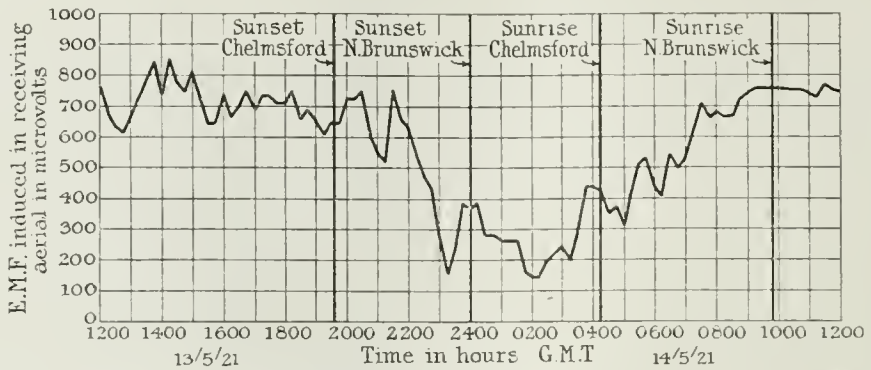


FIG. A.—Chart showing variation in strength of received signals during 24 hours from 1 200 G.M.T., 13 May, to 1 200 G.M.T., 14 May, 1921. Transmitting station, New Brunswick; receiving station, Chelmsford.

Effective height of transmitting aerial = 75 m.
Effective height of receiving aerial = 16 m.
Distance = 5 420 km; wave-length = 13 600 m.
Current in transmitting aerial = 550 amperes.
Zenith angle = 48·78°.

Austin-Cohen formula: $I_2R_2 = \frac{377h_1h_2I_1}{\lambda d}\sqrt{\left(\frac{\theta}{\sin\theta}\right)}\epsilon^{-0\cdot0015\,d/\sqrt{\lambda}}$ gives
E.M.F. induced in receiving aerial = 397 microvolts.
Mean daylight measured value = 750 microvolts.
Ratio: $\frac{\text{Measured}}{\text{Austin-Cohen}} = 1\cdot89$.

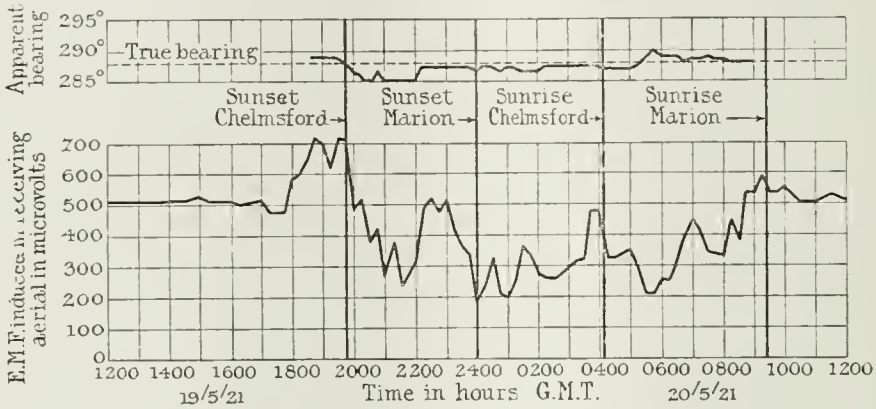


FIG. B.—Chart showing variation in strength and divergence from true bearing of received signals during 24 hours from 1 200 G.M.T., 19 May, to 1 200 G.M.T., 20 May, 1921. Transmitting station, Marion; receiving station, Chelmsford.

Effective height of transmitting aerial = 75 m.
Effective height of receiving aerial = 16 m.
Distance = 5 310 km; wave-length = 11 600 m.
Current in transmitting aerial = 380 amperes.
Zenith angle = 47·71°.

Austin-Cohen formula: $I_2R_2 = \frac{377h_1h_2I_1}{\lambda d}\sqrt{\left(\frac{\theta}{\sin\theta}\right)}\epsilon^{-0\cdot0015d/\sqrt{\lambda}}$ gives
E.M.F. induced in receiving aerial = 284 microvolts.
Mean daylight measured value = 517 microvolts.
Ratio: $\frac{\text{Measured}}{\text{Austin-Cohen}} = 1\cdot82$.

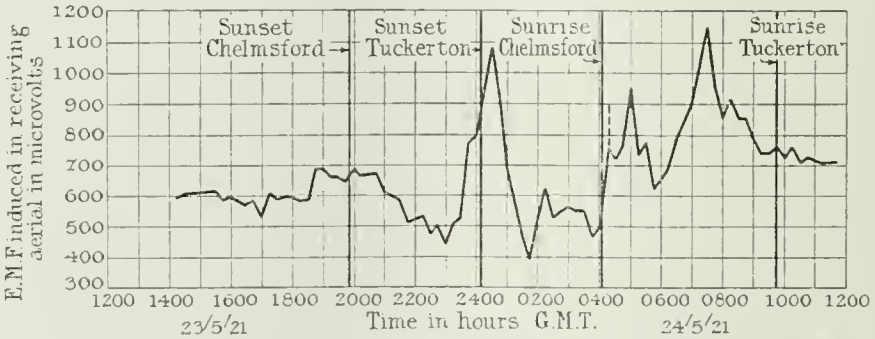


FIG. C.—Chart showing variation in strength of signals during 24 hours from 1 200 G.M.T., 23 May, to 1 200 G.M.T., 24 May, 1921. Transmitting station, Tuckerton; receiving station, Chelmsford.

Effective height of transmitting aerial = 100 m (assumed).
Effective height of receiving aerial = 16 m.
Distance = 5 520 km; wave-length = 16 000 m.
Current in transmitting aerial = 400 amperes.
Zenith angle = 49·75°.

Austin-Cohen formula: $I_2R_2 = \frac{377h_1h_2I_1}{\lambda d}\sqrt{\left(\frac{\theta}{\sin\theta}\right)}\epsilon^{-0\cdot0015d/\sqrt{\lambda}}$ gives
E.M.F. induced in receiving aerial = 365 microvolts.
Mean daylight measured value — 660 microvolts.
Ratio: $\frac{\text{Measured}}{\text{Austin-Cohen}} = 1\cdot81$.

and at greater distances in proportion to $1/(R \sin \theta)$, R being the radius of the earth and θ the zenith angle between the two stations. At distances large compared with h, an expression is suggested of the form

$$\frac{377}{\lambda} \cdot \frac{h_1 h_2}{\frac{1}{2}(R \sin \theta r')} \epsilon^{-0{\cdot}0015d/\sqrt{\lambda}}$$

where the quantity r' is introduced to correct the dimensions and is obviously some function of h and of the same order of magnitude, and represents the quantity

$$\frac{r_0 R \sin \theta}{r_0 + R \sin \theta}$$

When $R \sin \theta$ is small compared with r_0 the formula reduces to

$$\frac{377 h_1 h_2}{\lambda R \sin \theta} \epsilon^{-0{\cdot}0015d/\sqrt{\lambda}}$$

and when $R \sin \theta$ is large compared with r_0, to

$$\frac{377 h_1 h_2}{\lambda \sqrt{(R \sin \theta r_0)}} \epsilon^{-0{\cdot}0015d/\sqrt{\lambda}}$$

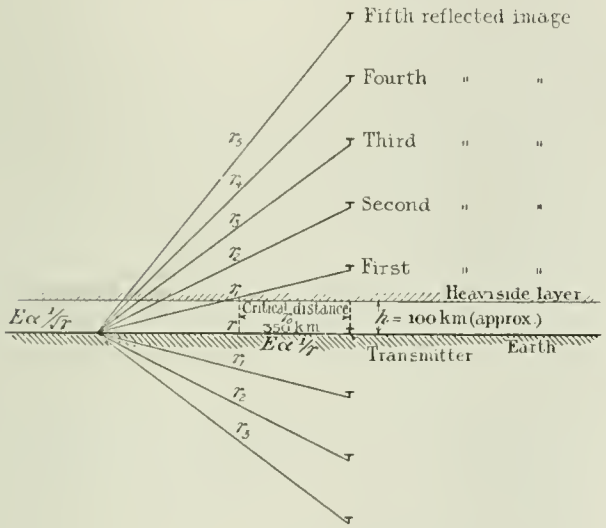


FIG. D.

Using this formula to determine r_0 we get, in the case of Marion and New Brunswick, a value of about 350 km. That is to say, at distances small compared with 350 km E should vary as $1/r$, but at distances large compared with 350 km E should be proportional to $1/\sqrt{(R \sin \theta)}$. This seems reasonable if we assume the height of the reflecting layer to be between 50 and 100 km.

This distance, i.e. 350 km, is the critical distance at which reflection or refraction from the upper layer begins to be appreciable. The reflection theory is at variance with the facts in one respect. On this theory there should be interference bands, i.e. places where the signals are abnormally weak, or abnormally strong. These interference bands are not observed except at night. In the daytime the upper reflecting surface is probably ill defined, which would account for the blurring of interference bands. The results obtained are in any case in agreement with some modified form of reflection theory and completely out of accord with the diffraction theory.

Mr. F. C. Lunnon: Mr. Elwell has mentioned in his paper that the chief reason for making measurements of signal strength is to accumulate data to assist mathematicians in arriving at a transmission formula which will give results in agreement with the facts. That being so, then the measurements, to be of any use, must be accurate. I think it would be of interest, therefore, to describe the method used in making the measurements of signal strength of the three American stations which Mr. Eckersley has mentioned. An aerial and a dummy aerial are arranged so as to be alternately equally coupled to a receiver. When the real aerial is coupled to the receiver the signals, by inducing an electromotive force in the aerial, produce a certain strength of sound in the receiver. When the dummy aerial is coupled to the receiver it is arranged that a local transmitter shall induce an E.M.F. of the same frequency as the signals in the dummy aerial, via a calibrated variable mutual inductance, and produce a sound in the receiver, which sound can be equalized with the sound produced by the signals in the real aerial by adjusting the E.M.F. induced from the local transmitter. If the dummy aerial has its constants made the same as the aerial constants, then, if we know the current in the local transmitter and the value of the mutual inductance between the local transmitter and the dummy aerial, we have quantitatively the E.M.F. induced in the dummy aerial, and consequently the E.M.F. induced by the signals in the real aerial. The apparatus for producing and measuring the local E.M.F. will be better understood by referring

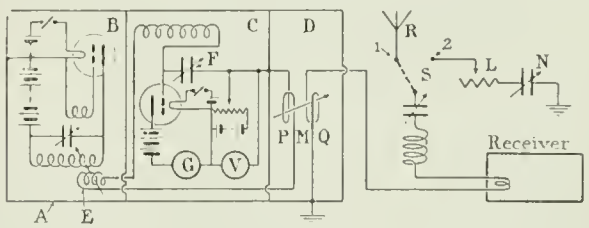


FIG. E.

to the diagram of connections in Fig. E. A is a wooden box, copper-lined inside and out and divided into three compartments, B, C and D, by two partitions which are also copper-lined on both sides; so that each compartment is shielded from the other two and the whole is completely shielded, there being negligible stray field from the box from the oscillatory circuits contained therein. In compartment B is a valve transmitting circuit coupled by the variable coupling E to an intermediate circuit in compartment C. The intermediate circuit is provided for the purpose of filtering out harmonics. The valve circuit shown connected across the intermediate circuit tuning condenser F is employed to measure the oscillating potential across the latter, from which the condenser current i can be calculated. Included in the intermediate circuit is the primary winding P of the calibrated

variable mutual inductance M in compartment D. The inductance of P is negligible compared with the total inductance in the intermediate circuit, so that the current in it is equal to the current i of condenser F. This current, being of the order of one milliampere, would be very difficult to measure by any direct method. One side of the secondary winding Q and one side of P are "earthed" to the copper lining, and the capacity coupling between them is inappreciable, perfect minima, separated by 180°, being obtained. Actually, in the set there are two calibrated variable mutual inductances giving a range of mutual inductance of from 0·25 to 25 μH, i.e. 100/1. The range of primary current i obtainable is 4/1, which gives a range of 400/1 in the value of the induced E.M.F. in the secondary winding Q. It is this E.M.F. which is induced in the dummy aerial and produces the local signals that are balanced against the received signals. Q is included as shown in the common part of the aerial circuit which can be connected by means of the change-over switch S to either the receiving aerial R or the dummy

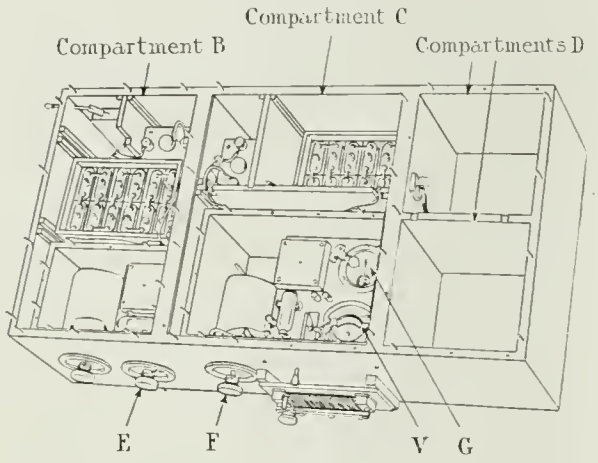


FIG. F.

aerial, the latter comprising the variable resistance L and tuning condenser N, by means of which its electrical constants are made the same as the real aerial constants. A relay is arranged to short-circuit Q when switch S is in position 1, and vice versa, so that by making reversals of switch S the received and local signals are heard alternately and the latter can be adjusted to give equality by varying the coupling E and the mutual inductance of M. Where this method, which was proposed by Mr. Round quite independently of Professor Vallauri, differs from that employed by the latter, is in the fact that Prof. Vallauri places his apparatus for producing and measuring the local E.M.F. at a distance of 30 metres from his receiver and relies on this distance to reduce any direct effects to a negligible amount, while in this method the apparatus, being entirely shielded, can be placed close to the receiver, thus greatly facilitating the operation of measurement. Also, Prof. Vallauri receives on a frame aerial and, instead of a dummy aerial, uses a second frame at right angles to the first. This has the slight advantage of associating atmospherics with the local signals, but with a little practice it is possible to allow accurately for the absence of atmospherics in the dummy aerial. The accuracy of the apparatus was checked in the following manner. Signals were transmitted from a certain station with a value of transmitting current sufficient to give a received current measurable by ordinary methods, e.g. a thermo-ammeter. The transmitting current was then reduced until the strength of the received signals was of the order of that of signals received from transatlantic stations, and the E.M.F.

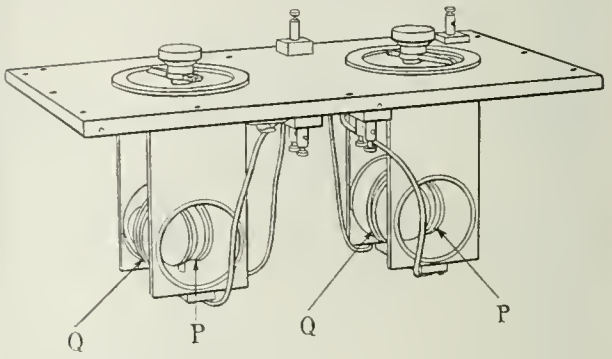


FIG. G.

induced in the receiving aerial was measured by the method which has been described. The resistance of the receiving aerial, which was the same in either case, was measured and the value of received current calculated. This experiment was repeated with three transmitting stations using a number of values of transmitting current and wave-length, and in each case the ratio of the large to small transmitting currents checked to within 3 per cent with the same ratio for the received currents. In order to obtain a quanti-

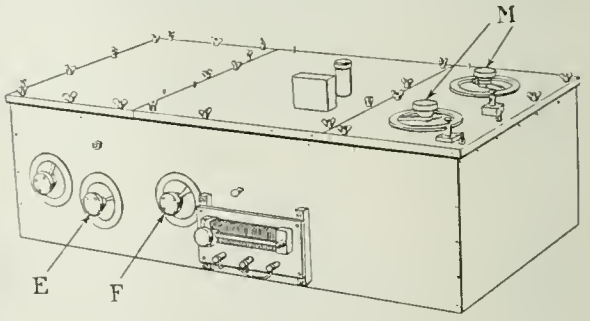


FIG. H.

tative measurement of signal strength in volts per metre the effective height of the receiving aerial was determined in the way described by Commander Pession in the April issue of the *Radio Review*. In this connection it might appear preferable, when making measurements of signal strength, to receive on a frame —the effective area of which can be accurately calculated—and so eliminate the necessity of determining experimentally the effective height of the receiving aerial, but a frame of practical dimensions has the disadvantage that the E.M.F. induced in it by signals of ordinary strength is of such a low value that, to

obtain equality in strength from the local transmitter, the current in the latter and the value of the mutual inductance M become so small as to render their accurate measurement very difficult. Also, in the case of the mutual inductance, the capacity coupling becomes comparable with the magnetic coupling and errors arise from that cause. For these reasons, and in consideration of the practical difficulties of obtaining a perfectly symmetrical circuit and eliminating stray capacity effects, there is no doubt that, up to the present time, a frame is inferior to a vertical aerial for the purpose of signal-strength measurement; but experiments are being continued to investigate the possibility of eliminating these sources of error in view of the many obvious practical advantages of a frame. An interior view of the shielded box containing the apparatus for producing and measuring the local E.M.F. is shown in Fig. F. Fig. G shows the two calibrated variable couplings which fit into compartments D, Fig. F, and Fig. H is an external view of the complete box. The distinguishing letters in Figs. F, G, and H refer to those parts of the apparatus similarly lettered in Fig E.

Major A. C. Fuller: It will, I think, be generally agreed that, for long-distance transmission, the use of the minimum power sufficient to secure a reliable service is much to be desired. This is sufficiently obvious on two grounds. First, economy in installation and maintenance of the transmitting gear—including aerials; second, the avoidance of what Mr. Elwell terms abuse of power. It will probably be agreed that at the present stage of practical development there are some conditions of atmospheric disturbance through which working is impossible. Leaving these conditions out, the essential transmitting power depends very largely on the receiving system. Such various values have been given for the energy which it is necessary should be received, that even could the engineer accurately forecast the transmitting powers necessary for any figure, he is not in a position to give a complete and standard solution. The first step, therefore, and certainly the easier, is to standardize the energy required for reception in any given area, and this involves a minimum standard reception efficiency. This efficiency will, of course, depend on several factors, but probably the chief of these are amplification and selectivity, and the best results are obtained by combining these two into selective amplification. It should not, therefore, be permissible to employ any receiver for long-distance work that does not come up to the required standard, which should be internationally determined. Tuned high-frequency amplifiers of great efficiency, particularly on fixed waves, can now be produced. In proportion as it may be found possible to provide a satisfactory reduction or elimination of atmospherics, so the transmitted powers of large stations can be reduced. I would suggest the following method of determining I_r. Set up a receiving system of standard efficiency or, for the present, of the type intended for use. With everything tuned to the proposed wave-length, the final tuned circuit should be connected to the grid filament of a Turner valve relay operating a recording instrument. The grid of the relay valve should be set to a potential, say 1 volt, more negative than the trigging point, and the recording tape left running. At this point in all probability several atmospherics and, possibly, jamming stations will be recorded. In this event the grid of the relay valve should be made more negative, say 1 volt at a time, and at each adjustment the tape should be run for a fixed time at various hours and the records observed. The first adjustment of grid potential at which the tape is practically free from marks should be carefully noted. Let us say this is — 8 with respect to the filament. The trigging point of the valve relay will be known and can be checked; assume it to be at — 5·5 volts, a difference of 2·5 volts. I_r is therefore that current which, when dealt with by a receiving system of standard efficiency, will produce in the assumed case under consideration an increase of positive potential of 2·5 volts on the grid of the trigging valve, or, in other words, that current which, when dealt with by a receiver of standard efficiency, will operate a standard valve relay adjusted to — 2·5 volts below the trigging point. Since this current will operate the receiving valve relay at an adjustment which has been proved to be free from interference (generally), it will give a satisfactory service. The value of I_r thus established may, however, be increased (in some agreed proportion) to provide a further factor of safety according to the importance and traffic needs of the service. Clearly I_r will vary according to the geographical position of the receiving station, and charts should be plotted to show these accepted values. Once determined, a reducing factor could be applied as advances in reception efficiency take place.

Mr. N. Lea: The problem of evolving a useful formula or series of formulæ hinges, as has been pointed out, on the taking of careful measurements of the effect produced by a given signal on whatever receiving system we chose to adopt. Before considering what receiving gear we shall employ, it is usual to say in general that the wave, on arrival, should have an amplitude which is some multiple of the arrival amplitude of the atmospherics or other disturbances. A practice seems to have grown up of expressing the arrival signal strength in terms of watts. This appears to be a tedious and altogether undesirable method as, without some reference to the nature of the receiving antenna, it is meaningless. A much better way is to hold fast to the idea of field strength and say that at a given point, in the sea of atmospherics, we must have so many lines of magnetic force or electric field per square kilometre. The magnitude of the fields we are dealing with is extraordinarily small and, consequently, great care has to be exercised in making measurements. There are, of course, a large variety of methods which can be employed, but perhaps it will not be out of place to outline a scheme on which I am working at the present time. A tuned multi-turn loop, 4 ft. square, is employed as an antenna because its constants are more easily determined than those of an elevated aerial. The loop is associated with a suitable amplifier, the output from which may be measured by audibility methods, or otherwise, for instance by a thermal device

or by double rectification and a micro-ammeter. The great objection to the use of any integrating device, e.g. an ammeter, is that it registers the atmospherics as well as the real signal, and it is only on rare occasions that results can be obtained in this way. It should also be remembered that it is quite possible to experience frequencies above audibility in the output of the amplifier. These frequencies may be produced by the beating of the heterodyne with stations on wavelengths differing widely from the nominal working wave. Most extraordinary effects have recently been experienced in this connection, and it is essential that great care should be taken to avoid errors creeping in in this way. We are therefore driven back to some form of audibility method. The shunted telephone is a rough-and-ready arrangement but is not accurate. A better result can be obtained by comparing the strength of the signal with that of a local oscillator whose strength can be accurately adjusted to induce an electromotive force of any desired value into the antenna. A small coil is included in the tuned loop circuit and is coupled to an oscillator of special design. The oscillator has its coils arranged so that the external field produced by them is very small, and the whole arrangement, together with the necessary batteries, is placed in a metal box having only a small opening for coupling purposes. The coupling coil has a capacity screen between it and the oscillator, but in spite of these precautions it is found that there is always some capacity mutual, which is quite comparable with the lower values of the inductive mutual. It is therefore necessary to find the relation between the angle of the coupling coil and the E.M.F. induced, by a separate experiment. For this purpose the oscillator is temporarily made more powerful, and E.M.F. readings are taken by means of a trigger relay across the coupling coil, and a curve is plotted showing this relation. It is then assumed—and I think it is a reasonable assumption—that by merely changing the scale of E.M.F. axis this curve will represent the state of affairs when the oscillator is made more feeble by using a reduced anode voltage. The new scale is determined by measurement of the weak and powerful currents in the oscillator itself. The experiments have not yet reached a stage where actual results can be quoted, but from preliminary calculations it appears that daylight signals from Annapolis (NSS) induce in the coil an E.M.F. of approximately $1{\cdot}5 \times 10^{-6}$ volts. Taking into account the number of turns on the coil (120) and the wave frequency, this means that the magnetic flux density is about 7×10^{-10}, a somewhat low value, but of the right order. The falling off in signal strength at night mentioned by Mr. Round is most noticeable. I entirely agree with him in regard to the great care necessary in making measurements of this kind, and I think it unlikely that the experience of the average amateur is sufficient to enable him to perceive and eliminate the many errors of experiment.

Professor G. W. O. Howe: Mr. Elwell has referred to the fact that I gave a figure corresponding to 400×10^{-10} watts. The figure which I actually gave was an electric field strength of 0·37 microvolt per cm, and this was based on assumption rather than calculation. I quite agree with those speakers who have expressed their preference for a given field strength rather than a given power in the receiving aerial. From various observations I was led to assume that 0·37 microvolts per cm would ensure commercial communication at all times, local thunderstorms excepted. Mr. Elwell suggests that this allows too liberal a factor of safety, and I was therefore interested to see that Mr. Eckersley's curves showed an electromotive force of about 500 microvolts in an aerial of 16 metres effective height. This is almost exactly one-third of a microvolt per cm, and I should like to know if the signals at Chelmsford are so strong as to make it advisable for the American transmitting stations to reduce their power. Mr. Elwell also says that in my tables based on the Austin–Cohen formula some of the values given for the requisite transmitting power are quite unreasonable. That the figures in some cases are surprisingly large I readily admit, but as to their reasonableness, I should like to know what criterion one is to take. Are the suggested figures any more unreasonable at the present day than the powers now being employed would have appeared to wireless engineers 20 years ago? Or again, are there any long-distance wireless stations at the present moment which are finding the power more than sufficient, or even sufficient, for a regular 24 hours' high-speed service? I think not.

Dr. W. H. Eccles: The fundamental law in this subject is that energy distributes itself in all directions according to the inverse-square law, and Hertz in his formula made clear certain details. The factors λ, h_1, h_2 and 377 are due to him. The measurements we have heard of to-night seem to show that the formula holds good for distances up to 250 km on a large scale. For great distances Poincaré adjoined an exponential factor to allow for diffraction. Then the Austin measurements showed that this factor might account for the diffraction round the bend of the earth and for the absorption of the medium; both these phenomena take place. In addition, there seems to be a refracting effect, as Mr. Eckersley pointed out, which has been debated very often, but the existence of which has never been very directly demonstrated. These three things are incorporated in the exponential term in the Austin–Cohen formula, so that it is very remarkable that the formula is so simple and useful. In discussing the actual measurements made by the American Navy with a rather crude method, i.e. the audibility method, Austin had to average factors that vary enormously. For instance, no wireless engineer listening in a receiving apparatus is very surprised if the strength of the signals falls 100 per cent in less than 10 minutes or goes up 800 or 1 000 per cent in the same time. With anything that changes so widely an investigator is naturally very doubtful whether he is taking the average in the proper manner. I think it is very surprising that the Austin–Cohen formula comes out wrong to the extent of only 80 per cent, as shown by Mr. Eckersley's daylight measurements. The optical analogy of the mirage may help in making us realize how variable these phenomena are. In a country like Egypt

it is possible to see trees and cities apparently up in the air, the actual objects being very often at great distances. Now, no one will say that because of those mirages we must abandon the ordinary laws of optics. I regard such a thing as the fact that Horsea is heard in New Zealand every night during certain parts of the year as an effect rather like a mirage, and we must not say that the Austin–Cohen formula is useless just because of these freaks. As a matter of fact, signals have sometimes been heard at great distances and lost at stations much nearer the transmitting station. I myself have been in the Arabian Sea when a signal from India was jammed by one from Aden, although the latter place was twice as far from the ship as the Indian station was. That kind of thing is quite frequent, and therefore we must not expect great accuracy in these formulæ and their applications.

I support Mr. Gray's remark that the real estimate of the value of a service is the average number of hours per day during which it is possible to work throughout the year. If we could get a formula which would show that for such and such an antenna current in a certain antenna we could work for 6 hours per day at a certain place on the average, and that if we liked to increase the antenna current to such and such a figure we could work 7 hours per day, and for another figure we could get 8 hours per day, it would be of great value. Such a formula would resemble that concerned with the propulsion of ships. If a ship designed to go at an economical speed of 25 knots is desired to go at 26 knots, much more coal in proportion has to be burned for the extra knot than would be required per knot of the proper speed. If it is required to make the ship travel at 27 knots it costs still more, and so on. At first the coal consumption goes up as the cube of the speed, afterwards as the fourth power, and it mounts faster and faster as the speed is still further increased. It is somewhat the same in wireless telegraphy. It is possible to get 6 hours' working over a certain range for a certain cost; for 7 hours' working we must pay more; for 8 hours' working we may have to pay a lot more; for 12 hours' working we may have to pay a price which is quite prohibitive.

We are passing at this time through an important epoch in our subject. The measurement of radiation is becoming fashionable, and apparatus for this purpose is being devised wholesale. One result of such measurements as have been made is that we are beginning to remodel our views. For instance, we have heard to-night that transmission across the Atlantic in the daytime is better than at night; and we shall have to get accustomed to the fact that the inverse-square law, though accurate at small distances, gives computed results rather too small at medium distances and too large at larger distances.

Mr. P. R. Coursey (*communicated*): Many interesting points have been raised in this discussion, and it is to be hoped that when they are all collected together it may become possible to summarize from them some data useful to the radio engineer and designer. While appreciating Dr. Eccles's remarks as to the surprising way in which the Austin–Cohen formula fits the observations, it nevertheless seems certain not only that it has its limitations but also that the results of measurements over long distances would fit in more satisfactorily if at least one modifying factor were introduced. In this connection it is of interest to refer to recent measurements which support the suggestion made on page 678 by Mr. Elwell that the phenomena of transmission may be different in different directions of transmission. It is surprising to note the discrepancies between calculation and measurement observed by Prof. Vallauri in his measurements in Italy of the signals from Annapolis,* as compared with the good agreement found by Dr. Austin in his recent measurements in America of the signals from Bordeaux.† Whereas the former obtained an average strength many times greater than that given by the Austin–Cohen formula, the latter obtained almost perfect agreement. The signalling ranges in the two cases are 6 917 and 6 160 km respectively, a difference which of itself would seem scarcely sufficient to account for the observed differences of strength. It is possible that the use of two different methods of carrying out the measurements may account for part at least of the differences, but it would appear to be highly desirable that on this question of East and West transmission alone much more data should be obtained. Several speakers have already criticized the form of the Austin–Cohen expression, and have suggested the desirability of a term introducing $\sqrt{(\sin\theta)}$. I should like to inquire as to the underlying reason (if any) for the term $\sqrt{(\sin\theta/\theta)}$ used by Mr. Elwell and by Mr. Eckersley. It would seem that the correct place in the formula in which to take care of the change in effective absorption at long distances (and this term is only effective at long distances) is in the exponential factor, even if this involves the introduction of additional terms of the form $[ad_1 + \beta d_2 + \gamma d_3 + \ldots]$ as an extension of the suggestion put forward by Mr. Elwell. These various absorption factors and distances could allow for the portions of the path over sea, over land, over forests, over mountains, etc., and if necessary further terms could be added, becoming effective only when d is large.

Major H. P. T. Lefroy (*communicated*): It seems to me that no further effective work can be done towards the more accurate determination of the wireless transmission "attenuation constant" α until: (1) A standard method of measuring received energy has been definitely decided on internationally. (2) Such a method has been employed on a large scale by those interested in wireless engineering all over the world, and numerous reliable measurements have been thus obtained for analysis, care being taken that in each case, date, time, wave-length, weather conditions, etc., are carefully recorded, thus enabling the analyst to trace simultaneous measurements. As regards transmission formulæ, it seems to me that the time has come when the Austin–Cohen and other kindred formulæ should be recast to bring them into line with the cable attenuation formula, partly as a move towards the unification of all telegraphic and telephonic work, and

* *Radio Review*, 1920, vol. 1, p. 652.
† *Ibid.*, 1921, vol. 2, p. 301.

partly with a view to the more general recognition of the marked similarity of form of the wireless and cable attenuation formulæ. With this object I would suggest that: (1) The received current be shown as a function of the sending current, instead of the converse as is now usually the case. (2) That the Austin–Cohen formula, etc., be stated in terms of frequency, instead of wave-length as at present. (3) That the "attenuation constant" α shall embody the frequency term of the exponential factor, and not merely the numerical term as at present. (4) That the quotient I_r/I_s be known as the "attenuation factor" for the wireless transmission under consideration. Embodying the above alterations in the Austin–Cohen formula, it becomes

$$I_r = I_s \times \epsilon^{-\alpha d} \left[(0{\cdot}00126 f h_s h_r / dR) \times \sqrt{(\theta/\sin\theta)}\right]$$

where $$\alpha = 0{\cdot}00000274 f^{0{\cdot}5}$$

This formula for wireless transmission seems to be closely analogous to the formula for cable transmission, namely

$$I_r = I_s \times \epsilon^{-\alpha d}$$

where $\alpha = kf^n$, as in the formula above, the value of the terms within the square brackets being unity in the case of a cable. Pernot has recently shown that for the Seattle-Sitka cable, using alternating current up to a frequency of 1400, $\alpha = 0{\cdot}0032 \times f^{0{\cdot}405}$, and that for a typical Atlantic cable $\alpha = 0{\cdot}000891 \times f^{0{\cdot}485}$ probably, whereas for three-dimensional (damped) radiation Austin makes $\alpha = 0{\cdot}00000274 \times f^{0{\cdot}5}$, as shown above; it seems probable that Dr. Fuller's results using continuous waves (arc) would, if measured by an absolutely reliable detector, agree with sufficient accuracy with an attenuation constant in which the power of f would not exceed $0{\cdot}5$, whilst the numerical factor would be larger than that given by him; it will be observed that Sommerfeld suggests $0{\cdot}333$ as a suitable power for f in the exponential factor. As regards nomenclature, I suggest that the product $h_s I_s$ be known as the "transmission constant" of a wireless station for a given wave-length; all stations could then be calibrated and results tabulated; amateur stations could be controlled by their transmission constant as well as by their wave-length and damping. Similarly the product $h_r I_r$ might be known as the "reception constant" of a wireless station for a given wave-length and using its normal detector. In future, lists of wireless stations should, when possible, state the transmission and reception constants as well as the wave-length and call sign and working periods.

Mr. J. Mamlok (*communicated*): Mr. Elwell has taken the point of view of the engineer, who has to establish a long-distance radio communication, and finds himself without reliable quantitative data concerning either the laws of transmission of the energy radiated and its absorption by the medium it traverses, or the amount of energy necessary to ensure a continuous service throughout the whole year, no matter what interference there may be from atmospherics. This question is of extreme importance in radio-technology, but at the same time it may be considered from a much more general standpoint. It is, in fact, of considerable interest from the point of view of knowledge of our atmosphere and of the electrical phenomena which take place in it. Various phenomena, such as the zones of silence observed by de Groot in the wireless radiation measurements which be carried out for the Dutch Government, the irregularities observed in making radiogoniometric observations, the variations in absorption at sunrise and sunset and accompanying the aurora borealls, will no doubt give, indirectly, information about the electrical state of the upper atmosphere. The question of atmospherics may be divided into two parts, their generation and their propagation, the second being analogous to the problem of the propagation of signals. It may be remarked that the study of atmospherics had been undertaken before the discovery of wireless telegraphy, and that the antenna had been employed for this purpose by the Russian meteorologist Popoff. It would appear that, with the creation of powerful wireless transmitting stations and extremely sensitive receiving apparatus, it should become of capital interest to meteorologists to take up this study again, taking advantage of modern wireless instruments and plant. The establishment of a general mathematical formula for long-distance transmission would appear to be impossible, in view of the extreme irregularities of such transmission. Hence, the work to be undertaken would be very extensive and would require a very elaborate organization to carry out observations and record the results over a period of many years. Meteorological stations would appear to be particularly adapted for work of this nature, which is likely to give results of great value from the purely scientific point of view as well as that of radio-telegraphy, provided they are properly co-ordinated and the necessary accuracy is secured. It is not impossible that such a research might eventually develop into a permanent adjunct to the meteorological service for predicting the weather. It would be necessary to arrive at standard methods of measurement, and possibly a standard form of apparatus. A classification of atmospherics, and of the methods of determining their intensity, should be elaborated in order to render possible methodical observation of atmospherics simultaneously with that of the intensity of the received signals. Arrangements should be made for the different receiving stations to carry out measurements of the same transmitting station at the same time. To this end it would be useful to invite the large wireless stations to contribute to the research by making special transmissions at certain hours of the day, with definite aerial currents, and at various wave-lengths, as was done, for instance, by the Lyons and Nantes stations when they sent standard emissions for the radiation measurements organized by the French Government. It would be of assistance in this matter if a Committee charged with the study of the question were to publish a monograph, as detailed as possible, dealing with the subject. Until a methodical and co-ordinated research that requires a certain delay is organized, measurements due to individual efforts will certainly be a great help

for the sake of the definite organization, and at the same time will furnish some provisional data useful to designers of wireless stations.

Mr. C. F. Elwell (*in reply*): Since writing the short paper with which this discussion was introduced it has been possible by means of the co-operation of the Admiralty and of Professor Vallauri to measure at Leghorn the strength of signals sent out from the Admiralty continuous-wave station at Horsea Island. The method, the apparatus, and the symbols employed were identical with those adopted by Professor Vallauri in the measurement of the strength of the magnetic field of the waves emitted by Annapolis.

d	1 182 km
Azimuth of Horsea at Leghorn	317·5°
Azimuth of the measuring aerial	306°
β	11·5°
λ	4·5 km
S	$1{\cdot}404 \times 10^7$ cm^2

Absorption factors $\begin{cases} \text{Austin } m_1 = e^{0{\cdot}0015 d/\sqrt{\lambda}} = 2{\cdot}31 \\ \text{Fuller } m_2 = e^{0{\cdot}0045 d/\lambda^{1{\cdot}41}} = 1{\cdot}89 \end{cases}$

$M_{\alpha=0}$, for $d_1 = 23$ cm is $1{\cdot}23\ \mu$H,
$M_{\alpha=0}$, for $d_2 = 30$ cm is $0{\cdot}59\ \mu$H.

$$hI = \frac{\lambda d}{377}\sqrt{\left(\frac{\sin\theta}{\theta}\right)\frac{3\times 10^5}{S\cos\beta}}\, mI_2 M$$

where λ, d, h are in km,
S is in km^2,
I, I_2 are in amperes,

$$M = M_{\alpha=0}\frac{\cos\alpha_1 - \cos\alpha_2}{2} \text{ in } \mu\text{H}.$$

upon whether damped or continuous waves be employed. This may possibly be taken care of as a different function of the wave-length or α or both.

An amateur who is just able to receive signals, when they are strong enough to operate inefficient apparatus, was not intended as a fit observer for the data required. The term was intended, of course, to cover those whose education, training, apparatus available and inclination fit them for the class of measurements suggested. It is quite true that audibility methods leave much to be desired, although perhaps not affording such large discrepancies as the behaviour of the transmitted waves. However, no doubt simpler and more reliable methods will be forthcoming.

The suggestion made, that the amount of energy which it is desired should be received need not be fixed when designing a long-distance transmission, is hardly in accordance with practice. Modern amplifiers have a lower limit. At Leghorn the $0{\cdot}1 \times 10^{-10}$ watt received from Annapolis was passed through an 8-stage amplifier before it could be said "to operate" satisfactorily the receiving device proposed to be used. The transmitting energy which could give this energy on one day might easily give so much less on the following day, that the amplifying arrangements would be impotent to transform it into sufficient energy to operate the receiving devices satisfactorily.

It may be a matter of indifference whether we use high or low receiving aerials, but unfortunately we have not yet arrived at the point where we can say the same about the transmitting aerial. What is required is a formula which will enable one to plan the most

Day	G.M.T.	d_2 in cm	α_1	α_2	I_2 in micro-amp.	$h I_s$ Austin	$h I_s$ Fuller	$h I_s$ Horsea
13/4/21	00·01	23	19°	34°	23·2	11·8	9·8	1·65
	11·54	23	4°	15°	24·1	3·4	2·8	1·70
	11·59	23	7°	16°	24·3	3·4	2·8	1·7
	19·55	23	6°	16·7°	23·0	3·8	3·1	1·6
14/4/21	00·01	30	26°	42°	23·3	7·7	6·3	1·77
	11·54	30	11°	19·5°	23·3	1·9	1·6	1·82

It will be noticed that the agreement between the Austin and Fuller formulæ is remarkably close for this distance. But they both give results considerably in excess of the actual value of $h_s I_s$ of Horsea, which I have tabulated upon the assumption that h_s is 0·1 km for the flat-top aerial suspended from three wooden lattice towers each 0·140 km in height. It will also be noticed in these tests between Horsea and Leghorn that the night values of I_r were considerably larger than those received during the day.

My opinion of the difference in the amount of power required as between damped and continuous waves is based upon tests carried out in 1913 by Dr. Austin between Arlington, Key West, and Colon. These were most convincing. It seems only reasonable to expect some difference in the transmission formula depending

economical transmitter for a given service. More accurate knowledge of the attenuation in terms of the costly items h_s and I_s would be of great value to the art. The wish to have signal power greater than atmospherics is uppermost in the minds of all radio engineers. All suggestions as to means which will enable this result to be obtained, whether by transmission or receiving station design, will be most welcome. It is curious to note that, parallel with the development of the amplifier, the power of transmitting stations has been increased enormously. The tendency in the past has been to look to the performance of existing transmitters with their corresponding receivers. Not content with the ability of the transmitter to furnish to the receiver for 8 640 hours or less per annum the minimum energy required, a decision would be made

to increase the transmitting energy, by increasing either I_s or h_s, or both. The arc station at Lyons started service with the United States with 150 amperes in the aerial and 120-metre towers. The amperes in the aerial were next increased to 200, then the towers to 180 metres, and finally the amperes in the aerial to 250. Would this have been done at considerable expense if a few more amplifying stages could have accomplished the desired result? And in view of all this data the Bordeaux station was planned with 250-metre towers and 500 amperes in the aerial in order that satisfactory signals could be obtained over a greater percentage of the hours in a year.

The suggestion by Professor Marchant that the Einthoven string galvanometer should be adopted as a measuring device seems to be a good one. It is a device which lends itself to recording methods, and these records could be calibrated. It might also be useful in a system of recording the strength and duration of certain classes of atmospherics.

The method, apparatus and carefully obtained results communicated by Messrs. Eckersley and Lunnon, represent the class of material which is sought in order that a better transmission formula or method of designing long-distance transmissions may be evolved. The suggestion made that receiving apparatus should come in for a certain amount of control and inspection is a step in the right direction, if power is not to be abused. It was found practicable and advisable to regulate and inspect the damping of spark transmitters. Legislation is now being thought out for wave-lengths, power, etc., and the opportunity to regulate inefficient receivers should not be overlooked.

Major Lefroy has stated the case. The first step will be to encourage a discussion on (1), "A standard method of measuring received energy." Once a good method has been decided upon and standardized the second step can be undertaken, and I have no doubt that much good will result, even if no better transmission formula than the Austin–Cohen be evolved.

WIRELESS IN THE ROYAL AIR FORCE.

By Major J. Erskine-Murray, R.A.F., Member.

(*Lecture delivered before the* Wireless Section of The Institution, 14 *May*, 1919.)

ABSTRACT.

As the officer in charge of the wireless experimental section of the Royal Air Force, and on behalf of my brother officers of the experimental establishment, I have been asked by the Air Ministry to describe the wireless gear which has been produced and used during the war by the Air Force. Of course it will be totally impossible for me to go through the whole in detail. The lecture, therefore, will be confined to a general technical description and, as this is a technical audience, the impossibility of doing more, in this lecture, than giving a preliminary sketch will be understood. It is hoped that further and more detailed papers may be given later on as the conditions of the Service permit.

To show that great detail is impossible, I will draw attention to Table I, which represents some very interesting measurements, taken during the war, of the variables and constants of an oscillating valve circuit. Twelve or thirteen of the 29 quantities illustrated in the table are actually read simultaneously. The rest of the quantities are measured or calculated; those marked "M" being all measured either before or after, the others being deduced by calculation from the measured quantities. It will be seen that the matter is far too complicated to be considered in detail at present. I could lecture for a month on that table.

Table I.

Oscillating Conditions in which the Wave-form of the Anode Current in T56 has been Plotted.

No.			A	B	C	D
1	—	Valve	T2B	T2B	T2B	T2B
2	C	Wave-length, metres	6 850	3 600	3 500	3 600
3	M	Aerial inductance, microhenries	2 462	2 462	2 462	2 462
4	M	Anode inductance, microhenries	165	812	812	812
5	M	Aerial capacity, milli-microfarads	5·50	1·47	1·47	1·47
6	M	Aerial current, amperes	3·50	1·24	1·64	1·92
7	M	Resistance of aerial, ohms	0	20	5	0
8	C	Resistance of instruments, ohms	3·6	5·0	5·0	5·0
9	C	Total high-frequency resistance, ohms	3·6	25·0	10·0	5·0
10	M	Supply current, milliamperes	79	70	36	20
11	M	Supply voltage, volts	1 000	1 000	1 000	1 000
12	C	Input, watts	79	70	36	20
13	M	Added anode resistance, ohms	148·6	148·6	148·6	148·6
14	M	Max. voltage across added anode resistance, volts	50·0	30·0	22·8	22·1
15	C	Max. anode current, milliamperes	337	202	153	148
16	M	Min. anode voltage, volts	283	254	22	− 69
17	C	Anode voltage amplitude, volts	717	746	978	1 069
18	M	Max. grid voltage, volts	252	135	148	149
19	M	Min. grid voltage, volts	−1 136	− 368	− 556	− 640
20	M	Mean grid voltage, volts	− 404	− 111	− 189	− 220
21	C	Grid voltage amplitude, volts	+ 656 − 732	+ 246 − 256	+ 337 − 367	+ 369 − 420
22	M	Grid current, milliamperes	11·3	3·3	5·6	6·5
23	M	Grid leak resistance, ohms	35 800	33 650	33 650	33 650
24	M	Grid condenser, microfarads	1	1	1	1
25	M	Filament current, amperes	3·6	3·6	3·6	3·6
26	C	Total high-frequency energy, watts	44·1	38·5	26·9	18·4
27	C	In instruments, watts	44·1	7·8	13·5	18·4
28	C	In aerial, watts	0	30·7	13·5	0
29	C	Aerial efficiency, per cent	0	45	37·5	0

To start with we had two experimental sections, the Royal Naval Air Service Wireless Section at Cranwell Air Station in Lincolnshire, and the Royal Flying

Corps Laboratory at Woolwich, with a testing aerodrome at Biggin Hill. The work done by the Royal Naval Air Service W/T Section was mainly in connection with the design of aircraft wireless telegraph instruments for comparatively long distances of communication, and in the development of wireless navigation methods, while the Royal Flying Corps was concerned in the development of aircraft radio-telephony and other short-distance communications.

I have not time here to deal with either the operation or the organization of wireless telegraphy, and I cannot describe what happened on the battlefields—to begin with I was not there. Therefore I intend to stick mainly to the development of the apparatus which has seen service, or very nearly seen service, during the war.

We start off with the seven tables numbered Table 2 to Table 8. These give a list, with short details, of the different types of gear which, with the exception of the first three or four of the spark transmitters and of one or two of the crystal receivers, were developed after the war commenced.

There has been a good deal of work done during the war, as can be seen. In these tables quite a number of items will be found, every one of which required experiment and design and either served a different purpose from, or was an improvement on, the one before. Some of these are really remarkable instruments. The spark sets commenced with No. 1. It was not, perhaps, a very brilliant beginning, but it was an exceedingly useful set. It does not differ very much from the ordinary wireless telegraph ground set, except in size and convenience; it runs off a battery, and has a comparatively short range when used from an aeroplane. The set has probably seen more service than any other in the war, because it was used for artillery observation and spotting at short distances. Its short range is an advantage, for the simple reason that it does not interfere with stations which are a reasonable distance away. The observer, as a rule, only communicates with his own base, and thus a very large number of these sets could be used on the Western Front simultaneously. Then we get to the various improvements on it. Nos. 1 to type 52M are driven by batteries, but type 52A has a little alternator driven by a windmill on it and has a somewhat longer range. Then we have some of the larger sets, most of them driven by a similar type of windmill and some sort of generator.

We come next to the use of the "tonic train," that is to say, continuous high-frequency waves varied so as to be audible in a receiver which is not oscillating; to be audible, in fact, in any receiver which will receive a spark signal. In all the modern textbooks one finds pictures of tonic trains and things of that sort, in which the scales are so hopelessly different that one does not realize the true proportion of things. One usually sees a tonic-train diagram with about six waves in a bulge, then it goes to zero, then there are another six waves, and then to zero again, and so on—the proportions being so utterly different from the truth that it gives a wrong impression.

At the top of Fig. 1 there is a spark represented as having 15 oscillations before it falls to about 25 per cent of its maximum. Below that a continuous wave is starting. From the black vertical line to the other black vertical line is 1/000th of a second and, as there are 200 waves in this space, it will be noted that the frequency is 200 000 per second, that is a 1 500-metre wave, which is the sort of wave used by moderate-sized ground stations and in some aircraft.

The first line below the continuous-wave diagram is interrupted continuous waves, and we can make the ratio of the wave train to the blank space as large as we like. The next is a tonic train, produced from an alternator with one valve in the transmitter. Still,

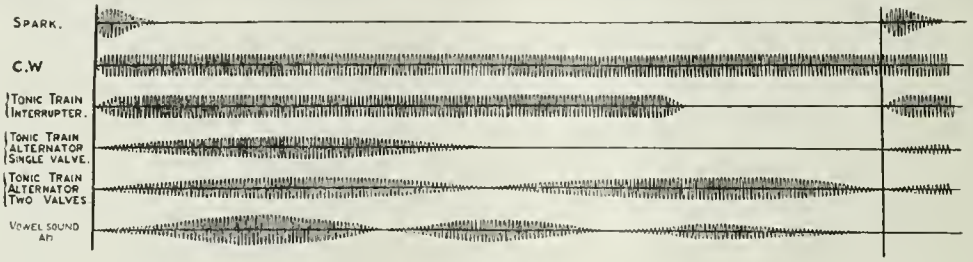


FIG. 1.

we get a long train of waves, much longer than from a spark. The next one is rectifying on both sides of the cycle, and so we get a train with a minimum in the middle. This would in fact give us a frequency of 2 000 a second, which is a high note.

The lowest line of all is what we get if we transmit the vowel sound A, or something resembling it, through a wireless telephone in which the carrier wave has a frequency of 200 000. There are three bulges of different size, which would give the vowel sound A when rectified. The diagram shows the enormous difference between continuous-wave tonic train and the spark. With the spark, to get the same power, one must have a very much larger voltage amplitude than is shown, but otherwise the dimensions of the diagram are reasonable.

In the general type of continuous-wave transmitter that has been developed in the Air Force the aerial has a condenser in it; below is a small vacuum tube attached to the aerial and fitted into the top of the instrument, so that the operator can see his signals as he makes them, the tube lighting up when the key is pressed. With continuous waves we do not hear anything; hence the use of the tube to make the signals visible. The high-frequency current goes through the aerial coil, down through a condenser and ammeter to earth. In aircraft the "earth connection" is to

the metallic part of the aeroplane or airship. On the right-hand side we have two valves with high-tension direct current applied. The supply is from one of these small direct-current generators on the table, which weighs only about 11 lb. and gives about 150 watts at they have to pass through a grid of fine spiral wire wound round, but not touching, the filament, and the grid is in circuit with a coil, coupled to the aerial circuit and so connected that when the current in the anode circuit becomes large the grid becomes negative and

ROYAL AIR FORCE WIRELESS SETS.

TABLE 2.

Spark Transmitters (Aircraft).

Transmitter	Wave-lengths, Metres	Weights	Power		Uses	Remarks
			Source	Input, Watts		
No. 1	100 to 260	9 lb. 12 oz. without batteries	6-volt accumulators	30	Artillery co-operation on all fronts	—
No. 2	100 to 260	9 lb. 12 oz. without batteries	6-volt accumulators	30		
T. 52	100 to 335	9 lb. 12 oz. without batteries	8-volt accumulators	40		
T. 52M	150 to 410	9 lb. 12 oz. without batteries	8-volt accumulators	40		
T. 52A	150 to 410	Transmitter 9 lb., Alternator 8 lb.	Alternator	150	Long-range spotting and anti-submarine	First windmill-driven set
T. 52B	150 to 410 500 to 600	50 lb. with batteries	16-volt accumulators	100	Grand Fleet spotting and patrol seaplanes, etc., and anti-submarine	—
T. 54A	200 to 335 500 to 600	Transmitter 25½ lb. Accumulators 21½ lb.	Two 14-volt accumulators	120		
T. 55A	200 to 600	Set complete 86 lb.	Alternator 500 cycles at 180 volts	2 000	—	—

TABLE 3.

Continuous-Wave Transmitters (Aircraft).

Type	Wave-length, Metres	Weights	Valves		Power		Uses	Remarks
			No.	Type	Source	Input, Watts		
T. 57	1 000 to 2 500	Transmitter 8 lb., Generator 11 lb., 6-volt accumulator 12 lb.	2	A, A2, or F	D.C. generator 1 200 volts	75	Seaplanes, etc.	Windmill driven
T. W	900 to 1 100	Transformer 4 lb., B.T.H. generator 18 lb., 6-volt accumulator 12 lb.	1	B (Army)	600-volt D.C. generator or battery	40	—	—
Mark II	800 to 1 050	Transmitter 5 lb., B.T.H. generator 18 lb., 6-volt accumulator 12 lb.	2, one being spare	B	—	40	—	—

1 200 volts, driven by a little windmill. When alight, the filament gives off a continuous stream of negative electrons, and the anode is continuously kept at a high positive potential, from 1 000 to 1 500 volts. The electrons stream across, forming a negative current from the filament to the anode, but in their course chokes it off. Thus the action of the whole circuit is like that of a single-acting slide-valve engine. When the grid becomes negative it prevents negative electrons getting from the filament to the anode; whereas if it is positive it attracts them towards itself and a vast number of electrons shoot through the grid, and the

current in the anode circuit is increased. Thus the potential of the grid controls the anode current.

Thus if a valve be connected to a circuit which can oscillate it will produce a negative potential at the proper time and a positive potential at the proper time on the grid, and thus control the current between the filament and the anode, and each rush of current from the filament to the anode gives the current in the oscillating circuit a hitch forward, almost exactly like the escapement of a watch. Thus each pulse from the anode current keeps up the oscillation, and the energy of the oscillation is radiated from the aerial. On the grid itself, in order to keep its average potential negative, which is important in view of maintaining the oscillations, we have a condenser and a leak of several thousand ohms through the key. When the key is down, oscillations commence, because some of the negative charge from the grid is able to leak away. When the key is up the grid becomes highly negatively charged, which chokes off all current in the valve, and the oscillation in the transmitter stops; thus we are not directly keying the high-tension supply, but the valve itself is acting as a relay and controlling the current.

Many methods for the speech modulation of the high-frequency current in order to obtain telephony have been tried. In the method adopted a microphone

TABLE 4.

Wireless Telephone Transmitters.

Type	Wave-length, Metres	Weights	Valves		Power		Uses	Remarks
			No.	Type	Source	Input, Watts		
T. 56B telephone attachment	1 000 to 2 500	—	1	T. 2B or T. 2A	D.C. 1 000 to 2 500 volts	250 to 300	Ground stations	—
Telephone W/T., R.A.F. Type XI	300 to 1 200	—	2, one power, one control	T. 2B	D.C. 1 000 to 2 000	200 to 300	Ground stations	—
W/T sets, field portable, 120-watt tonic-train or telephone	600 to 1 000	—	3	1 Marconi T. 2 erecta, one being a spare	Special H.T. unit producing A.C. from 28-volt battery	120	Ground stations	—
T. W aircraft Mark II	350 to 450	Transmitter 9 lb., B.T.H. generator 18 lb., 6-volt accumulator 12 lb.	2, one power, one control	B or F	B.T.H. D.C. H.T. generator 600 volts	40	Aircraft	Wind driven
T. 57 with telephone attachment	1 000 to 2 500	Transmitter 8 lb., Attachment 5¼ lb., Generator 11 lb., 6-volt accumulator 12 lb.	2	A, A2 or F	Newton D.C. H.T. generator	75	Aircraft	Wind driven

TABLE 5.

Crystal Receivers (Aircraft).

Type	Wave-length, Metres	Weights	Detector	Uses	Remarks
T. A	100 to 900	With relays 15 lb.	Carborundum	—	—
T. B	100 to 600	—	Carborundum	—	—
T. D	150 to 335	Without relays 5¼ lb.	Carborundum	—	—
Mark III, Mark III*, short-wave tuners	120 to 700	—	Carborundum perikon or valve	—	—
Naval "C" type receiver	300 to 8 000	—	Crystal or valve	Ground stations	—

TABLE 6.

Valve Receivers (Aircraft).

Type	Wave-length, Metres	Weights	Valves		Function of Valves	Uses	Remarks
			No.	Type			
C.W. Mark II	400 to 800	Receiver and remote control 8 lb., 6-volt accumulator and H.T. unit extra	3	R	1 rectifying. 2 low-frequency magnifying	—	—
T. F receiver	150 to 2 500	Receiver 9 lb., 6-volt accumulator and H.T. unit extra	3	R	1 rectifying, 2 low-frequency magnifying	—	—
Tuner aircraft Mark III	350 to 450 600 to 800	Receiver with remote control 11 lb., 6-volt accumulator and H.T. unit extra	3	R	1 rectifying, 2 low-frequency magnifying	—	—
Tuner aircraft R.A.F. T. 10	350 to 550	Receiver with remote control 8 lb., 6-volt accumulator and H.T. unit extra	5	4 C, 1 D	2 C valves high frequency, 1 D rectifying, 2 C valves low frequency	—	—
Mark III C.W. receiver	700 to 1 200	—	2	R	1 rectifying, 1 magnifying	—	—

TABLE 7.

Valve Amplifiers.

Type	Weight	Valves		Function of Valves	Remarks	Uses
		No.	Type			
Ground R.A.F. T. XII	—	5	C and D	4 C valves high-frequency magnifying, 1 D valve rectifying	Detecting amplifier 300 to 800 metres	Ground stations
T. b relay	Relay 6½ lb., 6-volt accumulator and H.T. unit extra	3	R	Low-frequency magnifying used with T. B receiver	Amplifier only	Aircraft
T. h	10¼ lb.	7	R	3 high frequency, 1 rectifying, 3 low frequency	Detecting amplifier, French type, hard valves, 900 to 10 000 metres	Aircraft

TABLE 8.

Tonic Train Transmitter (Ground).

Type	Wave-length, Metres	Valves		Power		Remarks	Uses
		No.	Type	Source	Input, Watts		
W/T set field portable 120-watt	600 to 1 000	1	Marconi T.	Special H.T. unit producing A.C. from 28-volt battery	120	—	—

with a low-tension battery and a transformer work on to the grid and filament of what is called the control valve. Every variation of the pressure of the air due to speech is reproduced as a change of voltage on the grid of this valve, and therefore on its anode, and, as the anode voltage of the control valve controls that of the power valve, the latter corresponds also to the speech current. As the energy radiated depends on the anode voltage of the power valve, it thus depends on the vibrations of speech, and these are reproduced as variations of radiated power. The control valve acts more or less as a sink of energy; the power valve would be always giving out uniform power, but when we start the control valve there is a variation in the radiation due to the rise and fall of potential at the point at the head of the choke. There are telephones put on by which a man is able to listen to his own speech when transmitting. That is the usual type of transmitter which we use in aircraft.

The aerial used in aircraft is usually about 150 feet long and is just a single trailing wire. It is wound on a reel and has a lead weight at the bottom. The aerial is let down a tube through the deck. In certain cases we have tried to use aerials made up entirely within the machine, consisting of wires spread about on the wings and inside the fabric, and we have been able to get quite reasonable ranges receiving on this fixed aerial from an aeroplane with a trailing aerial, e.g. speech at perhaps 7 to 10 miles from a machine fitted with a 100-ft. trailing aerial. Fixed aerials were designed for the purpose of fighting, because it is inconvenient, naturally, for fighters to have 150 feet of wire trailing below them; if they "stunt" it may get entangled and interfere with their controls.

There is one point I forgot to mention about tonic train. It must not be confused with the beat waves of a closely coupled spark circuit. It is not the same thing, since there is no change of phase. In the spark circuit there is a change of phase at every beat, but in the tonic train there is not.

Of course one great difficulty in wireless telephony has been to obviate the sudden step from non-oscillation to oscillation which occurs on raising the grid voltage. In a case where we have to superimpose the telephonic waves on the high-frequency waves this has been a serious trouble, and one will realize that an enormous amount of experimenting has had to be done in order to obtain reasonably good speech.

I have not time to describe all the amplifiers, but some of them will be seen in working order, which I think will really be of more interest. Amplifiers are very much the same thing all the world over. I give the French credit for having produced the valve which has been of most use during the war, and also for having produced some standard types of amplifier, although the amplifiers now (1919) used in aircraft are very different indeed from those we received from France and which weighed five or six times as much as those at present in use. [The methods of fitting wireless gear in aircraft were then illustrated and described by the lecturer.]

Type 15 is a rather interesting type of transmitter which has just been completed, and is being fitted on the craft which are trying the transatlantic passage. It has two large valves, producing continuous waves, and is driven by one of these $\frac{1}{2}$-kW direct-current machines at about 1 500 or 2 000 volts, and ought to have a range to a ground station of something like 1 000 miles—we are hoping for it anyway.*

We have an interesting photograph of a B.E. 2C machine fitted with a wireless apparatus taken from another machine flying parallel, in which one sees the trailing wire. It may be of interest to mention that our mathematical expert made a lot of very elaborate calculations to find the curve in which the aerial wire would hang with different weights, different diameters of wire, and at different speeds. The curve in the photograph comes within about 10 per cent of the one which he calculated for it.

Now I want to show some amplification. I felt it would be unkind not, at least, to try to let those present hear the signals themselves, so I have had a really powerful amplifier constructed. Instead of using an ordinary amplifier with about 60 volts on the anode circuits, we use, to give loud speech, about 1 200 volts on a large valve which is really designed for use in a transmitter. We receive in the ordinary way on an ordinary receiver such as the three-valve T.f receiver, of which particulars will be seen on the sheet. The first valve in it rectifies the high-frequency current and the others amplify. The current from the T.f receiver is then amplified by the high-voltage amplifier and actuates a loud-speaking telephone. [An experimental illustration was given by the lecturer.] Though it rather resembles the gramophone in quality of tone it has quite a useful amplification, more than one might imagine from what we have heard. When we get signals on it we shall see that it is a very large amplification.†

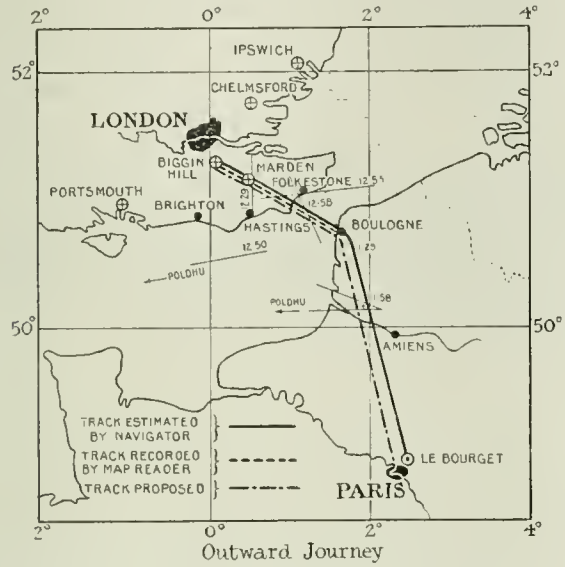
I want now to describe a totally different subject concerned with navigation, with the manner in which an airman can find his way through fog or cloudy weather without seeing the ground, if he is provided with the proper wireless apparatus. This particular apparatus, suitable for aircraft, was developed in the Air Force, and is somewhat different from anything that has been done before. A Handley Page machine started from Biggin Hill one day in October 1918 with a number of officers on board, and with the actual apparatus we now see in front of us fixed in the after part of the fuselage. The course shown on the chart (Fig. 2) was taken, the intention being to fly to Paris. There was a low mist driving in from the North Sea, and we, on the ground, lost sight of them in 3 minutes. The navigator gave the course. The first point they were to pass over was Marden, and they saw that all right. Near the coast they got "fixes" on the wireless stations at Poldhu and Chelmsford by observing their bearings and drawing lines through them on the chart, the point of intersection being the position of the aeroplane. The observer sitting in the front of the aeroplane thought he could see Boulogne, but was not quite sure, and after

* Greater ranges than this were obtained with this set on R34's voyage to America.

† Paris signals received in the lecture room were audible on the front-door steps of the Institution of Civil Engineers through two closed doors, a stair and a vestibule.

that he saw nothing but cloud until the plane descended at Paris on the instructions of the wireless navigator. They got down within about five minutes of the time stated by the navigator, and just over the aerodrome close to Paris where they intended to go. This invention opens up a new possibility with regard to aircraft. No longer is it a case of waiting for "weather suitable for cross-country flying." The only thing that is going to stop planes is real fog on the ground, or a hurricane.

The return journey (Fig. 3) was equally successful. Starting from Le Bourget the navigator was told to make a course for Boulogne. He did so, and on the way got bearings on various wireless stations; then he was told to alter course to Dungeness and the directions he gave to the pilot brought them to Dungeness at the hour he prophesied. Then he was told to go to Brighton, and at the moment he had calculated he should be there he told the General on board to look down, and Brighton



Outward Journey

FIG. 2.—Flight to Paris (20 October, 1918), navigated wholly by R.A.F. wireless direction finder.

was underneath. That was quite successful, and I will describe how it was done.

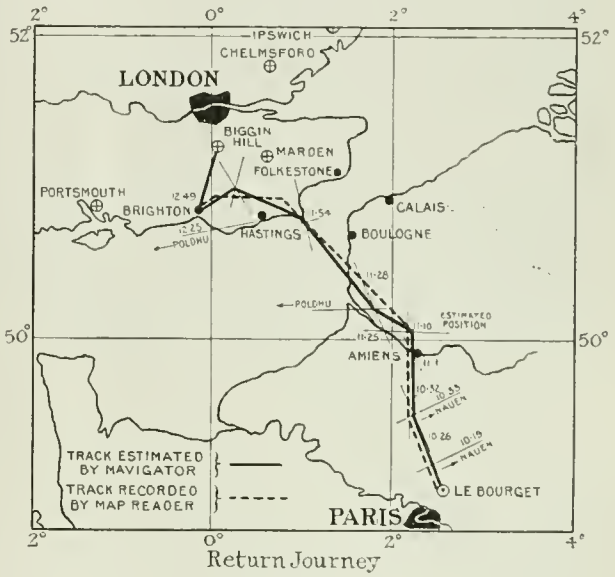
Here I have the actual gear that was used in this machine, only I am going to connect it up to the loud-speaking amplifier so that we can hear how we do it. First of all notice that these two coils are at right angles, and mounted on pivots so that they turn together about a vertical axis. At the corner of the hall there is a little buzzer on a pedestal, which we will regard as a sending station. If I turn a coil with its edge to the waves, as they come in, I get a maximum induction through the coil, and therefore I get a maximum signal from any apparatus connected with it. The operator at the station sends a signal, and I turn this round until we reach the point where a loud signal is heard.

What we do with this apparatus is first to listen on the one coil and find the general direction, that is to say, we get it into a position which gives a maximum signal, by just turning the coil. Then we switch in the second coil and reverse its connections several times.

Supposing this auxiliary coil is dead flat on to the waves as they come in, we shall get no energy from it at all, but only have the same signals we had before from the first coil, and there will be no change in signal strength on reversal. But if the main coil is not pointing straight towards the direction of the waves, there will be a change in signal strength when we reverse the auxiliary coil connections, since it is no longer in the position in which no energy is picked up. Thus the signals are now different in strength in the two positions of the reversing switch.

By these means we can determine in an aeroplane the direction of a sending station very easily to within a couple of degrees, which is quite good enough for navigation—in fact at 200 miles' distance this means that we can get our position within 5 miles.

There are many interesting problems as regards atmospheric variations in this direction-finding, but,



Return Journey

FIG. 3.—Flight from Paris, navigated wholly by R.A.F. wireless direction finder.

except for long-wave stations at great distances, these variations do not amount to much. So far with these small coils we have not observed more than two or three degrees of temporary variation. We can get permanent errors in direction by putting up a screen or mass of conducting material close by, but that is quite a different thing from a temporary variation.

This has great advantages over the minimum method. First of all, we have double the difference of sound, and, secondly, it is the difference of two easily audible sounds, and therefore a difference which we can easily distinguish. We can easily distinguish the equality of moderately loud sounds, but it is difficult to distinguish the equality of faint sounds, and very difficult to distinguish a blank spot where we hear nothing. I may say that the Air Force has given the advantage of this work, and of many other things, to the civilian companies who have wished to have the apparatus for the purpose of the transatlantic flight. On smaller machines we cannot fit a turning coil of sufficient size;

hence on a small machine we fit corresponding coils on the wings between the planes and then turn the machine instead of the coil; it is not such an easy operation, but still of considerable value. With the assistance of the Air Force one or other of these types has been fitted on several of the aircraft which intend to fly the Atlantic, and I believe also on the American seaplanes the same system is fitted, as the Air Force gave permission to use this system to the American officers who came over some time ago. By this means they will be able to determine their position, and, if they wish to fly towards any given station which is sending information, they can do so.*

Now there are other uses to which this apparatus can be put. Obviously it may be used on ships. About 10 days ago three ships were stranded in one afternoon near Cardiff, but if apparatus like this had been on the ships they would have been able to locate the direction of quite a number of wireless stations which are sending very frequently, or if the apparatus had been on the shore, say at Lloyd's signal stations, or the coastguard stations, they could have told the ships the direction in which they were, and accurately fixed their position, and the stranding probably would not have occurred. There is a good deal to be done in that way. I do not say that this is the only way in which we can deal with ships, but it is a perfectly simple and easy way and gives us in small compass the whole thing that we want—the amplifier, the switch-box arrangement, and the two coils, quite sufficient for determining the bearing of an ordinary wireless station several hundred miles away.

I am hoping to get through to some aircraft if the weather is not too bad, and the radiator does not leak, or the engines do not fail, or other things do not happen which occur occasionally even in the best-regulated aerodromes, and I hope to let those present hear what they are doing.

Now one of our aeroplanes has come over from Biggin Hill and is going to talk to us. [Messages were then sent to and from the hall and an aeroplane, the observer officer in which stated that he was at a height of 2 900 feet and three or four miles away. A long conversation took place which was audible to the entire audience.]

The President (Mr. C. H. Wordingham): I think it would only be right that we should pass a vote of thanks to our friend who is in the air.

Major Erskine-Murray: I will call him up, and tell him that the audience wishes to give him a vote of thanks and I will tell him to listen to the clapping. We will make as much noise as we can.

[The vote of thanks was carried with tremendous acclamation, and a reply came back from the aeroplane saying that the applause with which the proposition had been carried had been heard, and the pilot and observer thanked the Institution for the vote.]

Major Erskine-Murray: From what those present have seen and heard I hope they will agree with me when I say that the Wireless Section of the Air Force has justified its existence.

* I have been informed that the success of the U.S.A. seaplane NC4, the only one which completed the transatlantic crossing, was largely due to the bearings obtained by the R.A.F. direction finder which was carried.

ELECTRIC SUPPLY: PRESENT CONDITIONS AND THE HOPKINSON PRINCIPLES.

By J. R. Blaikie, Member.

(*Paper first received* 4 *September, and in final form* 7 *December,* 1920; *read before* The Institution, 28 *April,* 1921.)

Summary.

A proposal is put forward to divide consumers into two classes, viz. those requiring a supply for 7 days a week and those requiring a supply for 6 days a week; and a method is shown for obtaining a separate fixed charge, on the Hopkinson principle, for each group.

The advantages of dividing consumers as suggested are then considered, and an improved method of ascertaining the value of a coal clause is given.

Proposals are put forward to add a fixed-charges clause, varying automatically with the price of coal, and to record the percentage of output to plant kilowatt-hours run, for statistical purposes.

Synopsis.

The paper draws attention to the changes that have come about since the late Dr. John Hopkinson devised the well-known system of dividing the costs of electric supply under two headings, viz. fixed and running charges. At that time electricity was used almost entirely for lighting, and he showed how it might be sold at low rates to compete with gas and still make a profit.

The fixed charge consisted of three items, interest and redemption of capital, certain wages, rents, rates, etc., and a proportion of coal.

It was assumed that all consumers required the undertaking to be instantly ready to meet their demands, i.e. plant actually running awaiting only the closing of the consumer's switch.

As far as capital charges are concerned this assumption is true. With regard to coal and certain wages, some consumers who only use current at definite times must cost less than others who use current at any time during the day or night. But at the time Dr. Hopkinson was considering these costs, and for many years later, the capital charges were more than 50 per cent of the total fixed costs, and it was not worth while to draw distinctions between classes of consumers with regard to the remaining portion of the fixed charge. When large factory loads developed, important differences arose between this class of consumer and lighting consumers. It is found, for example, that the cost per kilowatt demanded for capital charges for factory loads may be half the capital charge per kilowatt for lighting, partly owing to mains and services, and partly to the relative sizes of the demand. The factory demands are large enough to require very large units of generating plant, having higher efficiency and costing less per kilowatt of rating.

Another important difference is a very marked reduction, per kilowatt demanded, in coal and wages due to the fact that the factory runs for a limited number of hours a week at definite times. The reduction in coal per unit is much greater than can be accounted for by a change in load factor (from, say, 12 to 20 per cent) on the whole output.

Since the war, the rise in the price of coal and rates of wages has caused these components of the fixed charge to predominate. The large amount of capital still active, obtained at pre-war rates, tends to keep the total capital charges per kilowatt fairly steady and only rising slowly with further extensions. At the present time the whole capital charges may be from 25 to 30 per cent of the whole fixed charge, instead of over 50 per cent.

The introduction of the metal-filament lamp has entirely altered the case of competition with gas for lighting; on the other hand, the large development of electric power in individual factories has made it possible for such concerns to consider the question of installing their own generating plant, instead of taking a supply from the mains.

On these points it appears that the time is ripe to draw distinctions between consumers in, say, two groups, principally in regard to the coal and wages portion of the fixed charge.

The paper gives a practical method of making this distinction, and introduces evidence in support of the proposals very much on the lines of Dr. Hopkinson's original conclusions. It amounts to finding a separate fixed charge for each of the two groups, after which each group can be treated on the usual load-factor scale.

By means of this, or a similar method, it is possible to construct a revenue account of the whole undertaking, showing the total costs and receipts for each class of service, which is of the greatest possible service in adjusting tariffs. Examples of this are given. As far as the author is aware it has not been possible to deal with the whole undertaking in this manner, since engineers have been obliged to quote special rates to each large factory.

This form of analysis is also of use in devising a suitable coal clause.

A further clause is proposed dealing with other rises in the fixed charges.

Introduction.

The problem discussed in Dr. Hopkinson's classic paper * was, how to supply electricity to carbon lamps, at a price to compete with gas. The concluding sen-

* Presidential Address delivered 4th November, 1892, before the Junior Institution of Engineers.

tences were: "It is hopeless to compete with gas in this country all along the line if price is the only consideration. But with selected consumers electricity is cheaper than gas. Surely it is to the interest of those who supply electricity to secure such consumers by charging them a rate having some sort of relation to the cost of supplying them."

It is important to notice the scope and limitations of the problem as thus set forth. There are certain assumptions which have been proved to be thoroughly justified so long as the bulk of the output was for the purpose of lighting, but which do not appear to be applicable, as they stand, to the large undertakings that have since grown up, where lighting constitutes only 10 or 20 per cent of the output, for example:

(1) That the central supply is always ready to supply the maximum demand, or that all consumers demand an equal degree, per kilowatt demanded, of readiness to supply, which means machines actually running.
(2) That all consumers cost the same in wages per kilowatt of demand.
(3) That all consumers cost the same in capital charges per kilowatt of actual demand.

If we divide consumers broadly into two groups, viz. those who require a supply for 7 days a week, and those who require a supply for 6 days a week, we find:

In the 7-day group: public lighting, domestic lighting, some heating, a small amount of power and, in certain cases, traction supply.
In the 6-day group: practically all factory power and lighting supply, shop lighting and some heating.

If we assume that the heating demand in the 7-day group is balanced by the shop lighting in the 6-day group and exchanged, the percentage error in the whole group is probably quite trifling in the majority of cases, and the problem is much simplified.

With reference to the Hopkinson assumptions it will be admitted that:—

(1) In the matter of coal consumption there must be a very considerable difference between the two groups, inasmuch as the 7-day group not only demands service for more days a week, but also for more hours per day. The factory group is usually limited to, say, 60 hours a week, at definite times.
(2) The wages of the 7-day group are much in excess of those for the 6-day group, first on account of the extra day per week, and secondly on account of the unavoidable excess per kilowatt due to the character of the load. Men cannot be engaged for two hours a day or otherwise occupied in proportion to the load at the moment.
(3) Capital charges are much less per kilowatt in the 6-day group, for many reasons. The expenditure in mains and services is undoubtedly less, as in the majority of cases low-tension distributors are entirely eliminated, substation space and sometimes buildings are provided by the consumer, and there is an opportunity at the generating station of installing large sets at a lower initial cost per kilowatt.

With these points in mind we can now consider the need of reviewing the whole situation and the possibility of measuring the various differences indicated, with the object of arriving at a satisfactory system of analysis and finally framing tariffs to suit.

Since the price of coal has risen so enormously and rates of wages, etc., have increased, we find a remarkable change in the relative values of the components of the fixed charge.

The following is fairly typical and may be compared with the original Hopkinson hypothetical station.

Fixed Charges.

	Hopkinson Hypothetical Station	Actual Case, 1914 to 1915	Actual Case, 1919 to 1920
	Per cent	Per cent	Per cent
Coal	21	23	32
Other charges ..	22	37	42
Capital	57	40	26
Total	100	100	100

So long as the capital charges were the dominating component of the fixed charge, the assumption that all consumers called for an equal readiness on the part of the undertakers to supply did not make an important difference. Now that coal and wages predominate, the differences between classes of consumers with reference to readiness to supply becomes very important, and it is this feature that we have to watch carefully in trying to hold our own against the possibility of private plants in large factories. The fear of gas competition in lighting disappeared with the introduction of the metal-filament lamp. Competition with private plant is the problem to-day.

If the central supply does not take full credit for the saving due to limited hours of demand, the margin of profit will become very narrow indeed at competitive rates. In coal consumption, the superior running economy of the central supply suffers a discount in mains and transformer losses. In other charges a factory can often claim as good figures as the central supply, particularly where a fair number of engineering hands are employed.

In capital charges it is doubtful if the factory cannot do as well, and it must not be forgotten that the central supply is always handicapped with the cost of mains and transformers.

Taking all these points into account I suggest that the factory load is a serious problem and justifies careful consideration in order that the supply department may feel sure of its ground in offering sufficiently low rates to consolidate the business, and work profitably at the same time.

To go a step further there are, I believe, many engineers who find it very difficult to decide how the inevitable increases are to be applied. There is a general feeling that they should be applied to the lighting, one of the most effective reasons being that power contracts are very difficult to alter.

On the other hand, municipal bodies are apt to favour the taxation of the power consumer.

In the light of past history it is probably not far from the truth to say that engineers have discovered that by analysing the whole station costs on the usual load-factor basis, i.e. the percentage of output to the possible output if the maximum demand were maintained for every hour of the year, they obtain figures for the price of current which are not likely to secure the business.

It becomes necessary to reduce this figure somehow and, chiefly supported by the prospect of larger and more efficient plant, the engineer persuades himself to take the risk and quote lower rates than the existing figures justify. He may overdo it, but in the majority of cases he is agreeably surprised to find material progress in the next balance sheet. After that, encouraged by this result, he continues the process until at last a stage arrives when the cost of supply to individual consumers becomes more and more vague and there is no general principle on which to adjust the charges.

The engineer has in fact discovered that he can depart to some extent from the Hopkinson principles as laid down for the supply of electricity to carbon lamps, but he has lost the precise measure and gauge that in earlier days made him feel sure that, though he was losing money in certain cases owing to statutory limitations in price, he was recouping himself handsomely in others with long-hour demands.

From conditions as they are to-day it is possible to retrace our steps and start again, following Dr. Hopkinson's methods, with new facts, new observations, and changed conditions. We have before us still the brilliant idea of grouping and selecting consumers, and dividing the costs between them in such a manner that we may safely quote low rates to certain classes.

Having stated the need and indicated the advantages of regarding consumers under the two headings of 6 and 7 days, the problem arises as to how the costs can be segregated.

Beginning with coal we can follow Dr. Hopkinson and take a hypothetical station running under average conditions.

Let us assume that the plant consists of four sets of turbo-generators each of 1 000 kW, three being in service at a time to meet demands as follows :—

For lighting	800 kW
For power	1 600 kW
Total	2 400 kW

	Output per Annum
Let the lighting load factor be 12½ %	876 000 units
Let the power load factor be 23·7 %	3 324 000 units
Total	4 200 000 units

Then the whole station load factor will be 20 per cent.

Let the total observed coal consumption be 6 lb. per unit sold, and the running coal 2·5 lb. per unit sold.

Then the total coal consumed will be 11 200 tons, which we will now apportion to the two supplies :

	Power	Lighting	Total
	Tons	Tons	Tons
Running coal units × 2·5 lb.	3 700	980	4 680
Fixed coal; kW × 2·72 tons per annum ..	4 350	2 170	6 520
Total	8 050	3 150	11 200
Lb. per unit sold ..	5·43	8·05	6

We will now consider the probable working of the plant.

The total capacity of the system in kilowatt-hours will be :—

	Plant kWh Run
One 1 000 kW for 8 760 hours	8 760 000
One 1 000 kW for 51 weeks of 60 hours	3 060 000
One 1 000 kW for 25 weeks of 6 hours	150 000
Total	11 970 000

For running this number of plant kilowatt-hours without generating any current we have already allocated 6 520 tons as fixed coal, or 1·22 lb. per plant kilowatt-hour run.

For comparative purposes between one station and another or between one year and another of the same plant, it is the percentage of units output to the plant kilowatt-hours run that is needed. In this case it is about 35 per cent, though the ordinary load-factor scale shows 20 per cent. It is interesting to note that Mr. J. S. Highfield* recently said : "The figures of average costs and average price received are most misleading when treated in any way as a figure of merit for comparing undertakings. Unless all the circumstances—the amount of load, proportion of lighting and power loads, the number of consumers, load factor, and the price of coal—are taken into account, the figures indicate very little."

To continue with our hypothetical station.

If power alone were required two sets would run 51 weeks of 60 hours. Lighting alone would require one set for 8 760 hours.

	Plant kWh Run
Two 1 000 kW for 51 weeks at 60 hours	6 120 000
One 1 000 kW for 8 760 hours	8 760 000
Total	14 880 000

But by the co-operation of the two supplies we have seen that only 11 970 000 plant kilowatt-hours run are

* *Electrical Times*, 1920, vol. 57, p. 521.

necessary, or about 19·6 per cent less. We can therefore reduce each of the above items by 19·6 per cent; the figures will then be as follows:—

6-day group (power)	..	4 920 000 kWh
7-day group (lighting)	..	7 050 000 kWh
		11 970 000 kWh

	Power	Lighting	Total
	Tons	Tons	Tons
Running cost: Units × 2·5 lb. ..	3 700	980	4 680
Plant kWh × 1·22 lb. ..	2 680	3 840	6 520
Total	6 380	4 820	11 200
Lb. per unit sold ..	4·3	12·3	6

If the station lost all its power load and a single factory absorbed the power plant, the two concerns would run as follows:—

Lighting Station	Tons
Running costs; Units × 2·5 lb.	980
8 760 000 plant kWh × 1·22 lb.	4 800
Total	5 780
Lb. per unit	14·5

Factory	Tons
Running cost; Units × 2·5 lb.	3 700
6 120 000 plant kWh run × 1·22 lb. ..	3 340
Total	7 040
Lb. per unit	4·75

In the probable working of the hypothetical station the percentage of output to plant kilowatt-hours run in the two groups are:—

	Usual Load Factor
Lighting 12½ per cent	12½ per cent
Power 67½ per cent	23·7 per cent

It follows therefore in the 6-day group that beyond a load factor of about 35 per cent on the usual scale we cannot expect a further reduction in coal consumption for power, as at this load factor the plant would be running at 100 per cent activity on the plant kilowatt-hour scale.

Any further increase in output could only be obtained by increasing the number of plant kilowatt-hours. The coal per unit could only be decreased beyond this point by employing more efficient plant.

In the present case it will be observed that the plant is not loaded to its full capacity. If it were loaded further, less coal per unit would be obtained. This condition will obviously vary from year to year, so that one must either find a further factor for the ratio of load to the capacity of the plant, or strike an average by reckoning with the results of several years.

Comparing the results so far obtained we find:—

	Usual Load-factor Scale		Plant kWh Scale	
	Per cent	Lb. of Coal per Unit	Per cent	Lb. of Coal per Unit
Lighting or 7-day group	12½	8·06	12½	12·3
Power or 6-day group	23·7	5·43	67½	4·3
Ratio of coal consumption of 6-day and 7-day groups		10 to 14·7		10 to 28·6

In these figures there are very substantial differences.

Following the Hopkinson methods we now seek actual figures to substantiate either one set or the other. As far as lighting is concerned, unfortunately both figures, viz. 8 and 12, are well within the range of practical experience. But probably the lower figure applies to direct-current and the higher to alternating-current systems. The actual figures are not of much importance, but which ratio is the more nearly correct?

In Bristol during the years 1900 to 1902 the percentage of power to the total connections varied from 3 to 13 per cent, so that it was practically a lighting system. The load factors were respectively 12·35, 11·92 and 12·41 per cent, and the coal per unit sold 12·37, 12·57 and 12·88 lb. respectively. This confirms our figure of 12·3 lb.

During the next six years the load factors varied between 13·7 and 14·7 per cent, but the percentage of power connections rose from 17½ to 43 per cent. The amount of coal during this period dropped fairly consistently from 10·06 lb. to 7·7 lb. per unit sold. Judging from the total output in the years 1902 and 1908, to estimate the power units, it appears that the power would take from 4½ to 5 lb. in order to bring about an average of 7·7 lb. on the total output. As

the power load factor during this year could not have been more than about 17 per cent, our figure of 4·3 lb. for 23·7 per cent cannot be far wrong. In the published returns there is no information as to new plant installed, but with the low load factor improved plant would have a very small effect.

Further confirmation can be obtained from present-day records by means of simultaneous equations over two or more years. It is commonly observed that the load factor of any one class of consumer remains wonderfully constant year by year, and that the only changes that occur in the whole station load factor are due to the growth of some particular class of consumer. If the load factor remains constant the total coal (i.e. fixed and running) per unit sold will also remain constant and therefore equations can be formed. If x equals the quantity of coal per unit for the 7-day group, and y the quantity per unit for the 6-day group, then x (units of 7-day group) + y (units of 6-day group) = total coal used. With two years' records the equations can be solved and values found for x and y.

It may happen that the assumption of constant load factor is not correct in certain cases, or that the introduction of new plant creates disturbances. From one cause or another we may get absurd values from such equations, but if after obtaining what appear to be reasonable figures it is found possible to reconstruct the coal bill correctly, to within a small percentage, for, say, four consecutive years, there is a strong presumption that the figures are correct and may be turned to useful account.

Through the courtesy of the several engineers concerned, I have been able to examine and experiment with the figures of several undertakings, and the results are as follows :—

Bedford	.. ..	10 lb. to 28·6 lb. per unit sold
Derby	.. ..	10 lb. to 25·0 lb. per unit sold
Burton	.. ..	10 lb. to 27·0 lb. per unit sold
West Ham	.. ..	10 lb. to 40·7 lb. per unit sold

In all four cases, to which perhaps Bristol might be added, the ratios are more in accord with the ratio of 10 to 28·6 than 10 to 14·7, thus supporting the plant kilowatt-hour scale of the hypothetical case.

Derby, Burton and West Ham all contain tramway supplies. As a further check, observations of the average Sunday performance will indicate the coal consumption of the 7-day group. As a weekday contains the 7-day group the consumption will be higher than in the 6-day group. If the equivalent 7-day group units and their coal are deducted from the weekday records the remainder will indicate the consumption of the 6-day group.

In Bedford 12 consecutive Sundays commencing in February showed an average of 12·27 lb. per unit generated, or about 14·71 lb. per unit sold. The weekday average for the same dates was 4·78 lb. per unit generated, or 5·76 lb. per unit sold, i.e. a ratio of 10 to 25·6.

The coal per unit for the two groups obtained on these lines contains both the fixed and the running coal. In practice it seldom happens that all the generating sets have the same characteristics; some have a higher running consumption than others. As a rule, probably the best machines are run on the 6-day group and only supply a small portion of the 7-day group, but it may be fairer to reckon with the average running consumption of the whole output. This procedure will show the running coal a little on the high side for the 6-day group and a little on the low side for the 7-day group.

	Year Ending	Coal per Unit		Calculated Coal	Actual Coal	Error
		6-day	7-day			
		lb.	lb.	tons	tons	per cent
Bedford	1915	4·73	13·5	11 165	11 429	− 2·3
	1916	4·73	13·5	10 894	10 634	+ 2·4
	1917	4·73	13·5	11 820	11 565	+ 2·2
	1918	4·73	13·5	15 330	15 650	− 2·8
Derby	1915	3·96	9·93	22 610	22 742	− 0·6
	1916	3·96	9·93	22 540	23 862	+ 2·9
	1917	3·96	9·93	28 530	28 155	+ 1·3
	1918	3·96	9·93	37 080	37 500	− 1·1
Burton-on-Trent	1915	4·66	12·65	9 670	9 764	− 1·0
	1916	4·66	12·65	10 690	10 545	+ 1·4
	1917	4·66	12·65	11 360	11 225	+ 1·0
	1918	4·66	12·65	12 340	12 442	− 1·0
West Ham	1916	2·83	11·5	77 400	75 535	+ 2·4
	1917	2·83	11·5	75 900	75 400	+ 0·72
	1918	2·83	11·5	76 900	76 939	—
	1919	2·83	11·5	79 700	79 606	+ 0·13
	1920	2·83	11·5	84 600	83 834	+ 0·9

The ratio of the coal consumed by the 6-day group to the 7-day group in the four cases proves to be :—

The running coal is found by the usual method of selecting two periods of unequal production but having

about the same capacity of running plant. The difference in units and coal shows that a certain number of units have been produced from a certain weight of coal. From this the rate per unit is obtained. The running coal for each group can thus be calculated and deducted from the total coal of the group, leaving the fixed coal.

The sum of the fixed coal, other charges and the capital charges, may be divided by the kilowatt demand of the group, expressed as so much per kilowatt-year and applied to the usual load-factor scale.

Running Charges.

It was suggested by Dr. Hopkinson that the running charge should contain, besides the running coal, certain proportions of wages and stores. Mr. Arthur Wright and others have shown this method to be very satisfactory for a number of years but, incidentally, always on a rising load factor.

If for some reason, such as "Summer Time" or shorter factory hours, the load factor drops, it is difficult to believe that there will be any reduction in expenditure except on account of coal. It is suggested, therefore, that the safest policy is to limit the running charge to coal only.

Fixed Charges.

A method of finding the fixed coal has already been described. The two other components of the fixed charges, viz. capital charges and all other charges, can be divided and allocated to the two groups by simultaneous equations, as in the case of the coal.

In these cases, of course, "kilowatts demanded" are substituted for "units," and values are found for each group per kilowatt demanded.

With capital charges it is no doubt advisable to take the average of at least four years, and test the results by reconstructing with the same values for the four years, as was shown in the case of coal.

Since 1915, wages and many other items have increased out of all proportion to output, and it is therefore suggested that a ratio between the two groups should be found from the average of the four years preceding 1915. This ratio can be applied throughout the disturbed years.

As a check, another form of analysis can be tried. Each item of "other charges" can be divided between the two groups on broad lines, for example:—

Wages.—In a moderate-sized station each shift employs a certain number of men as a minimum. With the use of large sets and mechanical devices for stoking, ash handling, etc., the extra wages required for a heavy shift is very small. Running a shift at all is the chief expense.

The 6-day group would require roughly 6 shifts a week, and the 7-day group 21 shifts per week. Incidentally, 3 shifts of the 7-day group may run at overtime rates. Making allowances for heavy shifts and co-operation between the two groups, perhaps a division of the cost in a ratio of 1 to 2 would not be unreasonable.

The wages are therefore allocated, one-third to the 6-day group and two-thirds to the 7-day group.

The oil used will be more in proportion to the hours run than to the demand. It may therefore be treated in the same way as the wages. Repairs may be regarded as insurance and divided in proportion to the capital charges of each group.

Assuming that the capital is supposed to earn a standard rate, rents, rates and taxes should also be allocated *pro rata* with the capital charges of each group. The same will apply to the salaries of the chief engineer and the first assistant.

The costs of distribution and the remainder of the office staff salaries may be divided in proportion to the number of consumers in each group.

No doubt there will be differences of opinion on this method, but I think it will be found that considerable modifications can be made without making a substantial difference in the final result.

In the Bedford case an analysis on these lines gives a cost of £1·75 for the 6-day group and £7 for the 7-day group, per kilowatt-year. The equation method for the four pre-war years ending March 1914 gives £1·34 and £7 respectively. These figures show remarkable closeness in reconstruction, i.e. the kilowatts of the 6-day group multiplied by £1·34, plus the kilowatts of the 7-day group multiplied by £7.

Year Ending	Calculated	Actual	Error	Whole Station	
				Cost per kW-year	Change from Preceding Year
	£	£	Per cent	£	Per cent
1911	4 945	4 962	— 0·34	4·30	—
1912	5 565	5 548	+ 0·31	3·75	— 12·75
1913	5 948	5 994	— 0·75	3·85	+ 2·67
1914	6 350	6 324	+ 0·41	3·55	— 8
1915	6 734	6 835	— 1·4	3·79	+ 6·8

DATA SHEET.

Coal.

	1914–15		1915–16	
	Total Cost, £7 347		Total Cost, £9 660	
	Tons, 11 429		Tons, 10 634	
	7-day group	6-day group	7-day group	6-day group
Units sold	1 194 226	1 871 456	973 936	2 383 756
Maximum kW demanded	762	1 038	717	1 033
Tons of coal running at 3·66 lb. per unit	1 951	3 057	1 591	3 895
Total value of coal divided in the ratio 13·5 : 4·73 per unit	£4 747	£2 600	£5 201	£4 459
Less value of running coal	£1 254	£1 966	£1 445	£3 538
Value of standing coal	£3 493	£634	£3 756	£921
Running cost per unit	0·252d.	0·252d.	0·356d.	0·356d.

Other Charges.

	Total Cost, £6 836		Total Cost, £8 622	
Value divided in the ratio 7 : 1·34 per kW	£5 424	£1 412	£6 760	£1 862

Capital.

	Total Cost, £7 602		Total Cost, £7 898	
Value divided in the ratio 6·17 : 3·07 per kW	£4 530	£3 072	£4 599	£3 299

Total Standing Charges.

Coal	£3 493	£634	£3 756	£921
Other charges	£5 424	£1 412	£6 760	£1 862
Capital	£4 530	£3 072	£4 599	£3 299
Total	£13 447	£5 118	£15 115	£6 082
Standing cost per kW demanded	£17·64	£4·932	£21·08	£5·888
Maximum demand—Private lighting	615 kW	—	630 kW	—
Maximum demand—Public lighting	95 kW	—	38 kW	—
Maximum demand—Sewage pumping	52 kW	—	49 kW	—
Maximum demand—Power and heating	—	1 038 kW	—	1 033 kW
Units—Private lighting	670 016	—	630 774	—
Units—Public lighting	276 794	—	109 821	—
Units—Sewage pumping	247 416	—	233 041	—
Units—Power and heating	—	1 871 456	—	2 383 765

These figures are given in detail to contrast with the cost per kilowatt-year on the whole supply, which shows a somewhat irregular variation of from 2 to 12 per cent from year to year. With the two groups there is an example of the wonderful uniformity from year to year that is frequently found in studies of this kind, and which not only inspires belief in the results but offers every encouragement to experiment further and search out new factors governing the cost of production.

We are now in a position to apply these methods to the ordinary accounts. The following further data are required, viz.:

(1) The tons of coal used.
(2) The capital charges on public lamp-posts, lamps, wiring service connections, and special mains, if any.
(3) An estimate, subdividing the maximum load of the year amongst the large classes of supply.

REVENUE SHEET FOR 1914–15.

Expenditure.	£	£
7-day group.		
Private lighting—		
615 kW at £17·64	10 842	
670 016 units at 0·252d.	699	
		11 541
Public lighting—		
95 kW at £17·64	1 676	
276 794 units at 0·252d.	291	
Capital charge on lamp-posts, etc.	2 013	
Attendance, new lamps, etc.	668	
		4 648
Sewage pumping—		
52 kW at £17·64	917	
247 416 units at 0·252d.	260	
		1 177
6-day group.		
Power and heating—		
1 038 kW at £4·932	5 117	
1 871 456 units at 0·252d.	1 965	
		7 082
		24 448
Less error to balance		17
		24 465
To Reserve Fund		1 610
To General District Fund		2 000
		£28 075

Income.	£	£
7-day group.		
Private lighting—		
By sale of current at 5d. per kWh	13 294	
By sale of current (slot meters)	514	
By sale of current at 6d. and 4d.	754	
Proportion of meter rents (369 : 2 389)	855	
Embankment illuminations, etc., *less* stores and labour	269	
	15 686	
Less bad debts	29	
		15 657
Public lighting		4 306
Sewage pumping		1 031
6-day group.		
Power and heating—		
By sale of current at 1d. per kWh	6 697	
By sale of current at 2d. per kWh	325	
Proportion of meter rents	59	
		7 081
		£28 075

From this information it is convenient to prepare a data sheet as shown.

From the data sheet and the accounts a revenue account on the whole supply can be constructed.

If such an account on the whole supply can be produced with a reasonable degree of accuracy, I submit that the attempt is well worth careful consideration.

It shows at a glance the position of various important classes of consumers, and what effect anticipated changes are likely to produce.

In the majority of large undertakings special contracts have been arranged on flat rates, estimated on the probable load factor and the general costs prevailing at the time. It is very difficult to say how such cases stand to-day, whether a high coal clause has saved the situation, or whether it is possible to amend the terms on the same lines that will hold good for a few years ahead. It would certainly be very much easier if all these special cases could be reduced to one system, showing the undertaking working at a profit and the other at a loss.

From these sheets the extent of profit or loss in the large sections is apparent.

Analysing the cost of the various services obtained in this manner we find, in 1915, as follows:—

Private lighting	4·15d.
Public lighting	1·91d.
Power and heating ..	0·907d.

with coal at 12s. 10d. and an overall load factor of 19·4 per cent.

REVENUE SHEET FOR 1915–16.

Expenditure.			Income.		
7-day group.			7-day group.		
Private lighting—	£	£	*Private lighting*—	£	£
630 kW at £21·08	13 277		By sale of current at 5d. per kWh	11 939	
630 774 units at 0·356d.	935		By sale of current (slot meters) ..	553	
		14 212	By sale of current at 6d. and 4d. ..	788	
			Proportion of meter rents (441 : 2 479)	820	
			Market lighting (*less* stores, etc.) ..	181	
				14 281	
			Less bad debts	34	
					14 247
Public lighting—			*Public lighting*		2 871
38 kW at £21·08	801				
109 821 units at 0·356d.	163				
Capital charge on lamp-posts, etc. ..	1 923				
Attendance, new lamps, etc. ..	326				
		3 213			
Sewage pumping—			*Sewage pumping*		968
49 kW at £21·08	1 033				
233 041 units at 0·356d.	345				
		1 378			
6-day group.			6-day group.		
Power and heating—			*Power and heating*—		
1 033 kW at £5·888	6 082		Coal surcharge	459	
2 383 756 units at 0·356d.	3 536		Sale of current at 1d. per kWh ..	8 411	
		9 618	Sale of current at 2d. per kWh ..	309	
			Proportion of meter rents	147	
		28 421			9 326
Add error to balance		7			27 412
			From General District Fund ..		1 016
		£28 428			£28 428

on broad lines, and furnished with sliding scales to provide corrections for the serious changes that are now occurring every few months. The first step is to put the accounts into such shape that we can see the position in broad sections.

Two examples from actual figures are given, one

From the above figures it will be seen that the cost of private lighting is about 4½ times the cost of power, and the cost of public lighting about twice the cost of power, with low-priced coal. I submit that these ratios are in close agreement with general practice and, therefore, this form of analysis is

satisfactory, and should give reliable results with high-priced coal and increased wages.

If we apply the same process to the 1920 results, with coal at 32s. 9d. and other costs in proportion, and the overall load factor 17·9 per cent, the results are rather startling :—

Private lighting	.. ..	9·35d.
Public lighting	.. ..	3·72d.
Power and heating	..	1·73d.

Coal has risen in price by 155 per cent and is inferior in quality. Other charges have risen about 55 per cent and we have a reduction in load factor partly due to Summer Time and partly to shorter factory hours; so it is not very surprising if the costs should have risen about 100 per cent.

If the present-day rates are applied to early records, before the power load developed, much the same figures will be obtained.

It appears to be time to apply for double the statutory limit, or at least a broad reading of the regulation, substituting the word "average" for "maximum" and so give gas engineers a chance.

Tariffs.

The majority of tariffs employed in this country, and possibly in America as well, are based on Dr. Hopkinson's methods of viewing the costs of production, which disclose the all-important feature called load factor. A large number follow more or less directly his proposal of a compound charge, that is to say, a fixed charge per quarter or half year per kilowatt demanded, and a low rate per unit used. This method provides automatically a resultant charge which is directly proportional to the load factor.

In the view of some engineers and managers this method is open to two objections, viz.:

(1) The difficulty of ascertaining the demand, which might be increased by the consumer without giving due notice to the undertakers; and
(2) The direct tax on the kilowatt demand, which may tend to discourage extensions.

These objections have been largely overcome by Mr. Arthur Wright, formerly of Brighton, whose name cannot be mentioned in connection with tariffs without calling for a tribute to his good and effective work, not only in connection with his well-known variation of the Hopkinson proposal, but also for his energetic teaching of Hopkinson principles.

Mr. Wright devised, and put into commercial form, a maximum demand indicator which overcomes the first objection to the Hopkinson proposal, and also the method of using it which is associated with his name. By measuring the actual maximum demand instead of assuming the possible maximum demand (from the plant installed) the objection to extending the installation, from the consumer's point of view, was much reduced, as in many cases additional lamps could be connected without risk of increasing the maximum number in use at any given moment. In Mr. Wright's method the standing or fixed charges were covered by charging a high rate, usually the maximum allowed, viz. 8d., for a sufficient number of units to cover both the standing charge per kilowatt and the running costs of these units. All units in excess of this number were then charged at a low rate, say 1d. or 2d.

The result was a scale of charges proportional to the load factor.

The chief objections to this system are :—

(1) The additional cost of the demand indicator; and
(2) The irritation caused amongst consumers on occasions when the demand indicator appears to operate against them. If, for example, a man has been acccustomed to pay at an overall rate of, say, 4½d. and on some occasion an extra demand is made, he may find the quarter's bill charged at the rate of 6d. It may not amount to much, but it is a very marked difference and calls for explanation. It is not good business to refer the man to the terms of the agreement and let him go away feeling that he has been tricked. It is necessary to explain that the extra demand actually cost the company more to supply, and unless one has time and material properly booked on the correct day the explanation is rather a sorry business.

An incident such as this may seem to be very trifling, but a few discontented consumers may be quite a serious consideration.

With objections to both the Hopkinson and Wright tariffs, some tariffs are made by quoting flat rates based on load factor to classes of consumers who are assumed to have similar load factors, such as private residences (6d.) hotels and public-houses (4d.), etc.

Another variation is what Americans call a "block system," that is, a certain number of units called a "block" are charged at one rate, and successive "blocks" in the same quarter at lower rates. These rates are arranged to suit estimated steps in the load factor. The Wright maximum demand system is described in the American "Standard Handbook for Electrical Engineers" as a "two-charge block rate."

This publication refers to the Rate Research Committee of the National Electric Light Association and contains a very refreshing review of the subject, which has become rather tiresome at Conventions in this country. It commences with a paragraph entitled "Determining Factors," which are subsequently described as :

(1) The value-of-service theory, and
(2) The cost-of-service theory.

The first simply means getting as much as possible for the goods, having regard to the possibility of competition. Incidentally, it may be remarked that the word tariff is said to have originated as the name of the form of blackmail levied by the pirates of "Tarifa," who doubtless worked on the value-of-service theory and collected as much as circumstances permitted.

The second is a rate which includes a comfortable return on capital, and is found to be very palatable by public utility commissioners and the like, but, as

in the case of what the modern building contractor calls the " cost-plus system," difficulties occur when the question of cost arises. It is astonishing how much there is to learn about the cost of providing and laying one additional brick, and the cost of leaving one out. If there are such difficulties in a simple matter of bricks and mortar, one can well understand the irritation of the electricity consumer solemnly lectured on costs and fogged with technical terms.

Strangely enough the American book says : " The Hopkinson method of estimating rates is now generally recognized by all authorities as correct where rates are to be based on cost of service." No doubt it can be used in this way, but a careful reading of Dr. Hopkinson's paper clearly shows leanings towards the pirate's policy. It is true that he believed in charging consumers at a rate " having some sort of relation to the cost of supplying them," as a sound business principle, but he also says : " There is no object in reducing the cost of electricity in the case of any consumer much below the cost of the equivalent lighting by gas, unless there are competitors in the field willing to do it, hence the current charge proportional to the power supplied may be safely increased." His reply to objectors is also characteristic. He says : " The best answer that can be made is to give everyone the choice of being charged the maximum simple rate provided by the Order, or by the compound rate as they prefer."

The whole gist of his paper is the method of causing gas consumers to adopt electricity and benefit the undertaking, and the tariff was designed with this end in view.

An extreme case of the value-of-service theory is found in a scheme which is in successful operation in a few undertakings, known as the " Point Five " system. It is a compound charge which has no sort of relation to the cost of supply ; the fixed charge is based upon the rateable value of the property, and current is supplied at $\frac{1}{2}$d. per unit.

Having regard to the statutory limitations contained in the Order which safeguards the public and the interests of other monopolies in the supply of light, heat and power, there appears to be very little objection to the policy of selling to the best advantage, in conformity with general trading principles. Open competition compels the ultimate selling price to approach the cost of production.

For those who feel constrained to supply on the cost-of-service theory, as public policy, the following methods can be applied :

(1) The Hopkinson compound rate and variations.
(2) The Wright maximum demand system.
(3) The Doherty system (American) which is a three-charge rate similar to the Hopkinson, the third rate being a consumer's charge to cover book-keeping, meter reading and such charges that are common to every consumer, large or small.
(4) Various time-controlled two-rate meters.

None of these systems, however, defines what the cost really is. The Hopkinson method of analysis undoubtedly proves wide differences of cost between certain classes of consumer, but the cost of any class may vary between rather wide limits, perhaps as much as 100 per cent in the estimates of experts. In commercial practice it is usual to allow a variation of about $2\frac{1}{2}$ per cent in the determination of quantities, and if costs cannot be ascertained to within something approaching this degree of accuracy it is useless to talk about the cost-of-service theory.

To make this point clear, I suggest that any consumer who agreed to pay on the cost of production (including capital charges) could challenge any figures, and it would be a most difficult matter to prove what the cost was, to within $2\frac{1}{2}$ per cent.

Of tariffs in general it may be remarked that a most usual criticism is the tendency to promote or restrict a demand for current. This point of view opens up other considerations. If current can be supplied at rates that will provide cheaper light, heat or power than can be obtained from other sources, there is an advantage to the consumer and the nation. If it is cheap enough to encourage waste there is a national loss. At the present time we may be suffering from the effects of too cheap coal and too cheap labour. There may not have been sufficient incentive to economy or striving for higher efficiency.

Dr. Hopkinson's discovery that the costs of preparation were very high and that the costs of continuing to supply were very low, naturally suggests that the first charge should be high and the second low. This form of tariff may encourage waste, at the high rate in order to attain the low, and at the low rate because it is not worth the trouble of economizing.

Proposals to dispense with meters altogether are a first-class encouragement to waste, and are against national interests.

In the opinion of the author the value-of-service theory is the only one of practical value, and the aim of the management should be substantial profits, with an eye to future competition. In the case of public authorities these profits can be applied to various reserve funds or to the relief of the rates.

Returning to the form of analysis proposed in the former part of the paper, it remains to show the practical application of one form of tariff dealing with the points that arise.

Having divided the supply into suitable groups, there is no reason why the usual load-factor differentiation should not apply to each group, as far as is legally and commercially practicable.

In addition, a coal clause should be devised to adjust automatically the rates of charge with the price of coal. This usually takes the form of the addition or deduction of a minute part of a penny per unit for each 6d. or 1s. variation in the price per ton of coal, and is based on the average consumption of the whole supply. Since we have seen that one group consumes nearly three times as much coal as the other, a coal clause based on the average consumption means that one group is being subsidized by the other. Obviously, each group should have its own coal clause. But coal figures in both fixed and running costs. In the latter the charge is on the unit, and is the same in all groups. It may be expressed as so much per 1 000 units, to

the nearest farthing, for each variation of say, 6d. per ton in the price of coal.

For the coal in the fixed charges an opportunity occurs to compensate also for rises in wages and the increasing price of plant and capital. It will be generally admitted that these costs must follow more or less, the price of coal. If, after analysing a series of accounts as described, the rates per kilowatt-year for the fixed charges are plotted with the price of coal, it is probable that some approximation to a straight line for each group will be obtained. In Bedford for 6 years the points follow nearly straight lines, from which it appears that the rates of the fixed charges per kilowatt-year rise by 1·6s. and 6·3s. for the six- and seven-day groups respectively, for each 6d. per ton rise in the price of coal.

It is, therefore, possible to arrange what may be called a fixed-charge clause as well as a coal clause, both varying automatically with the price of coal

Applications of Tariffs, Protective Clauses and the General Findings of the Analysis to Various Classes of Supply.

6-day group.

Power.—In order to compete successfully with private plant, and to meet the changing condition of hours of labour, it seems essential that all power consumers should be strictly on a load-factor tariff. At the same time, in the author's opinion, a suitable maximum charge should be fixed in order to keep the operation within the limits of fair trading. The proposal of so much per kilowatt-year of maximum demand or per kilowatt installed, and so much a unit, is a little too safe for the undertaking. It is more in accord with other forms of trading to aim at a little higher profit and accept a little greater risk. It would be unfortunate to charge a power consumer 8d. per unit for one quarter if circumstances beyond his control temporarily reduced his load factor to bring about this result, and such a possibility would doubtless have a bad effect in arranging terms with new consumers.

As a practical method, the load factor can be obtained by assuming a maximum demand of, say, 50 to 60 per cent of the plant installed. The consumer can have the option of providing an approved maximum demand indicator.

Basic rates of 1d. per unit and £1 per ton for coal, with increments or decrements of 6d. per ton, may be adopted.

In such a case the terms of supply would be:

(1) 1d. per unit plus or minus a sliding scale of percentage, according to the load factor (set out in detail).
(2) Coal clause, say, 4d. per 1 000 units for each 6d. variation in the price of coal per ton.
(3) Fixed-charges clause, a sliding scale per 1 000 units according to the load factor for each 6d. variation in the price of coal per ton (set out in detail).

[*Note.*—The total rate is not to exceed 3d. per unit.]

The terms would not differ materially from a very usual trade form, such as: List price less 33⅓ per cent plus 10 per cent plus 15 per cent.

Generally speaking, more or less complicated terms with large concerns are workable, as a shrewd clerk readily perceives the advantage of getting acquainted with the system. A load-factor proposition in domestic supply is quite another matter, as the account is rather too apt to receive the personal attention of the principal.

Small motors for odd jobs can well afford the maximum rate and, notwithstanding the low load factor, the moderate rate is justified on the grounds that a sufficient number will probably have a fairly good load factor between them.

Cooking and heating.—As a first approximation this section has so far been included with the 6-day group, and in justification it may be pointed out that it possesses in a marked degree many of the characteristics of the large 6-day consumers.

Cooking makes substantial demands on the generating station for short and definite periods, therefore:

(1) As regards coal consumption, the efficiency on the plant kilowatt-hours scale is high.
(2) As regards wages, the demand is well within the range of two shifts, if not one.
(3) Large units of plant can be employed with advantage in capital cost, economy in running coal, and attendance.

There are mains problems to be taken into account, but actual practice may show that in many cases the cooking load will not overlap the heavy lighting load.

Some of the pioneer undertakings in this branch may already possess sufficient data to investigate the case thoroughly. In the meantime others will not be far wrong in adopting an arbitrary charge near the 6-day group rates.

Heating.—Promiscuous heating naturally occurs during the winter months, and is also confined to definite periods. The same arguments apply as in the case of cooking, the only difference being that, whereas cooking loads occur at definite times during each day throughout the year, heating occurs at more irregular times but only for about half the year. The plant set at liberty in the summer months may be utilized by the power section as relief to the power plant for summer overhauling.

Lighting.—The lighting of factories where the supply is high-tension only and limited to factory hours, may be treated as power load. It tends to lower the load factor of the factory and draws extra payment in this way.

7-day group.

Power.—If power is used on Sundays it will probably have a very good load factor on the usual scale, and therefore the price will not be unreasonably high If there is a considerable Sunday load the analysis will show no very marked difference between the fixed charges of the two groups. It is clear that any competitive scheme would have to pay Sunday rates in wages, so that a somewhat higher rate can be charged without endangering the business.

Public lighting.—The form of analysis gives direct

the charge for public lighting. In many instances the public lighting is charged to the public authority at cost. It is usual to prepare rate estimates twice a year, so that an opportunity arises of revising the public lighting estimates by applying the coal and fixed-charges clauses to suit the price of coal at the time, or what may be anticipated.

Private lighting.—For private lighting the rates found by the analysis with the present price of coal, etc., leave no room for variations on a load-factor scale under about 12 per cent. The analysis shows the proper flat rate to apply, and if there should be any further increase in the price of coal, etc., it also shows what rise in the price will meet the case.

Conclusions.

(1) It has been shown by a hypothetical case and by actual figures that there is a substantial difference in the cost of supply, depending on the degree of readiness to meet the demand.
(2) It has been shown that there is a substantial difference in capital charges to supply different classes of consumers.
(3) A method has been shown by which consumers may be segregated to satisfy (1) and (2) within reasonable limits.
(4) A system of analysis has been shown, making it possible to produce systematically revenue account sheets, year by year indicating the profits and losses on the several services.
(5) Protective clauses, coal and "fixed charges," have been reviewed and discussed.

Frequent reference has been made to the relation between units output and what has been called plant kilowatt-hours. It might be expressed as a percentage and is in need of a name. Until a better one is forthcoming I suggest "Work Factor."

For all statistics connecting coal and units it is essential to know this factor.

Comparisons between different undertakings on thermal efficiency are useless without this factor for correction, and, very possibly, a few more factors. We have seen that the consumption of the same plant with the same load factor can vary from about 12 lb. to 8 lb. or less, for no other reason than the development of power load, which has a high "work factor" and therefore improves the average.

In cases where the boiler feed is measured, the employment of the "work factor" for the boilers is necessary in estimating the effect of bad coal or improved stoking.

On the whole supply the effect of improved plant can be measured by correcting the "work factor," and gives a useful indication when it is necessary to balance capital expenditure with improved fuel consumption.

If we had statistics of "work factors" they would no doubt throw light on the big power-distribution schemes.

Load factor, as we have seen, is by no means the whole case. Comparatively small stations situated in districts with a load factor of about 30 per cent, but with a high "work factor," might easily be more efficient than a super-power station.

With the large amount of data collected day by day in the larger undertakings, there should be as much scope for special and continuous research as there is for a chemist in a steelworks. Continuous checking and testing of the tariffs are as important as watching the cost of generation. Occasions arise when the timely correction of properly measured amounts may easily be worth 10 times the saving due to the best engine-room and boiler-house supervision.

It should never be forgotten that practically all tariffs are based on certain assumptions, liable to vary with every new development. It may be the expansion of some particular service, the price of coal, factory hours of labour, or a gradual change of power factor.

In fact, the more these points are studied the more problems awaiting investigation crop up.

It is not suggested that tariffs should be frequently altered, but that weak spots should be watched and judicious remedies prepared ready for the psychological moment when it comes.

But, of all the many thoughts that arise in this subject, perhaps the most interesting is the scope and meaning of the Hopkinson principles.

None will deny that these principles, as expressed in his famous paper, have been of immense service to the electric supply. They were so remarkable and so reliable that many engineers came to accept them as articles of faith and, as such, some may feel an uncomfortable disturbance and almost resentment at the idea of attempting to analyse or question the various assumptions upon which they are based. But, so far from overthrowing an idol, closer scrutiny only serves to enhance the fame of the master. In the glamour of load-factor miracles we are apt to lose sight of Dr. Hopkinson's general design. The pattern is bounded on all sides by the consideration of gas competition for lighting purposes. It is set out on established facts and test figures, and is decorated with happy analogies. Systematically the various factors are interwoven and assume a pattern, with an echo in the compound charge. With the design before us we can modify the boundaries, substitute later facts, subdivide the factors and vary the pattern. There is no finality in the process, no limit to the patterns, and each in its day may serve a useful end. To Dr. Hopkinson we owe this art of pattern-making.

The discussion on this paper will be found on page 719.

MULTI-PART TARIFFS FOR DOMESTIC ELECTRICITY SUPPLY.

By J. W. Beauchamp, Member.

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Summary.

The object of this brief paper is to draw attention to the urgent necessity of accepting the principle of multi-part charging for electricity supply; also to show that flat rates, and minimum charges based upon them, have outlived their usefulness, and that the progress of electricity supply has left behind the conditions which gave rise to certain features of early electricity legislation, and so rendered them out-of-date and obstructive to progress.

The ground covered by the paper is by no means new, but there are certain features of the domestic load which seem to call for special consideration and treatment; for instance, the desirability of regarding such load and its characteristics in blocks or by averages in place of the particular attention which has in the past been given to the individual consumers.

Attention is also directed to the need of establishing a basis for two-part tariffs which shall take into consideration the growth of maximum demand and the need for increased mains capacity which would follow from extensive domestic electrification.

Many of the existing multi-part tariffs do not regard this, and it is feared on that account the running rate per unit may have to be higher than would be the case if a method were adopted whereby notable increases in domestic demand would be accompanied by reasonable increases in the fixed charge payable by such consumers.

The fact that a reasonably complete state of domestic electrification may give us 3 times the revenue at present obtainable from such consumers, associated with a demand for 10 times the amount of electrical energy in units, suggests that the business is well worth cultivating, and that in all probability it will become possible to cater for it directly on a basis of averages rather than by precise individual measurement.

At the present time opportunities for discussing this matter are valuable, and proper consideration for it must form a feature of legislation in the immediate future if the last, and perhaps greatest, field for the detailed distribution of electricity is to be secured.

Introduction.

I propose to deal with this subject in the light of recent work of the British Electrical Development Association in regard to domestic electricity supply, particularly supply for the working-class type of house, and for the flat or dwelling usually occupied by those known as the middle classes.

At present we are interested in modernizing the home, and in finding out to what extent science and mechanical devices can reduce labour and make housekeeping easier under present conditions, without entailing too much capital expenditure.

A study of the subject reveals the fact that in introducing electricity as a domestic aid we do much more than substitute one form of light and heat for another; the characteristics of the electric method are such that its use fits in with many changes which can be made in home management; reorganizations, impossible even with the best existing equipment, become easy when we employ a single medium which will provide light, heat or mechanical power, and perform its work without creating any additional work.

One of the most important factors in bringing about the fuller use of electricity for domestic purposes will be the principle of charging adopted for the electricity supply.

Multi-Part Tariffs.

It is generally accepted that the multi-part tariff in some form is the most promising and equitable, and its progress has been delayed perhaps by too strong a desire to fix upon a correct or uniform tariff, rather than acceptance of the underlying principle and determination to put it into use and allow experience to dictate the details most suitable for each district.

Furthermore, there has perhaps been too much tendency to consider proposed tariffs in relation to individual consumers, rather than in relation to groups of them; apparently we must now think in larger units, and if we are to give supply in every house we may properly generalize and draw our statistics in terms of streets and districts rather than of individual houses.

The multi-part tariff and its calculation divides itself into two sections, one concerning the running rate or rates per unit for the energy supplied, which is not difficult to determine, the other concerning the fixed charges. The latter presents greater difficulties, because they must bear some relation to standing costs incurred now, and to be incurred later as a result of the new tariff and the extra business it will bring; in addition, they must bear a relation to something tangible and easily understood by the consumer in order that he may feel their equity.

A new method of charging should be of such a character that if it succeeds and brings an immense development, the tariff, or at least the principle underlying it, must enable the added business to be carried out profitably.

It is here that danger may arise; at present it is obvious that when considering multi-part tariffs many engineers think of the fixed charge in terms of what to-day appears to be the normal lighting bill for a particular consumer.

If a tariff based in this way should produce a big increase in the heating and cooking load, it may call for extensions in mains and plant which the fixed charge would not cover, and which would have to be

met by the unit charge. This would entail an average charge too low for the consumer who makes but little use of electricity, and too high to encourage the one who employs it extensively.

For this reason, in spite of obvious difficulties, it seems necessary that the fixed charge should be a function of the standing costs which the consumers or groups of consumers may entail as development proceeds; in short, it must be related to kilowatts installed or maximum demand, allowance being made for the character and diversity of the load resulting from the different uses of the supply.

In systems where the fixed charge is based on lighting installation, floor area, and rateable value, it is apparently calculated in regard to the consumer's rational use of electricity for lighting, that is to say, his lighting bill under present conditions and with existing lamps and methods.

We may find it necessary to maintain some basis of this sort, because the maximum demand of each consumer arising from lighting seems likely to decrease for a long time to come by reason of development of the lamp and by the more scientific use of it; we may be approaching a time when we shall ask ourselves: "What, in view of the cost of alternative methods of lighting and the superiority of electricity, will a consumer pay per annum for providing his house with normal artificial light?"

The figure thus obtained would then be a sort of primary fixed charge or subscription per annum for the lighting service.

Beyond giving this service we should desire to sell to the consumer as much electricity as he could use for heating, cooking and power, and, although the efficiency of the appliances may increase somewhat, new methods of cookery, of heating and of ventilation may in the future give to the user a bigger return for the energy consumed, yet so far as we can see it is not likely that he will obtain apparatus for turning electricity into heat or power with an efficiency increasing from year to year as has been the case with the light-producing appliances. The relation, therefore, between maximum demand, units used and service rendered may be stable for a long time to come in the case of heating and cooking, but subject to progress and inventions in regard to heat storage.

It is suggested, therefore, that in considering tariffs of this class lighting should be dealt with by itself, and an annual charge in regard to it determined, after which further annual charges should be evolved for all other uses of the supply by considering the consumers where possible in groups as persons who want electricity for heating, lighting and power. In other words, by considering the house, or blocks of houses, as one would consider a factory applying for supply.

In this way one can obtain a clear view of the influence of groups of houses on the load curve and the mains, and what must be charged per year against apparatus connected and per unit for energy used in order that there may be justification for carrying out extensions of mains, the principal revenue earned by which will be derived, not from lighting, but from the other uses of the supply.

Although to-day the maximum demand associated with lighting is small actually and very small relatively to the demand arising from heating and other appliances, there is no need for alarm on account of the expenditure which increasing use of the latter may entail; their "diversity factor" is very large, storage methods will help, and the very fact that electrical apparatus is always ready for use leads to its employment over a wider range of hours, and so again increases the diversity value.

Nevertheless, development will entail extension and must be able to pay for it, and it is for this reason that new tariffs should be designed to take care of the standing costs so that the unit charge may be kept low, resulting in a distinctly dropping average cost as the load factor increases. Many tariffs have failed in their object because, whilst sound perhaps in principle, they did not in practice offer progressive encouragement to the steady introduction of more and more consuming devices.

The home supply tariff may start with quite a high rate for normal lighting, and may secure a fair price for small additional use in fans, kettles, etc., but should continue to decrease through the consumption in cookers, fires, etc., ending in a minimum value which would apply to storage heaters or other 100 per cent load-factor uses.

The Minimum Charge.

The necessity of collecting a definite amount per year on account of standing charges has been recognized in the minimum charge.

Originally and in most cases this amounted to the equivalent of 20 kWh per quarter at the statutory maximum price, which, if we take to be 8d., gives us the figure of £2 13s. 4d. per annum.

Recently, in connection with increases in the maximum charge, it has become the general practice to allow 15 kWh for each winter quarter, and 10 kWh for each summer quarter, or 50 kWh per annum at the maximum rate, say 1s. per unit, or £2 10s. per annum.

Unfortunately, in many places to-day a consumer who pays less than £5 per annum is a source of loss to the undertaking.

Any minimum charge is unpopular; it is not convenient to the consumer to understand the need of it, and in justice to him it must be said that no great effort has ever been made to explain the matter.

The unpopularity of a charge which appears to be made for something not delivered has led many of the larger undertakings in the past either to reduce or waive it. This is unjust to the users whose consumption exceeds the minimum, but, owing to the small amounts usually involved generally considered worth while as avoiding trouble, this action has caused trouble to small undertakings who could not afford to do the same, and also to large ones serving great numbers of small consumers who, by the use of modern lamps and on account of daylight saving, need for economy, etc., have been able to get sufficient light with an annual consumption below the minimum amount.

During the period of lighting restrictions a good deal of attention was attracted to this unpopular

charge, and no sympathy is to be expected in any attempt to increase it. Apparently, in considering this question, and also that of the statutory maximum per unit, consideration has been given only to the increase in cost of supply, and not at all to the value received by the user.

When the minimum of 80 kWh per year was established, carbon lamps were in use, a small consumer using, say, two 30-c.p. lamps for 1 000 hours per annum requiring 240 kWh at 6d., i.e. a cost of £6.

To-day he would use only 60 kWh at 1s., or £3.

In the first case his maximum demand was 240 watts, and in the second 60 watts.

Thus, for the same light value received the consumer's bill has fallen to one-half, in spite of the charge per unit being doubled.

Most of the two-part tariffs in use constitute a minimum charge, the annual or quarterly amount being payable irrespective of the number of units used. In some systems the fixed charge covers a certain number of units, and is so far an agreed minimum payment; in others the fixed charge covers no supply but only readiness to supply, or service.

This form, in which the first charge is in the nature of a subscription, is the best and should be developed because the charge is not made for any particular supply, but rather for maintaining the connection, so disputes cannot arise as use is restricted. On the other hand, the knowledge that every unit is supplied at one or more low rates and that the meter records only these cheap units encourages use, reduces anxiety about waste, and leads the consumers to think and talk about their electricity supply in terms of those running or secondary charges. This is as it should be, because the increasing use of the supply and particularly the employment of apparatus always in circuit soon causes the average cost of the unit to approach the running rate.

It is desirable in tariffs of this class to take some trouble to show the users the need for the fixed charge, and what it covers; during the war many consumers, whilst agreeing readily to increases on flat or running rates, objected strongly to advances on the fixed charge, because they failed to appreciate that such items as wages were included in them.

The best method of bringing home the soundness and advantages of the multi-part tariff lies in putting it forward as an alternative to a very high flat rate, and it will be necessary either:

To secure the right to associate such tariffs with much higher statutory maximum rates than now possible; or

To introduce the new systems by means of very active propaganda, and support them by hiring out many of the appliances which the consumers will need if they are to take the fullest advantage of such tariffs.

It has been urged upon the authorities that new methods of charging designed in this way are essential if the domestic load is to be secured and the benefits of fuller electrification are to be obtained in the near future in the home.

A request was made that the Electricity Commissioners should have the power to make such tariffs compulsory in any district or part of a district where the suppliers desired it, and could show good reason for its adoption; or failing this that where the suppliers were willing to charge on such a system their action should be supported by permission to impose an alternative flat rate much higher than would otherwise have been allowed.

This is a subject of great interest to manufacturers and contractors, and they can do a good deal to urge supply undertakings to introduce such rates, the authorities to help them to do so, and the consumers to understand the system and appreciate how they may turn it to their advantage.

As a matter of history it may be recorded that in 1901 the London Chamber of Commerce suggested certain electricity clauses for submission to the Board of Trade, amongst others the following: "The Undertaker should be entitled to charge the consumer at the following rate per quarter.

A. A service charge of 10s. per 100 watts of the maximum power which has been demanded by the consumer.

B. For the actual amount of energy supplied at 3d. per unit."

Suggested Skeleton Domestic Tariff.

Fixed charge per annum.—On capacity of apparatus in use or usable (divided quarterly into equal parts or otherwise).

(1) *Lighting.*—Up to and including 250 watts, £*x* per annum. Above 250 watts, per complete 50 watts, £*x*/10 per annum.

(Intermittent lighting can be excluded, or a percentage only of installed lighting taken into account.)

(2) *Heating and power.*—Small appliances (say up to 600 watts each, with an aggregate limit of 2 000 watts) having a very high "group" diversity in time and extent of use. No fixed charge.

(3) *Heating and power.*—Appliances as (2), above 600 watts each and 2 000 watts aggregate. Ranges, fires, water heaters, etc. At the rate of 250 watts for £*x*/20 per annum in steps of 250 watts.

Running charge per unit.—A flat rate for all purposes.

Under such a system as outlined above, the average cost of energy would decrease as follows:

Fixed and Running Charges.

Lighting only; £5, + 1½d. per kWh:

	Average per Unit d.	Total Units	Total Account £ s. d.
$\frac{£5 + (300 \times 1\frac{1}{2}\text{d.})}{300\ \text{kWh}}$	= 5·5	300	6 17 6

plus small accessories; say, 100 kWh extra:

$\frac{£5 + (400 \times 1\frac{1}{2}\text{d.})}{400\ \text{kWh}}$	= 4·5	400	7 10 0

plus electric range; say, 1 750 kWh extra and £1 addition to fixed charge:

$\frac{£6 + (2\,150 \times 1\frac{1}{2}\text{d.})}{2\,150\ \text{kWh}}$	= 2·16	2 150	19 8 9

plus electric fires; say 2 000 kWh extra, and £1 addition to fixed charge:

$\frac{£7 + (4\,150 \times 1\frac{1}{2}\text{d.})}{4\,150\ \text{kWh}}$	= 1·9	4 150	25 18 9

Tariffs for Working-class Houses.

The introduction of electricity supply into the new housing colonies affords a good opportunity for experiment with multi-part tariffs on account of the standardized character of the houses and the probable similarity of habits amongst those who will occupy them.

So far, electric lighting has been provided for many of these colonies with meters in the ordinary way and with some minimum charge; also with current limiters and no meters; and also without any measuring device, at inclusive rates per week. Experience varies very much as to the amount of waste taking place where such contract supplies are given without meters; in many cases it is obviously very small, and the contract method with or without the use of current limiters and with some arrangement for control of lamp capacity gives satisfaction, and may even be the only means of introducing electricity to such users in cases where service and accountancy costs must be kept very low.

The best and most flexible method, however, is to impose a fixed charge and provide for all supply to be recorded by one meter for each consumer and charged at a simple flat rate for all purposes.

These installations may need only 80 to 100 kWh per annum for lighting at 8d. per unit, say 52s. to 66s. 8d. per year, or 1s. to 1s. 3½d. per week.

Even a flat-iron used for two hours per week would increase the annual consumption by 40 units, or 50 to 40 per cent and, at 2d. per unit, the revenue by 6s. 8d., or 13 to 10 per cent.

The use of a cooker at, say, ¾ kWh per day for 5 persons for only 200 days would call for a further 750 units, increasing the lighting consumption by 6 or 7 times and raising the revenue (at 2d. per unit) by 125s., i.e. 2 to 2½ times.

It is, therefore, obvious that the lighting business should, in connection with such property, be regarded rather as a means than an end, and the tariff and metering systems should permit of that progressive electrification of the houses from which so substantial a return is promised. For the same reasons, special treatment of such installations in regard to pressure of supply is to be deprecated, as the smallest house occupied by a family would consume not less than 1 000 kWh per annum, and should take a great deal more.

Water Heating.

Every encouragement should be given to devices of the heat-storage class; unfortunately, inventors are attracted by the possibilities of the electric geyser owing to its quickness and simplicity, and they are apt to regard the indifference of the supply engineer towards such appliances as showing lack of enterprise. The remedy is to encourage development in the opposite direction.

There will no doubt be a field for small geysers, and the difficulties arising from this class of apparatus are only relative; where distribution mains are heavy they may not be so unwelcome, and the use of very large numbers would produce a valuable load owing to diversity in use, but the heavy cost of internal wiring must also be against them from the consumer's point of view.

To raise 6 gallons of water from 58° F. to boiling point requires (at 90 per cent efficiency) 3 kWh; on a 200-volt supply an appliance to effect this in

20 minutes would be of 9 kW capacity and take 45 amperes; and in
10 hours would be of 0·3 kW capacity, and take 1·5 amperes.

Expenditure on Distribution.

This important item perhaps only touches the subject of tariffs in so far as it is necessary to increase the utility factor of all distribution apparatus, and a correct tariff will work towards that end.

To allow of developing the domestic load, mains must be provided on a generous scale and can only become remunerative when the load is obtained. Under present circumstances it is not easy to obtain money for such works which, although practically imperishable, may yet be a source of loss for a considerable time.

As against this it must be remembered that the cost of a mains network does not rise at the same rate as the energy-delivering capacity provided; particularly is this the case where road work is expensive.

The following figures indicate approximately the relation between capacity and cost of low-tension cables laid under average road conditions.

Copper Area	Ratio of Area	Cost Laid
sq. in.	Per cent	Per cent
0·0145	100	100
0·0225	155	104
0·06	413	134
0·1	690	163

This shows how much future success may depend on the ability to invest freely in such works at first, and the will to load them up by the proper use of publicity, hire of appliances, etc.

The piecemeal method of laying mains to suit immediate demand and following up the consumers rather than laying out a network in advance of them, was much practised even when costs were less than one-half present values, and has been responsible for a good deal of inflation of distribution capital charges.

In connection with the difficulty of financing mains extensions and even services on some undertakings, it might be worth while to explore the possibility of offering to those potential consumers who call for any considerable expenditure some means of subscribing towards a capital fund devoted to the purpose, taking up small shares or bonds, redeemable after a term of years, bearing interest, and admittedly issued to support a special extension fund out of which the subscribing consumers' cable work would be paid for.

Something of this kind has been done on a larger scale in connection with power supplies, and at first sight there appears to be no reason why the small companies and municipalities should not adopt it for general domestic and business consumers whilst the raising of money in the usual way remains difficult. The 6 per cent Housing Bonds offer an example, although it is not suggested that 6 per cent need be paid, seeing that the money advanced is to be spent on works which the subscriber requires to be carried out.

One undertaking in the Midlands with a very high output per head of population, low average price, and good financial results, reports :—

"The use of multi-part tariffs has greatly increased the demand for supply and for apparatus; very few complaints have been received, and although the accounts of this group of consumers have increased considerably they are the most satisfied of all the consumers.

"Small consumers who were unremunerative even at 8d. per unit have been converted to this tariff, have installed heaters, etc., and have become profitable to the undertaking.

"There are, however, some very small office users who are not profitable even at 2s. 6d. per unit, and complain of the low minimum charge in force (5s. per quarter). Their unjustified complaints have a bad influence on potential consumers.

"The multi-part tariff has helped the business in small appliances by cutting out the cost of special wiring and plugs. At the present high lighting rates people hesitate to use irons, etc., on lamp circuits, and object to the cost of separate circuits for small apparatus. The multi-part tariff has overcome this difficulty.

"Tariffs of this class have come to stay."

Conclusion.

If this paper stimulates further thought on domestic tariffs, particularly perhaps amongst those on the commercial side, it will have been of service. Many of us have considered this subject for years and have discovered most of the disadvantages which attach to such systems of charging, and perhaps also imagined others which would not prove serious in practice.

Any successful solution must be in the nature of a compromise. To discover it we must not be too scientifically exacting and must, I suggest, regard statistics and results rather in relation to groups and areas than to individuals, considering what is likely to happen at the end of a feeder rather than at the end of a service.

Note.

The attention of members is drawn to a paper read by Mr. E. W. Cowan before the Economic Section of the British Association in September, 1910.

This paper supports differentiation of charges and deals with the question of one part of a supply being a by-product of another. Many of the author's arguments have been discussed before the Institution, but the paper should be referred to by those interested in the subject.

The number of consumers on public electricity supplies in the United Kingdom is stated to be between 900 000 and 1 000 000 (services) or, say, 10 per cent of the total number of buildings and dwellings; assuming that 25 per cent are at present beyond the reach of supply, the remainder, 65 per cent or 6½ million, are waiting for the progressive supply engineer who, in entering these premises, also admits the manufacturer and the contractor.

APPENDIX.

Example of Consumption in a Flat or Villa.

	£	s.	d.
(*a*) Lighting; 300 kWh at 8d.	= 10	0	0
(*b*) Small appliances; 200 kWh at 1½d.	= 1	5	0
(*c*) Cooking; 1 750 kWh at 1½d.	= 11	0	0
(*d*) Fires; 1 000 kWh at 1½d.	= 6	5	0

If we take (*a*) as normal, or 100 per cent, we obtain :

	Consumption Per cent	Account Per cent
(*a*)	100	100
(*a*) plus (*b*)	166	112·5
(*a*) plus (*b*) plus (*c*)	750	222·5
(*a*) plus (*b*) plus (*c*) plus (*d*)	1 080	285·0

Or the complete electrification, excepting only the use of one or two coal fires and provision of household hot-water supply, represents an increase in consumption over lighting only, to 10 times, and an increase in the account over that with lighting only, to nearly 3 times.

Example of Consumption in Working-class Houses.

(*a*) Lighting: 100 kWh per annum at 8d. = £2 16s. 8d. (or 1s. 1d. per week).

(*b*) Cooking: 1 500 kWh per annum at 1½d. = £9 7s. 6d.

(*c*) Occasional heating: 1 000 kWh per annum at 1½d. = £5 5s. 0d.

If we take (*a*) as normal, or 100 per cent, as before, we obtain :

	Consumption Per cent	Account Per cent
(*a*)	100	100
(*a*) plus (*b*)	1 600	431
(*a*) plus (*b*) plus (*c*)	2 600	651

That is, the consumption is increased to 16 and 26 times respectively that with lighting only, whilst the account is increased to about 4¼ and 6½ times respectively.

DISCUSSION ON "TARIFFS."

Before The Institution, 28 April, 1921.

The discussion was opened with introductory papers by **Mr. J. R. Blaikie,** Member, entitled "Electric Supply: Present Conditions and the Hopkinson Principles" (see page 701), and by **Mr. J. W. Beauchamp,** Member, entitled "Multi-part Tariffs for Domestic Electricity Supply" (see page 714).

The President (Mr. Ll. B. Atkinson): I think the method Mr. Blaikie has adopted, of taking two or three different years where the class of load has varied and of finding certain factors by an equation, is at least quite novel. That method of ascertaining some of the constants is one of the points that will justify a very full consideration of the proposition which he has put forward. I am not quite sure in my own mind that the division of consumers into 6-day and 7-day groups, beyond being a broad method, is at all correct, because it is found to be not so much a question of the group to which they belong as whether they are users during particular, well-defined hours of the day. Mr. Beauchamp's paper gives some useful particulars of how to achieve what we are all struggling to do, i.e. to get electricity supply installed in the new dwellings which are being erected in such large numbers throughout the country, and where only a moderate expenditure on lighting and heating is entailed. The Council desire to give this question of tariffs the very fullest consideration, both from the technical point of view and also from that of what may be necessary in regard to alteration of the legislation by which we are at present guided. It is necessary first of all for engineers to arrive at the proper method of charging, and then, if it transpires that legislation is necessary, we shall go forward with much greater force if we have thoroughly established in our minds that the present system is inapplicable or unfair under existing conditions.

Mr. Arthur Wright: I think the Hopkinson principles should be supplemented as follows: (*a*) Acknowledgment that it is commercially impossible to measure the demand on the investment by the intermittently-used, small and ever-increasing electrical appliances in domestic houses, whose diversity factor entitles them to a good load-factor rate. (*b*) The advisability of granting rebates in proportion to the total consumption of electricity. (*c*) The necessity of making an adequate service charge. (*d*) The wisdom of looking to the running charge as the main source of profit. And (*e*) the necessity of a power-factor clause based on a kilovolt-ampere demand and not on average power factor. With these additions I still believe that the Hopkinson principles enable us to make the best and most equitable tariffs and analysis of costs of supplying electricity. There are now in use two distinct types of tariffs: those based on what the market can bear and those based on a reasonable profit on the cost of supplying each individual consumer. While the first has the advantage of following ordinary trading practice and is easy of application, I think that the second has greater chances of extending the use of electricity in profitable directions and of meeting with the support of Public Service Commissions because of its greater equity. A compromise between the two should also be considered, as it can be justified on many grounds; namely, a tariff based upon the maximum amount the market will bear above the increment Hopkinson cost of the additional supply. The test of a correct tariff is the annual rate of increase of a profitable load, and only the Hopkinson analysis can enable this to be done. The usual analysis of average cost and company's profit-and-loss accounts does not enable us to determine, for instance, when interconnection is advisable from a generating point of view. In my opinion, standardization of accounts on Hopkinson principles should now be attempted, so as to provide for the time, which is bound to come in the near future, when finance will not permit of the further installation of generating plant in a city when a supply at lower cost can be obtained from outside. It should be remembered that there is now no great difficulty in ascertaining the cost of supplying electricity to the end of feeders, or to substation switchboards, and, as the revenue of a district is equally ascertainable, a correct profit-and-loss account of a district can be made out. This also applies to large consumers and bulk supply. Therefore it only remains to allocate among the various smaller customers the distribution demand and service cost, and of these the service cost is easily ascertained. The remainder must, for the present, be arrived at by a cut-and-try method, tempered by expediency. The office and window lighting of shops will not, in my opinion, permit of any profit, and must be looked upon as a tax for the concession or a gift to a city for reasons of policy, because the sufficiently high price would not generally be permitted in this country. Experience abroad has convinced me that rapid electrical development depends as much on efficient salesmanship as on simplification or lowness of tariffs; in other words, the selling department should be looked upon as the most important one in the business and the best men should be encouraged by attractive salaries. In case any members should think that complication in charges for electricity tends to retard electrical development, I invite them to study the elaborate rates adopted by the very successful (electrically) cities of New York, Chicago, Los Angeles and Boston. These are fully given in the National Electric Light Association Year-Book. The reason that complicated tariffs are successful is the general confidence that people seem to have in the Public Service Commissions. They now realize that rates are a very technical matter which they cannot hope to understand. We cannot make rates intelligible to the general public; the theory is far too complicated. Mr. Blaikie appears to think that the present-day fixed charges bear a smaller proportion to the total cost of electricity supply than they did in pre-war days. This may be so on his pre-war investment,

but tariff discussions should be based on future costs and ratios, and I think he will find the ratio will be as great if not greater in the future, especially if he continues to separate his coal into stand-by and running costs. I think the cost of coal and rough labour will, in the future, diminish more rapidly than the cost of money, equipment, taxation, skilled labour and management. We have lately been so occupied with the coal shortage and labour troubles that we are apt to overlook the fact that there is at present, and will be for a long time to come, an equally serious shortage of something that concerns us almost as much, namely, capital, of which there is very little available for electrical purposes and which has also struck for a much bigger rate of interest than it obtained seven years ago, for the very good reason that it can obtain it. I am convinced that electrical development in this country will not be able to get the requisite capital for extensive progress until it is recognized by the powers that be that there is now a very limited amount of money available for all industrial enterprises, and that the past returns on electrical investments, in view of the rapid obsolescence of plant, etc., have not been encouraging. Until electrical authorities are allowed a return on their ordinary capital amounting approximately to double the average bank return during the year in question, and obtain a permanent tenure with the ordinary business practice purchase clause, it will be difficult to obtain capital. While the last seven abnormal years form no criterion as to future costs of supply, or the correctness of existing tariffs, they have been of substantial use to the electrical industry, (i) in greatly increasing the appreciation of the necessity of a general electric supply; (ii) in the creation in this country of an independent Government department of experts to help in electrical matters; (iii) in obtaining authority for raising the maximum charges; (iv) in helping to realize the necessity for a more efficient use of capital, skilled labour and fuel; and (v) the lesson has been learnt that all great industries dealing with two or more towns, to be run efficiently must be managed by well-paid men and financed by private capital. Experience has convinced me that the radius of economical electricity supply to-day exceeds the radius of efficient municipal control, and that a free trading monopoly with long tenure, under the control of expert Public Service Commissions, is vital to successful electrical development. I do not at present understand Mr. Blaikie's grouping of consumers into 7-day and 6-day groups, and I hope he will enlarge on that point in his reply. The Hopkinson principle supplies all the division that is necessary for determining the rates. I entirely agree with most of Mr. Beauchamp's conclusions. There is one subject which I have carefully studied, namely, electrical cooking. On the other side of the Atlantic it has developed to enormous proportions, but it has been found there that the old standard of regulation of pressure has to be departed from very largely if sufficiently cheap electricity for cooking is to be supplied Unless cookers can have some automatic thermostatic control to allow for varying pressures so that the consumer does not complain that the cooking takes three times as long as it should, due to low pressure, or unless the cookers have multiple switches so as to provide for the lower pressure, it will always be extremely expensive to supply electricity for cooking, because in winter the latter synchronizes with the peak. There is no reason to-day, in my opinion, why the old pressure regulations, which are based on carbon-lamp efficiencies, should be maintained. The metal-filament lamp is almost self-regulating on pressure, and it is found that consumers will stand a very much bigger variation of pressure than they could when carbon-filament lamps were used. The cooking business is so huge and so promising that it is worth while for manufacturers and everybody else, first to get the regulations in regard to pressure modified, and secondly, to get the apparatus adjusted so as to work efficiently under slightly different pressures.

Mr. H. W. Couzens: I propose to devote my remarks to Mr. Beauchamp's paper, which I consider to be of paramount importance. Now that the arrears of post-war consumers have been practically cleared off by the supply companies, the next important field is that of cooking. Hitherto, cooking apparatus has been developed by the manufacturers and traders, with the central station engineer as a mildly interested spectator. That is all wrong, and I think the central station engineer has to take the primary place. One of the most important features, apart from the hire of apparatus and propaganda by educated instructors, is the development of a tariff which will appeal to the ordinary consumer. I have recently taken a small flat in London where the tariff for electricity supply is 8d. per unit for lighting, 3d. per unit for heating, $2\frac{1}{2}$d. per unit for power, and $1\frac{1}{2}$d. per unit for cooking. This was too complicated, and I therefore went to the supply company, and we made an estimate of my lighting requirements. We added £1 to that, and I now pay £5 per year plus $1\frac{1}{2}$d. per unit. I use electricity for all purposes except domestic hot water, and the amount I have to pay is about £15 a year, so that the supply company receive nearly four times as much as they would otherwise have done, and I am thoroughly satisfied with the bill. I think it would be very difficult to make a rigid fixed charge at the moment, because the circumstances in regard to electric cooking, particularly as to the maximum demand, are not thoroughly known. Take, for instance, the Brompton and Kensington Company. They tell me they have about 500 cookers on their circuit, and they have developed the load within the last 12 months so that there are now about 2 000 kW installed for cooking purposes. Their maximum load in respect of that is rather difficult to ascertain, but appears to be not much more than 250 kW. Therefore it does not seem to me to be necessary to make the fixed charge too high. On the other hand, the fixed charge ought to be simple, so as to be understood by the consumer. I recently received a "Schedule of Electricity Charges" from an indignant consumer who is supplied by a Borough Council in Greater London. The rates for power, heating and cooking are on a sliding scale, and are very high. There is an alternative "telephone system" at a charge payable quarterly of £30 per kW per

annum of maximum demand, plus 2d. per unit. That is enough to frighten anybody. With regard to the question of mains, I do not think the distribution will be quite such a difficult problem as some people imagine, particularly where the supply is alternating current. Where it is direct current I think it will be necessary to change over to alternating current.

Mr. P. D. Tuckett: I propose to confine my remarks to Mr. Beauchamp's paper, which seems to me to afford a way out of the difficulty in which a large number of the smaller undertakings in this country are now involved. At the present time, under the maximum lighting rates which they are allowed to charge, many of their lighting consumers are being supplied at a loss, which other consumers are necessarily called upon to make good; in fact, in certain cases the present tendency is for the maximum price to become the minimum price. It is entirely uncommercial, but is inevitable with the artificial and arbitrary price restrictions to which we are subject Under a multi-part tariff, with a fixed quarterly charge adequately covering the standing costs, this difficulty would be avoided, whilst the comparatively low unit charge would encourage consumers to make the fullest possible use of the supply. I am in substantial agreement with Mr. Beauchamp's arguments in support of a multi-part tariff, but there are one or two points to which I should like to refer. First, I think it is hopeless to try to secure a scientifically correct or uniform tariff. The conditions vary in every town, and we all know the practical objections to attempting to apply a scientifically correct tariff. What we want is either much higher maximum rates as an alternative to our permissive multi-part tariffs, or the right to enforce these tariffs in place of the present flat rates. I am sorry to say I do not share Mr. Wright's confidence that the controlling authorities in this country can be relied on to sanction compulsory multi-part tariffs to yield an adequate return under the varying conditions which exist, and I should therefore prefer a high maximum rate, leaving each undertaking free to secure the adoption of an alternative multi-part tariff on its merits. At present, owing to the inadequacy of the maximum rates, there is great difficulty in applying a permissive multi-part tariff where it is most needed. There may, for example, be a number of identical houses with bills ranging from £5 to £15, with an average of £10, owing to the very different use which the various consumers make of their installations. Under these conditions, with an insufficiently high alternative flat rate, a permissive multi-part tariff based on the £10 average will be readily adopted by the £15 consumer but will not be entertained by the £5 consumer, and consequently, so far at any rate as existing consumers are concerned, the initial tendency is for the undertaking to lose by its adoption. In connection with the determination of the proper fixed charge under a multi-part tariff there is a very important consideration, referred to by Mr. Beauchamp, which I should like to emphasize. He points out that if the encouragement of the domestic load is to be capable of indefinite development the tariff must be based on the post war cost of capital and plant so as to ensure a steadily-increasing and productive return. Otherwise its very success will bring financial failure when the expansion of the business calls for new plant and mains at 3 or 4 times their pre-war cost, for that is what I am afraid they are likely to cost for some years to come, when the higher cost of capital is added to their higher cost. There is one point in connection with the development of the domestic load which I am inclined to question, viz. the encouragement of a low-priced accessory heating load; I am not speaking of cooking. Are we satisfied that the ordinary radiator load is so much better than the lighting load as to justify a very much lower charge being made for it? It is mainly a winter load and is liable to overlap the peak of the lighting load, without, I think, having any better load factor or a much better diversity factor. Under these circumstances I find it difficult to believe that it is really a paying load at rates greatly below the lighting rates.

Mr. F. W. Purse: The subject of tariffs is the be-all and end-all of the existence of those connected with the electricity supply industry, because the spending of time and money in getting the last refinement is of no use if we cannot sell our electricity at a profit. Mr. Beauchamp read a paper of a similar character before the Incorporated Municipal Electrical Association last summer, and I differed from his conclusions to some extent. He says that we must look at this question from the point of view of load rather than from that of individual consumers. A consumer will see how much the one next door is paying and will be always complaining. We know that in the case of companies the latter tell their consumers they must "either take it or leave it," but with municipal authorities the consumer can come to the Council and argue that the engineer is mistaken and that the tariff is wrong. That is the sort of trouble with which we have to contend. I do not like Mr. Beauchamp's suggestion of starting with £5 for so much, then adding another pound and then another pound. We are asked to make the tariffs simple, but it is not simple to go to the consumer and say: "We shall charge you £5 and 1½d. a unit for lighting and allow you to use a few accessories. Then if you install a range it will cost £1 extra per kilowatt, and so on." That means an equivalent of four or five tariffs, and it is not simple. The consumer wants to know what he will have to pay in the end. I agree with the other speakers that we have sold electricity at too cheap a rate. Consumers do not mind paying a little more if they get satisfaction, but we require the apparatus as well as the tariff to do so. I think it is of all-important interest to manufacturers to produce goods worth handing over to our consumers. I mentioned at Ilkley last year that we had in the North the old 0·5d. tariff. In effect that worked out that the lighting was equivalent to the flat lighting rate and everything else was at the ½d. rate. Consumers would not have minded if it was 1d., provided they could have obtained well-made apparatus. I want to impress upon manufacturers that they must produce apparatus which we can put into consumers' premises and forget about, and not have to pay for maintenance twice the amount that we receive for hire. There is one point which is continually em-

phasized in connection with the multi-part tariff and which I have often protested against, namely, to have one meter and one system of wiring throughout the house. The lighting circuit must be kept absolutely separate from the heating and power circuits throughout the house. A single meter may be permitted, although I do not altogether agree with that method, because if all the consumption is registered on one meter it must be big enough to carry 25 amperes. It may be said that a loss of a few units can be afforded, but if a few units at 1½d. are lost all round, what will be the effect on the electricity undertaking? I do not think it will pay. Then Mr. Beauchamp suggests that we ought to go in for generous mains extensions, but, in view of what they cost at present, we can hardly do that. We have always been deterred by the cost. We have never received enough money for our current to encourage us to go on laying mains and, as Mr. Wright says, the position will be worse in the future. I think Mr. Beauchamp is a little unhappy in his suggestions with regard to the tariff. He says: "Suggested skeleton domestic tariff." It will be noticed on page 716 that he gives £5 plus 1½d. per kWh, and then he adds some extras and makes a total of £25. He gives us, again, another example of 300 kWh for lighting and he makes the total £28 10s. He goes back to 8d. for lighting. It is not very important, but it would have been rather better for comparison if he had kept the details exactly the same. Generally, with regard to a multi-part tariff I entirely agree with him if the two-rate tariff can be imposed as an alternative to a fixed charge, but I entirely disagree with estimating and similar work going on. We must endeavour as far as possible to measure it, otherwise there will be complete dissatisfaction, which is most disastrous to satisfactory management. I think both the papers show that the cost of lighting justifies a charge of at least 8d. and in some districts 1s. a unit. Electric lighting is cheap at that price, and people will pay it. Mr. Blaikie's paper is an excellent exposition of the reasons for differentiating between lighting and power charges, and explains them in a way which has never been employed before. I am, like Mr. Wright and other speakers, not altogether in agreement with Mr. Blaikie's method of grouping. He says that lighting is the 7-day group and power is the 6-day group. The "Sunday load" term has crept in because of the 47-hour week in the generating station, but I do not think that has made all the difference. We have to recognize that the principal load is the lighting load; it is the peak load, and any load, whether it be power or lighting, 6-day or 7-day, that comes on the peak must bear a higher charge than one which does not come on the peak at all. Mr. Blaikie wants to omit the 6-day lighting, but I consider this is equally a peak load and should also have a higher charge. Mr. Blaikie says on page 702 that the "capital charges are much less per kilowatt in the 6-day group, for many reasons." I do not agree that the capital charges are much less for shop lighting which, as Mr. Wright has pointed out, is one of the most disastrous loads possible. It is the cause of a great deal of our trouble. This statement is made on page 711: "Since we have seen that one group consumes nearly three times as much coal as the other, a coal clause based on the average consumption means that one group is being subsidized by the other. Obviously, each group should have its own coal clause." That answers the point that has been made several times recently with regard to increased charges. Those of us who had a coal clause have put up our costs by so much, perhaps ½d. or ¾d. per unit, because of the increased cost of coal. Several wiseacres have said: "Yes, you have raised the power load rate by ½d. or ¾d., but why, just because the cost of coal has gone up, have you raised your lighting rate 2d.?"; and it has been difficult to convince them. Mr. Blaikie has shown very well why it is necessary to charge a larger proportion throughout. I disagree with his statement on page 712, where in referring to the 7-day group he includes power. He puts his 7-day group in the analysis at £17 per kilowatt. I assume, therefore, that he would include a power load which was in the 7-day group at £17 per kilowatt, whereas if it were a 6-day load for power he would put it at £4 per kilowatt. A rate of £17 per kilowatt at 100 per cent load factor is equivalent to 0·466d. per unit. That would be his 7-day group. If exactly the same load is obtained only on 6 days a week, omitting the Sunday—that is, Mr. Blaikie's 6-day load—it is obtained at £4 per kilowatt. That 6-day load gives approximately an 85 per cent load factor if it runs for 6 days a week and every hour in the day. £4 a kilowatt on an 85 per cent load factor is equal to 0·13d. per unit; in other words, Mr. Blaikie suggests that because he runs all day on Sunday and increases his load factor from 85 to 100 per cent his fixed charge for every unit has to be increased from 0·13d. to 0·466d. I cannot agree with his contentions in that respect. Then he includes traction and public lighting as a 7-day group, but I certainly cannot agree with the sub-division that he puts in; nevertheless, I think his analysis very useful and one that will be very helpful to us in framing tariffs. Consumers at present have the option of choosing whatever they like, and unless, as Mr. Tuckett suggested, they can be compelled to adopt the multi-part tariff, and unless the maximum charge is sufficiently high, the multi-part tariff will not be as successful as we hope. Before it can be a success we must have reliable apparatus. I agree with what Mr. Tuckett said on the question of heating. I have maintained, and still maintain, that there is no proof that a certain amount of heating load is worth more to the undertaking than the same amount of power load. If that is so, why should less be charged for heating than for power?

Mr. E. W. Cowan: Both papers refer to the two factors which when taken together determine price, namely, the cost of service and the value of service. For many years consideration was given only to the influence of the former. The present papers give evidence that that attitude no longer prevails. The electric supply industry is, I think, gradually taking its place with other industries in endeavouring to adapt prices to market conditions, as only in this way can any industry be of the greatest advantage to the community. To consider cost of supply only is not incorrect, but it is inadequate. Unfortunately, there

has been in the past a tendency to regard consideration of the influence of value of service as involving neglect of the proper influence of the cost of service. That is a mistake that Dr. Hopkinson never made, and I am glad to note that Mr. Blaikie's paper supports efforts I have made in the past to dissociate Dr. Hopkinson's name from responsibility for rigidly basing tariffs upon cost of service. Both papers strike a note of freedom from that restricting and, I submit, unscientific method of arriving at tariffs. After all, the cost of service to any individual consumer is indeterminate. The aggregate cost of service is determinable for any selected period, but the cost of service to any one consumer is not. The most that can be done is to arrive at an approximate relation between the cost of different services. It is a useful plan to obtain those approximations, but they should only be treated as an influence in determining tariffs, or much advantage to the community will be lost. In making calculations as to the cost of supplying any consumer, it is usual to take the cost for, say, a year's supply. Some period must be taken, but the cost of supplying any consumer varies from hour to hour, from season to season, and from year to year. Such factors as the distance from the supply works are necessarily disregarded. But the most important factor which is disregarded is the measure of development consequent upon supply to any particular consumer. It is a latent factor, but a very real one. It cannot be determined precisely but, unless its existence is realized, growth—the growth which is a consequence of wise tariffs—will be lost to the undertaking. Mr. Blaikie suggests consideration of the cost of service to selected groups of consumers, and Mr. Beauchamp to blocks of consumers. That is, I think, a step in the right direction. Mr. Blaikie, however, goes rather too far in his desire to pay regard to the value-of-service factor. At the bottom of column 2 on page 710 he defines the "value-of-service" theory as meaning "simply getting as much as possible for the goods, having regard to the possibility of competition." I do not like that definition. It would embrace profiteering, and profiteering by a municipality, under the protection of a monopoly, is, to put it mildly, a very distasteful operation. The only legitimate use of the influence of value of service is in the differentiation of charges in some measure according to such value, not in increasing the aggregate or bulk of such charges. Further, one consumer or group of consumers should never be charged at a low rate at the expense of others charged at a higher rate. Then, about the middle of column 2 on page 711, he says: "In the opinion of the author the value-of-service theory is the only one of practical value, and the aim of the management should be substantial profits, with an eye to future competition. In the case of public authorities these profits can be applied to various reserve funds or to the relief of the rates." I do not agree with that, and I do not think that the general drift of his paper is consistent with the expression of such a view. It seems to me that the aim of such an undertaking should be the realization of the greatest advantage to the public. The detailed administration of the undertaking should be directed towards the realization of that aim, including the compilation of tariffs. As to the distribution of that advantage, personally I am in favour of its falling into the hands of the consumers, the undertaking retaining only such profit as may provide prudent reserves and a normal interest and sinking fund on the capital employed. The consumers should not be taxed for the relief of the non-consumers. Mr. Beauchamp, on page 715 of his paper, says: "Unfortunately, in many places to-day a consumer who pays less than £5 per annum is a source of loss to the undertaking." I consider that that statement is apt to be misleading. It does not follow that consumers, the cost of supplying whom appears in the cost account-book to show a loss, are really a loss to the undertaking. If the increment cost of supplying them is more than £5 there is a loss, but it does not follow that because the £5 does not cover that consumer's equal or proportionate share of the capital, management, and administration charges of the undertaking, his supply involves a loss. It may be a profitable supply. The result of my investigations has demonstrated to me that all businesses must include amongst their transactions many which show a loss according to this restricted method of computation. Indeed, I believe that the best business can be realized only by deliberately departing from such a principle as that of equal profit from all consumers on the basis of equal share of cost (capital and operating).

Mr. W. J. Minton: As a meter engineer I must thank the authors for showing me the problems that are ahead. Mr. Blaikie, in my opinion, is trying to perpetuate the faults of accountants. As mathematicians we ought to refuse to adopt the rule-of-thumb methods adopted by the average clerk. The "pluses" given at the top of page 712, column 2, lead to error. As anyone who has had anything to do with allowance for meter error in accounts knows, the error is caused by taking percentages in the wrong order. The "plus" should be substituted by "multiply." I have on a former occasion shown that £100 + 10 % − 10 % does not equal £100. It would therefore be better to rewrite the figures on page 712 as "multiply by $0{\cdot}666 \times 1{\cdot}1 \times 1{\cdot}15$." That is more simple and involves only one calculation. There is another point to which I should like to refer: Mr. Blaikie divides consumers into two groups. The 6-day group, to my mind, could be called the one-meter group; the 7-day group could be called the "multi-meter" group. It may sometimes be true, but not always, that inaccurate meters cancel each other out. Under the one-meter group falls the bulk supply, which is not mentioned at all. Bulk supply necessitates experts on the consumer's side who will see that the meter is accurate (or slow)—so there will not be a "fast" meter to cancel a "slow" one. In my opinion there should be a fourth clause added, called the "Meter Accuracy Clause." This meter accuracy should be obtained after a three months' run of the meter, or from the date when it was last repaired, and should be obtained once a year. It is worth the trouble when £50 000 or £100 000 is involved.

ADJOURNED DISCUSSION BEFORE THE INSTITUTION, 5 MAY, 1921.

Mr. A. N. Rye: Mr. Beauchamp's paper appeals particularly to me because I have to deal with numerous undertakings in small country and sea-side towns, which must depend entirely on the domestic load. One of the great problems that confront us to-day is how to obtain sufficient revenue per consumer, quite apart from the price per unit. The information in Mr. Beauchamp's paper and in Colonel Vignoles's communication (see page 733) seems to show that, for the small consumer at any rate, the maximum amount that is obtained per service is more important than the actual price per unit. The generating cost for these small consumers is trifling compared with the total costs when capital charges are included. My interest in the two-part tariff arose a good many years ago, when the metal-filament lamp was substituted for the carbon-filament lamp and the revenue from consumers began to drop very heavily. The company with which I am connected made very great efforts to develop other uses of electricity, and we adopted a two-part tariff, which was generally termed a contract tariff at that time. In one town this tariff in the course of three years became so popular that over 50 per cent of the total revenue from lighting consumers was derived from it. The principal reason that the contract tariff was so favourably received there was that it allowed small radiators to be connected to existing wiring in many cases and to be supplied through one meter. Unfortunately, with the altered conditions brought about by the war, our experience is that the two-part tariff is going out of use. I think the reason is an inadequate maximum price. In very few cases have we been able to get a maximum price allowed which really takes into account the changed conditions. Before the war the Provisional Order allowed us to charge, say, 8d. per unit. To-day after a great deal of effort and long delay we have perhaps got 10d. in some cases and 1s. in others—say, a 50 per cent increase. But we have found it necessary to increase the charges under the contract tariff in order to try to make ends meet, not by 50 per cent but by something more like 100 per cent, with the result that, in effect, we are trying to make the better-paying consumers pay the losses of the large block of consumers who are charged the maximum price per unit. They remain on the flat rate because we have no power to compel anyone to take the contract tariff or the two-part tariff if it does not suit his convenience to do so. Mr. Beauchamp has raised a most important point, namely, if a compulsory two-part tariff is impossible owing to legal restrictions, some consideration should be given, in the form of higher maximum prices, to those undertakings which operate a satisfactory two-part tariff. I had occasion to look up some figures in regard to one of our towns. They are not selected figures at all; the line of investigation was to take a certain number of consumers who happened to be in the same premises at two very widely separated periods of time, namely, 1905 and 1920. I took a dozen of those cases. In 1905 those consumers used approximately 9 500 units; in 1920 they used 5 000 units, the number of units per customer having dropped by one-half in that period due to the introduction of metal-filament lamps, to the Summer Time Act and, in certain cases, to the shorter shopping hours. Some of the cases relate to residences and others to shops; I took them quite indiscriminately. The price of 5d. per unit in 1905 in this particular group of consumers brought us in approximately £200. In 1920 the price had gone up to double that figure, viz. 10d., but the money received was only £3 more. I think that bears out the argument that tariffs for the small consumer to-day have to be considered very much more on the total amount of money per consumer than on the price per unit; and that argument, to my mind, is most forcibly illustrated by Colonel Vignoles's figures.

Mr. W. A. Gillott: It is only by closely studying the characteristics of the domestic load on the system that engineers are able to design a satisfactory tariff; by a satisfactory tariff I mean one that provides an adequate return to the supply authority and at the same time encourages the increased use of electricity for all purposes of lighting, heating and cooking, and is simple to understand. I think the last is essential. I am firmly convinced that the domestic load is only waiting for an attractive tariff to be put forward. A short time ago, on seeing a daily load curve of one of the London gas companies, I was struck by the enormous difference in the height of the peak on a Sunday compared with that on a weekday. It was very interesting to note that the output on the Sunday practically equalled that on a weekday, but in the case of the latter the period of the demand was naturally about 10 to 11 hours, whereas on Sunday the whole of the load occurred between 10.30 a.m. and 2.30 p.m. That proves the enormous amount of domestic use at midday, and the load comes on in the majority of cases when either the gas company or the electricity supply company can deal with it. I read before the Institution in 1914 and 1918 two papers* relating to electric cooking, in which I touched upon the question of tariffs. In each of those papers I gave a series of curves relating to the characteristics of the loads of various consumers, and they showed quite clearly, as far as the station supplying the energy was concerned, that the cooking load came on at the times the power station needed it, i.e. in the depressions in the load curve. The town I am referring to is Newcastle. Many tariffs were designed, and it was not until after four or five years' careful investigation that a reasonable tariff was established, and it was decided to make a fixed charge relative to the actual lighting consumption of the consumer. The watts per lampholder over some 6 000 installations averaged 40, and that formed the basis of the fixed charge, which was worked out at the rate of 6s. per lampholder per annum, chargeable upon 75 per cent of the lampholders installed, thus allowing for "convenience" lights. The possibility of half-

* "Electric Cooking and Heating in Private Houses," *Journal I.E.E.*, 1915, vol. 53, p. 42; and "Electrical Cooking as applied to Large Kitchens," *Ibid.*, 1918, vol. 56, p. 92.

watt lamps was foreseen and the fixed charge was based to compensate for that. The fixed charge covered the previous year's lighting units if charged at a rate less the equivalent of the low running charge, so that when the consumer used his lighting under the new system he paid exactly the same for light as he did in the previous year. The company was thus assured of its revenue, although it was offering a lower rate per unit on the running charge. It was only on the extra units that the consumer benefited, and I suggest it is the extra units that the supply companies need. As an illustration take my own case. At home I have roughly 20 kW installed, made up of lighting, heating, cooking and accessories. The maximum demand very seldom exceeds 4 kW, and the consumption is in the neighbourhood of 3 500 units per annum. The differences in the capital charges of the purely lighting consumer's services and meters compared with an installation similar to my own are so very small that the difference in the initial outlay is hardly worth consideration. The revenue the supply company receives from my house is approximately £30 per annum, whereas the revenue from the consumer next door, with a 2-wire service and a meter for lighting, is not more than £5 per annum. It is interesting to note that the diversity factor on the domestic cooking in the curves to which I have referred was somewhere about 9·1. As a result of the Newcastle tariffs, over 12 000 kW of electric cooking were installed, and the extra load was not noticed on the peak at all. Between noon and 12.30 p.m. a peculiar hump of about 2 000 kW appeared, and nothing further was seen of the cooking load until the next day, thus proving that the peak of the cooking coincided with the time when the works were shut down for the dinner hour. How the supply authorities base their fixed charge for the domestic tariff is mainly a matter of the local conditions, but an endeavour should be made to arrange the fixed charge so that the consumer can see its relationship to his own installation. We have seen cases where so much is charged per 100 sq. ft. of room area; in other cases the rateable value is used; in my own case it is a fixed charge on the number of kilowatts installed for lighting. These charges are understood and appreciated by consumers. The high diversity factor offered by electric cooking is such that one can afford to omit an extra kilowatt or fixed charge on cooking and heating appliances. The real aim of the electric supply authorities should be to increase their output, and the only way to do this is to offer a satisfactory and simple tariff, when all the load needed will be obtained. There is no doubt that the domestic load is the load of the future, and electrical engineers will soon realize that they must cater for it, as the public will demand electricity for cooking and heating in almost every case at no distant date. Thoroughly reliable cookers with every facility for quick replacement being available, there is no need to fear excessive maintenance charges.

Mr. W. R. Cooper: In Mr. Beauchamp's paper, with which I am very much in agreement, there is one point I should like particularly to support, and that is the importance of encouraging heat storage devices. For a good many years I have been advocating water-heating on this basis. The importance of water-heating on a small scale (apart from high load factor) lies in the fact that consumers will more readily adopt other electrical devices if the convenience of a continuous supply of hot water, which can be obtained electrically, is available. This is particularly the case in these days when there are so many domestic difficulties. Thus it is a question of expediency rather than the amount of revenue obtainable therefrom. It is impossible to gain this load on the ordinary tariff for low load factors, as the price becomes prohibitive. A two-part tariff, however, permits such a load to be handled, provided the running charge is sufficiently low. It may happen, however, that the running charge will not be low enough, and in any case dealings in tenths of a penny cannot be contemplated for the ordinary consumer. On the other hand, there is no difficulty in fixing, to a much greater accuracy, a quarterly charge for apparatus continually in circuit. Moreover, a quarterly charge can be adopted with any tariff, whether two-part or straight; the corresponding number of kilowatt-hours can be deducted from the quarterly bill and the remainder charged on the ordinary tariff, so that only a single meter is necessary.

Turning to Mr. Blaikie's paper, it will be readily agreed that the question of suitability of tariff is most important as an inducement to prospective consumers, but it is equally important that a tariff should be sound. There is no question as to the soundness of Hopkinson's basis for a tariff, provided that common-sense modifications in detail are introduced; but I do not feel convinced that the suggestion of dividing consumers into 6-day and 7-day groups is really sound. We must remember that once a station gives a supply it must be kept running, whether for light or power, and cannot be shut down during odd and inconvenient intervals. Although many single factory loads may not, on the average, run much beyond a single shift, the combined loads would necessitate two shifts in these days, even if the station were shut down at night time. It seems scarcely justifiable to suggest that the 6-day group would only require "roughly six shifts a week." This is surely penalizing the 7-day group. Finally, the author adopts a basis of one to two in favour of the 6-day group, both in labour and in capital. On this kind of argument a 5-day group would be let off still more lightly, and a factory running on two shifts would be saddled with a higher tariff because it would entail a second shift at the power station. This is surely fallacious. The argument of the author is all against the improvement of load factor through longer hours. In fact, the author goes so far as to say there is no gain in coal per kilowatt-hour by carrying the load factor beyond 35 per cent, and this is true if only a limited period per day is taken. We thus come to the penalizing of high load factor. Some differentiation in regard to capital is reasonable, because lighting consumers require a heavier expenditure on mains per kilowatt of demand than do power consumers, but I do not think that a distinction can be drawn in regard to station equipment on the ground that power consumers give an opportunity of employing large sets as against small ones. If small sets were used exclusively for

lighting and large sets for power in the same station, this distinction would be justifiable, but usually the same sets are used for both, and thus all classes of consumer benefit by the smaller outlay per kilowatt. Where the lighting consumer is at fault is that he provides only a small load at night, and such load as there is may be run at a high coal consumption per kilowatt-hour. But to urge that all 7-day consumers should be saddled with very high fixed charges is not justifiable. Unless we are to take a new view that off-peak load is of no value, then public lighting should be encouraged because it provides a night load. Domestic heating and cooking provides a good load, notwithstanding its 7-day characteristic, and equally so does electric traction. Electric lighting alone is undesirable. It appears to me that to claim all the virtues for the 6-day group leads to fallacious results, which may be demonstrated by asking any station engineer whether he would prefer to have a tramway load for six days a week or for seven. The reply must inevitably be in favour of the 7-day load. Why then saddle it with higher fixed charges per kilowatt of demand? With regard to coal, it is generally confusing to express fixed coal as coal per kilowatt-hour, because this must vary with the number of machines in use. It depends on the "utility factor." Similarly, it is doubtful whether "fixed coal" should be expressed on a maximum demand basis; for either the maximum demand varies without any variation in the fixed coal, or a small variation in the maximum demand may cause a considerable variation in the fixed coal. At one point in his paper the author proceeds to substantiate one set of figures for coal per kilowatt-hour against another set. But is substantiation necessary? The two can only coincide if no set is run for an appreciable time under-loaded. The difficulty is that the total fixed coal depends upon the duration of the load, and consequently cannot be treated satisfactorily on a kilowatt basis; in this respect it differs from capital charges. In fact, for many purposes, the "fixed coal" is variable and the running coal is fixed. The only coal item that can be logically treated on the basis of coal per kilowatt-hour is the running coal. The author proceeds to analyse coal consumption by the use of simultaneous equations. It is necessary to remember, however, that the use of such equations, if they are to be valid, depends upon the coal per kilowatt-hour remaining constant for each group in the two years taken. I doubt if this assumption can be made. This point is brought out by Fig. A, which gives the hourly characteristic of a station running three 1 000-kW sets, the running coal being 2·5 lb. per kilowatt-hour, the figure given by the author, but the "fixed coal" being taken as 1 000 lb. per hour for each set, instead of the author's higher figure (which seems unduly high). The characteristic line cuts the axis at A, B or C, depending on the number of machines running, and it varies abruptly when a machine is put on or taken off. The average for the year would be a line such as DE, assuming the conditions given by Mr. Blaikie. The upper part of the diagram gives the corresponding coal consumption per kilowatt-hour, and it is seen that this varies to a marked extent. If in one year the running were largely at a point G instead of F, or H instead of G, in the previous year, the coal per kilowatt-hour would be very different in the two years, and simultaneous equations would not hold. Another objection to the use of simultaneous equations in this way is that the percentage growth of an undertaking is often not very large. In that case we have to deal in comparatively small differences of large quantities, and the accuracy is doubtful. For example, if the growth is 10 per cent then an accuracy of 1 per cent in the initial figures becomes an accuracy of only 10 per cent in the final result. In many cases the growth is not as much as 10 per cent. Thus at West Ham in the year 1918–19 the 7-day total was 8·1 million kWh and the 6-day 30·1 million kWh. The corresponding figures for 1919–20 were 8·2 and 33·3 millions respectively. These lead to the result that the coal per

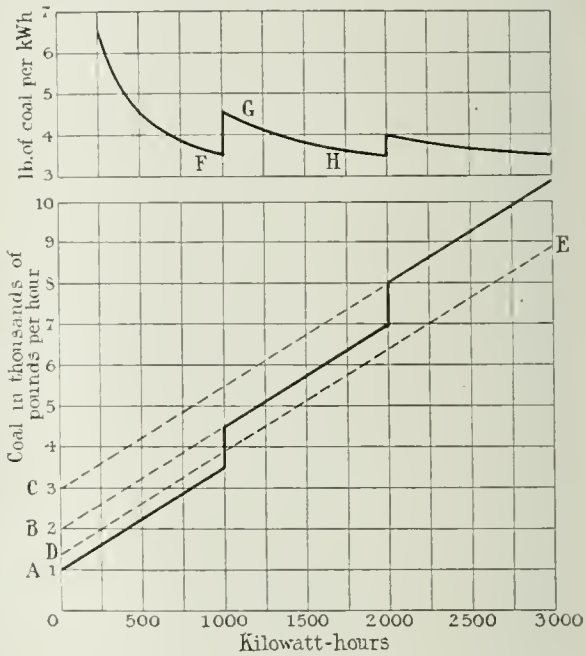

FIG. A.—Station characteristics (hourly).

kilowatt-hour for the 6-day load was approximately 2·57 lb. If now the output in 1919–20 had been 8·1 millions instead of 8·2, which would involve only a small percentage correction in the total coal, the calculated consumption changes from 2·57 to 2·96 lb., thus showing that the method must be applied with much caution, because small differences in the data may introduce large differences in the final results. Among the results obtained by equations I notice the figure of 40·7 lb. of coal per kilowatt-hour at West Ham, which seems extraordinarily high unless the conditions are very unusual. The figures in the tables indicate a high degree of accuracy, but I am not sure how this accuracy is demonstrated; between any two years that are taken for the purpose there can, of course, be no differences or errors. I think it would be a great advantage if Mr. Blaikie in his reply would give the working out of one of his examples so that the method

might be thoroughly understood and more exactly judged.

Mr. J. R. Dick: Mr. Blaikie has perhaps gone rather too far in discriminating between the two definite categories of 6-day and 7-day consumers, but the principle involved is quite sound. It is necessary to be more and more analytical with all the elements of costs and, although the actual figures obtained and the relative accuracy of the equations may be doubted, there is not the slightest question that a close scrutiny should be made of the labour, which is nearly all indirect in central station work. This element and the stand-by coal should be sub-divided and allocated in quite a different proportion from that given by the simple formula developed by Dr. Hopkinson and Mr. Arthur Wright, although it was quite sufficient in the early days to consider any costs in this class as equivalent to charges against investment. Another point I should like to comment upon in Mr. Blaikie's paper is his fear that, in the present abnormal monetary conditions, the effect of capital will become of diminishing importance in relation to the running and other charges. This, I think, is probably a mistake, because it is only a temporary phenomenon. The cost of plant, labour and coal are all, at any one epoch, functions of the prevailing currency, and when this becomes more stable the original relative proportions of all those factors in the general cost equation of electricity or of any other commodity will resume approximately their former values. It is evident from Mr. Beauchamp's paper that he is somewhat conscious of the danger of the heating and cooking load finally becoming exceedingly large compared with what it is now, in the by-product or temporary stage. If we want to be honest and deal with the matter on the basis of direct cost of production, we must face the time when the heating and cooking load will swamp the lighting load. In the distribution system, it will necessitate the adaptation of, and additions to, the existing network to deal with the heavier currents. For instance, in a suburban street with about 20 villas, it may be assumed that there will be a coincident maximum demand of about 2 kW each. If the street is 400 yards long, fed by a two-wire cable at 200 volts, it would require a 0·3-sq. in. section to keep within the 3 per cent limit of pressure-drop. Probably there is sufficient margin in the existing much smaller cable to take all the present heating and cooking load, but the possibility of heavier mains must be faced if this new business is going to preponderate, as we all hope it will. In order to overcome the excessive cost of the network due to heating and cooking loads it has been suggested that we should run all the distributors at a constant current-density; i.e. that Kelvin's law should be applied to the distributors as well as to the feeders and a constant current-density be maintained throughout, with a maximum loss of 10 or 12 per cent. It would be quite impossible to run the lighting on this system, and therefore there would be no alternative but to run two separate networks, although the duplication might mean less total costs. In considering the economy of working at constant current-density, it must be noted that the curve of efficiency representing Kelvin's law is very flat; that is to say, the efficiency as a function of the current density varies slowly, so that even when we have obtained a minimum value for the I^2R losses and the investment losses, we are still left with a very formidable amount of standing charges to be debited against the heating and cooking load.

Mr. W. L. Madgen: As is generally known, Electricity Commissioners have been appointed for the purpose of encouraging the supply of cheap and abundant electricity throughout the country. It would surely be thought that they would commence by administering a tonic to the distributing centres (by which I mean the business of supply in towns of varying sizes, some of them small) throughout the country, because, unless the local businesses are in a healthy financial state, it will be very many years before it will be worth while linking them up by main transmission lines. At present the business in these centres is not by any means in a healthy condition. In towns of moderate size, most of the middle-class members of the community have already been connected up at prices which yield some profit, but now that that market has been supplied those who are left are comparatively small consumers, while, as Mr. Beauchamp says in his paper, a connection which does not pay at least £5 per annum is a losing proposition. Consumers taking very much less than that are, however, being connected up even now and, as their number tends to increase, the profit made upon the class I first referred to is gradually being worked off, so that undertakings which are impelled to follow this blind policy must inevitably come to a financial deadlock. There is no antidote for that except some form of multi-part tariff, and there is not much sign of the Commissioners really grappling with this serious element in the situation. The President headed a representative deputation and urged the matter upon the Commissioners; he asked them to insert in what is called the No. 2 Electricity Bill a clause to authorize multi-part methods of charging, but they did not seem to be in favour of it, and we are still in a state of uncertainty as to whether they really mean to help us. I think there is nothing more important to the electric supply industry at the present time than that there should be some alleviation of the difficulties affecting the health of the distributing centres to which I have referred. I am concerned with a number of these small undertakings, and it is most difficult to get a young managing engineer to realize that he should not go out and get customers regardless of their probable consumption. It has to be forced upon him that it is more important to get existing consumers to increase the uses of electricity by employing auxiliary apparatus and by increasing their consumption, than it is to take on new consumers for lighting purposes only. The main elements in this matter are that the lamps in common use to-day consume only about $\frac{1}{2}$ watt per candle-power; we then have the Early Closing and Summer Time Acts which have largely restricted lighting during the summer months, so that shops and offices require little or no lighting during 6 months in the year. The position has become more serious for another reason. Since the country has been open to visitors from the Continent, it has been discovered

that, while the war was in progress, German and Austrian scientists had been working upon the improvement of the incandescent lamp and that lamps of still higher efficiency are on the horizon. The serious position that the supply authorities will be in when these lamps come into use will be readily appreciated. None of us can possibly take up the position of deprecating improvements in lamps, but it will be fatal to many of these undertakings if, before $\frac{1}{4}$-watt lamps arrive, a compensating form of tariff has not been authorized.

Mr. J. C. Elvy: We have heard very little so far about the actual registering instruments, meter manufacturers being significantly silent. Perhaps they do not regard Mr. Beauchamp's proposals seriously. While coal and other generating costs are calculated at under a fraction of 1 per cent, the condition of the meters, one of the most vital points on which the revenue depends, is frequently neglected. From the consumer's point of view I am entirely in agreement with Mr. Beauchamp. As the adoption of cooking, heating and power necessitates a larger meter, he will no doubt obtain some light free of charge. Say, for argument's sake, a consumer has a 200-volt 100-ampere direct-current meter; at 1/40th full load (2·5 amperes) it may be, at least, 10 per cent slow in registration, and most probably at 1/200th load (0·5 ampere) there is no registration at all. Some may contend that the meter may be $2\frac{1}{2}$ per cent fast on the higher loads. Having been a Corporation official, I fully appreciate the meter engineer's difficulties. On our supply we had flat rate, demand-indicator rate, double tariff, factory rate entailing an office rate, and a shop-frontage lighting rate, which was a concession for contributing to the street lighting. There was also a kilowatt-demand rate plus a charge per unit. On some three-wire installations there was quite a large number of meters. By carrying on an active campaign in the direction of overhauling meters and introducing a further number of Bastian meters for small lighting installations, we obtained the following results at the outbreak of war: Efficiency of sales 87·09 per cent of units generated, compared with the average efficiency of 81·3 per cent of all other London municipal undertakings. With millions of units this becomes an important item. The effect on revenue would not be so marked with lower-priced units, but if larger meters were used the total lighting units might not be registered. The difficulty of providing a safety fuse has been mentioned, but I do not think this is insurmountable. Safeguard the one set of rising mains, and arrange sub-circuits so that the lighting circuits are lightly fused and the power circuits heavily fused, within the correct limits. We must cheapen the cost of installations somehow. With regard to current limiters, will they perform their functions satisfactorily if called upon to operate infrequently?

Mr. H. M. Sayers: Mr. Blaikie's division of consumers into 6-day and 7-day groups has been criticized a great deal, but I think it is fairly logical, although some exception may be taken to it, especially to the putting of traction undertakings among the 7-day consumers. This makes the maximum demand charge per kilowatt for traction several times that for factory use. It has been my lot on several occasions to ascertain what should be the proper price for traction supply, and I have generally had the satisfaction of convincing the arbitrator that traction supply should be properly charged at a lower rate than most industrial loads. Mr. Blaikie takes the coal consumption for no-load running as 30 per cent of the full-load consumption. That is too high. I have tested it a good many times and never found it more than 20 per cent, which makes a material difference. His hypothetical station is taken as running a 1 000-kW set for the lighting load, which has a maximum demand of 800 kW. This means that, for 7 or 8 hours per day, a 1 000-kW set is running on a load of 150 to 200 kW. No station engineer would do that if it were avoidable. Probably Mr. Blaikie will say that the figures I have extracted are illustrations, and that the correct figures can always be worked out for any given case. I agree that they can. The simultaneous-equation methods used by Mr. Blaikie are not explained in detail, so cannot very well be criticized. I have found similar methods very successful in showing the ratio of the standing coal and other costs to the output coal and costs. Mr. Blaikie says that no name has been given for the "plant kilowatt-hours." Colonel Crompton suggested a good many years ago the term "running-plant load factor," and I have used the term myself. It is an important factor in economical generation. As to Mr. Beauchamp's views on tariffs, is it any good for the electrical supply industry of this country to talk about a tariff on the basis of the value of service? I do not think there is the slightest possibility of our getting anything but tariffs based on costs, because the whole of the electrical supply industry of this country is more or less under legislative regulation and is not in the position of even a regulated monopoly. Therefore the basis of cost and a proper return on the capital seems to be a fair basis and the only practicable one. The question of tariffs for domestic use is difficult, but there is another difficulty in the way of the great extension of domestic use, and that is the cost of apparatus and its installation. We know how the gas companies have met that difficulty, and I suggest that the electricity supply undertakings should follow their example. They will never get a large number of small consumers for other than lighting purposes unless the small consumers can get the apparatus either free or at a small rental charge; and I suggest that the place of the fixed rate which we otherwise call the demand or assessment rate might very well be taken by such a rental rate. It would appeal better to the consumer, who is apt to look upon a demand rate as a payment for no tangible consideration.

Mr. W. B. Woodhouse: I endorse Mr. Beauchamp's claims for a two-part tariff. We are considering, as Mr. Dick pointed out, the sale of only one particular commodity, and it is a sale under peculiar conditions. We are bound to supply electricity for all purposes, and as we are subject to that obligation I think we are entitled to ask that we should at least be in a position to charge the cost of being ready to supply in every case. The sale of electricity represents an annual turnover of possibly 20 per cent per annum

on the capital employed. The importance of capital charges, where the turnover is such a small proportion of the capital invested, is enormously greater than when a commodity such as soap is sold. Therefore I think we are entitled to ask, in fact to demand, either that we shall be relieved from the obligation of supplying an unprofitable customer, or that we shall be entitled to put forward a tariff which will give us a reasonable return. The alternative is that the people in the electricity supply business will go out of it. We cannot possibly continue to face the prospect of supplying light at to-day's rates. Charges of 8d. and 10d. a unit are talked of. Neither 8d. nor 1s., nor 2s., will pay the actual costs in a great many districts, and I think that position has to be faced. I feel that Mr. Beauchamp's paper is most timely, and I hope it will have the desired effect. A point of interest, which is perhaps a little more abstract, is whether we are entitled, in selling our commodity, to distinguish between the classes of user, between the incidence of use, and so on. I think we realize that we must divide our consumers into classes. Mr. Blaikie has given us an interesting example of such a division, but we must be able to say that in a particular class we may be entitled to make a higher profit or be content to take a smaller profit than we do for another class. If we do not do that we cannot find money for development, and the growth of the industry will be retarded.

Mr. R. W. L. Phillips: Mr. Cooper's remarks about the simultaneous equations are correct to an extent when there are two simultaneous equations with the differences small and the values of the terms high. But it will be noticed that Mr. Blaikie has taken a period when the differences are very great. The method has been applied to two cases where the differences are small, and the very close results that he gets go to show, I think, that that is one way to segregate our costs. I think the principle, as laid down in Mr. Blaikie's paper, which does not depend upon a 6-day and 7-day group entirely, but on some form of grouping, should be carried on with some idea of getting a real idea of our costs, which should be ascertained before we frame any tariffs. I should like to endorse what Mr. Madgen said, that what we want now is a real licence in the way of tariffs. We are quite out of date, and I think that if some standard form of tariff could be devised the state of things in the industry would improve.

Mr. E. S. Ritter: I should like to criticize page 707 of Mr. Blaikie's paper. The cost of a kilowatt-year at 100 per cent load factor works out at about £34 and £17 for the 7-day and 6-day groups respectively. Why do the two figures differ so greatly, unless some essential factors have been neglected? If the method is correct it should stand the test I have given. Why has £5·88 per kW not been taken for public lighting and sewage pumping, instead of £21·08? Is not sewage pumping a power load? Both papers and the discussion have demonstrated that a charge per kilowatt-hour, i.e. a single-part tariff, is unsound, as it does not give the lighting consumer the benefit of a reduced rate for long-hour or other uses of his supply; not does it enable short-hour or small consumers to be supplied at a profit, two most essential conditions for the further development of the domestic side of the business. Some simple and universal method of charging for the supply which, by measurement or otherwise, takes into account the costs incurred, irrespective of whether the energy is used for lighting, heating or power purposes, is wanted in place of a multitude of tariffs.

The President (Mr. Ll. B. Atkinson): The discussion has emphasized the necessity and propriety of changing over definitely our methods of charging to multi-part tariffs. The meetings that the industry and the profession have had with the Electricity Commissioners have all resulted in this: we have received fair words from the Commissioners but nothing has happened. I do not believe for one moment that the difficulty arises with the Commissioners, because I am sure that they understand the necessities of the industry quite as well as we do. Unfortunately there are always political considerations in connection with our industry, and whether we shall be able to get past the political obstacles is another question. All we can do is to push the matter along as well as we can. The Institution has another conference of the interested parties to-morrow, which has been called with a view to put forward representations, if necessary, in connection with the Bill, and the discussion this evening will, in my opinion, strengthen the hands of all those who take part in to-morrow's conference. Mr. Madgen has suggested that we may have to consider ¼-watt lamps in the future. It may be that by the time these lamps are available something else will have occurred. I was told the other day on good authority that the problem of getting energy from the atom has been solved. If that is so, a single-part tariff will meet the case, but it will be a fixed charge, and whether people use much or little electricity will not matter.

Written Communications to the Discussion.

Mr. H. Bentham (*communicated*): Mr. Beauchamp's paper is undoubtedly a clear and concise exposition of the case for the two-part tariff and a higher minimum charge, both of which deserve the warmest support. Owing to the Early Closing and Summer Time Acts, practically all shop lighting whether large or small is now unremunerative in small undertakings at the prevailing statutory maximum of 8d. per unit. In one undertaking with which I am familiar 28 per cent of the domestic consumers have an annual consumption of 80 units or less, and naturally, as the lamp efficiency improves, this percentage will grow and become still more unprofitable unless the alterations suggested by Mr. Beauchamp can be prescribed as a remedy.

Mr. Blaikie's paper is most useful in many respects, but I am afraid many of his premises are incorrect. The proposal of a 6-day factory supply is certainly hypothetical; it is not practicable. I should like

Mr. Blaikie to hear the views of our power consumers on such a suggestion, for they are very annoyed when we cut off the supply for three or four hours to make a new connection, even in these days of trade depression. Frequently a guaranteed supply for week-end repairs has enabled us to supplant large private installations, and I have in mind a works power station of 1 500 kW where we might not have obtained the business but for the excessive cost of running their own plant for the small week-end repair load. Apparently Mr. Blaikie proposes to give the customer the benefit of the diversity factor. Such a concession would destroy the principal weapon possessed by supply authorities when competing for the supply to large factories requiring 1 000 kW or more, and this suggestion coupled with a price based on a 6-day supply (actually 6-day costs) would undoubtedly secure the business, but at such a figure that the load would be unprofitable. By the proposals outlined by Mr. Blaikie power consumers both small and large seem to be entitled to a supply at the same figure if classified in the same group. Load factor, however, does not entirely control production costs in these extreme cases, and the obtaining of a large power consumer is a much more competitive proposition than one of a smaller category. Our lighting load is 2 per cent of the total load, and I should like to know if Mr. Blaikie would deal with our case on the lines proposed. The fixed charges given on page 702 closely approximate to my experience.

Fixed Charges in 1920.

	Author's figures	Actual results
	Per cent	Per cent
Coal	32	33
Other charges (including depreciation)	42	36
Capital..	26	31
Total	100	100

Mr. Blaikie's design of a basis for a perfect method of allocating the fixed and running charges does not appear very safe when ingenuous reasons are advanced at the end of his paper for departing from such a basis in the case of cooking and heating, promiscuous heating, small power consumers, etc., and the granting of tariffs to such consumers on more favourable terms.

Mr. S. E. Fedden (*communicated*) : The presentation of these papers emphasizes the difficulty of constructing a tariff applicable to all classes of consumers, and equally fair alike to consumer and undertaking. It will be readily agreed that a standing charge and a running charge, which is in effect a load-factor basis, is the ideally fair method of charging. The real problem is to find a satisfactory basis for the annual standing charge. The rateable value is satisfactory up to a certain point, but, when grounds and outbuildings are included, the annual charge is sometimes altogether out of proportion to the consuming capacity of the installation. The capacity of the installation is not a satisfactory basis, as it may easily and does actually restrict the adoption of fires, cooking ranges, and other apparatus with high ratings. Floor space—or rather part-floor space—of the premises to be supplied is another suggestion for fixing the annual charge, but in all the schemes put forward with this object the word "expediency" plays an important if not a predominating part, and the real object is to fix this annual charge at something which the consumer can afford to pay, and which will ensure to the undertaking an amount approximately equal to the revenue derived from the ordinary lighting consumer. If, then, this is the object, a two-rate system with an economic rate for lighting and a very low rate for additional uses would meet the case. There is, however, the objection of double wiring for the two rates retarding the use of small appliances with low ratings, which might otherwise be used on the lighting circuit. When larger appliances with high ratings are considered, the lighting circuits are of little value and it becomes necessary to install a special circuit. On the whole, it is perhaps preferable that the entire supply for every purpose should be at one rate. What I have now to say may not apply to undertakings where lighting or domestic use is the principal source of revenue; but where an overwhelming proportion of the units supplied are for power it will be interesting and instructive to examine arguments which are now being put forward in favour of a method of charging which is very aptly described as "Equal quantities of the same article at the same price." It must be understood, however, that I am not advocating the system, but merely putting forward the arguments used in its favour. It is asserted that the policy of municipalization involves the principle of equal service on equal terms to all citizens, and relies upon the working of the law of averages to enable this principle to be carried out. The load on a generating station is created at all hours of the day and night by the entire demand for every purpose. An undertaking does not supply light, or heat, or power, as separate and distinct commodities. It supplies units of electricity as and when required by each consumer. The name of the department acknowledges and confirms this statement of its functions. The cost of generating every unit at any given moment is the same to each consumer, and has no relation whatever to the use to which it is put when supplied. As a general statement it may be said that the demand for light, heat or power, is satisfied from the same cable. On many routes both the cost of the mains and the expenses of connecting the consumer have been extinguished by the portion of the sinking fund to which each customer contributes through the charges. Even should the customer prove to be a small and irregular consumer, the department is involved in no more trouble and expense than the actual cost of connection with the main, and save for this exception the small consumer at the worst is no more a disadvantage to the department than his neighbour who is not a consumer at all. The adoption of a uniform basis of charge to all consumers of electricity, with a sliding scale in favour of large consumers as far as this can be shown to be com-

COMPARATIVE FIGURES FOR THE YEAR 1920, RELATING TO (*a*) RESIDENTIAL DISTRICT, AND (*b*) INDUSTRIAL DISTRICT.

	(*a*) Residential	(*b*) Industrial
STATISTICAL.		
Mains—		
Street frontage (miles)	13·7	9·3
Total length laid (miles)	17·5	17·5
Weight of copper (tons)	33·3	101·0
Services—		
Number	613	492
Number of meters	794	669
Total capital cost of mains, services and meters	£30 035	£69 144
Consumers—		
Number	593	456
Per mile of mains street frontage	43·36	49·03
Per ton of copper	17·8	4·52
Per £100 of capital expenditure	1·97	0·66
Units supplied during 1920—		
Total	477 560	24 037 935
Per consumer	805	52 733
Per meter	601	35 931
Per yard of mains street frontage	19·8	1 468·6
Per ton of copper	14 341	238 000
Per £100 of capital expenditure	1 590	347 650
FINANCIAL.		
Revenue during 1920—		
Total	£6 693	£128 971
Per consumer	£11 5s. 7d.	£285 0s. 5d.
Per meter	£8 8s. 7d.	£192 15s. 7d.
Per yard of mains street frontage	5s. 6d.	£7 17s. 7d.
Per ton of copper	£201	£1 276 17s. 9d.
Per £100 capital expenditure on mains	£22 5s. 8d.	£186 9s. 4d.
Per unit supplied	3·365d.	1·288d.
Maximum demand	500 kW	10 000 kW
Capital costs—		
Generating plant at £13 per kW.	£6 500	£130 000
Mains, services and meters	£30 035	£69 144
Total	£36 535	£199 144
Interest and sinking fund at 10 % *per annum*—		
Per annum	£3 653 10s. 0d.	£19 914 8s. 0d.
Per unit supplied	1·837d.	0·198d.
Distribution, management and rates—		
Per annum, at £6 1s. 6d. per consumer	£3 602 9s. 5d.	£2 770 4s. 0d.
Per unit supplied	1·8d.	0·028d.
Income tax (divided proportionately to revenue)—		
Per annum	£335	£6 448
Per unit supplied	0·168d.	0·066d.
Generation—		
Per annum, at 0·81d. per unit	£1 501 10s. 3d.	£81 128 0s. 7d.
Per unit supplied	0·81d.	0·81d.
SUMMARY.		
Cost per annum—	£ s. d.	£ s. d.
Generation	1 501 10 3	81 128 0 7
Distribution, management and rates	3 602 9 5	2 770 4 0
Income tax	335 0 0	6 448 0 0
Interest and sinking fund	3 653 10 0	19 914 8 0
Total	9 092 9 8	110 260 12 7
Revenue per annum—	6 693 9 8	128 971 13 8
Loss	2 399 0 0	—
Profit	—	18 711 1 1
Cost per unit—		
Generation	0·81d.	0·81d.
Distribution, management and rates	1·8d.	0·028d.
Income tax	0·168d.	0·066d.
Interest and sinking fund	1·837d.	0·198d.
Total	4·615d.	1·102d.
Revenue per unit—	3·365d.	1·288d.
Loss	1·250d.	—
Profit	—	0·186d.

mercially sound, is a just and equitable apportionment of the working costs and capital charges of a municipal electricity undertaking. Even in the case of the largest consumers it cannot justly be held that the generating costs of the last thousand units are any less per unit than the first thousand. The argument employed to justify a high price to lighting consumers, based upon the alleged maintenance of stand-by plant, is not backed up by any definite information as to where such special reserve plant for lighting is located or what it consists of. A sudden demand for current to produce light is no more of a tax upon the department than a sudden demand for power or any other purpose, and any possible demand for lighting under any conceivable circumstances can bear but small relation to the productive capacity of the generating station. From every point of view it will be in the interests of the department to establish an equitable system based upon the actual character and bulk of the commodity supplied, irrespective of the use to which it is put, and the adoption of such a system will be a long step towards the realization of the hopes which actuated the corporation and citizens in the municipalization of the undertaking. A large proportion of commercial and manufacturing consumers of current for lighting are also users of current for power and heating as well. The absurdity of a differential charge in such cases is self-evident, and a system of computation which requires three different meters to measure a supply of exactly the same commodity in the same building cannot be considered to be an example of modern labour-saving organization. The ideal domestic supply ought also to include the use of current for all purposes, whether light, heat or power. Under existing rates not only can every conceivable inequality of charges be found side by side, but the alleged impossibility of supplying current for lighting at the same price as current for power seems to present no insuperable difficulty. Practically the whole of the enormous development during the past few years has been in the interests of power users, who might fairly be expected to accept the principles of municipalization together with its advantages and to recognize the principle of equal service for equal charge, which is at the least the ideal of all national, municipal and other statutory undertakings which are carried on for the common good. While it is gratifying to observe the enormous expansion as an adjunct to industrial enterprise, it must not be forgotten that one of the objects for which an undertaking exists is the betterment of the condition of the community at large, and it can hardly be doubted that a rearrangement of the charges for current on the basis of equal quantities of the same article at the same price would remove an existing injustice of no small degree, and give widely-felt encouragement to the more universal use of electricity for every suitable purpose.

With the object of making a comparison with reference to the cost of distribution for lighting and private supply, with that of a supply principally for power and furnace use, I have taken two districts of precisely the same area, i.e. $1\frac{1}{2}$ square miles in each case. Full details are given with reference to the cost of distribution of the supply in each of the districts, the first being a purely residential district, and the second a manufacturing district. In each case the cost of producing the current, including administration charges, has been added. It will be seen that the total costs of production and distribution in the residential district are 4·615d. per unit; and the cost of generation and distribution in the industrial district 1·102d. per unit. The average revenue from the residential district is 3·365d. per unit, which being deducted from the cost of production and distribution set out above shows a loss of 1·250d. per unit. If the maximum price for electricity be reduced to 2d. per unit, the loss will be increased to 2·6d. per unit. The revenue from the industrial district is an average of 1·288d. per unit.

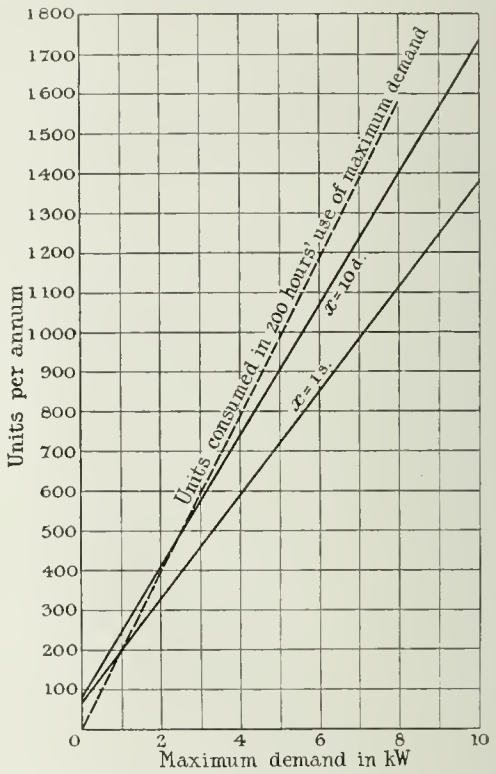

FIG. B.—Diagram showing minimum number of units at xd. per unit that must be consumed to cover standing charges.

Deducting the cost of production and distribution—1·102d. per unit—there is a profit of 0·186d. per unit in this case. Interest and sinking fund in respect of capital outlay are charged in each case. The cost of production of the current as it leaves the power station is charged at the same rate in each case, viz. 0·81d. per unit, but this is unduly favourable to the residential district, as it is obviously more economical to produce the unit with an output of 24 million units spread equally over each day than to produce the unit with an output of half a million units during the hours of lighting, which occur principally during the winter months. The supply for a residential district is required only for an average of 3 hours per day, whilst in the case of an industrial district the supply is required

for an average of 8 hours per day throughout the year. The result of the investigation goes to show quite conclusively that the loss on the cost of current supplied to the residential district has to be borne out of the profit received from the industrial district. The revenue figures used in both cases are for the full year ending 31st December, 1920.

Lieut.-Col. W. A. Vignoles (*communicated*): I entirely agree with Mr. Beauchamp as to "the urgent necessity of accepting the principle of multi-part charging for electricity supply." With the increased cost of mains and services, the cost of "being ready to supply a consumer" is a very serious problem, and one which presses very hardly on undertakings distributing electricity in districts where small houses predominate. Assuming that a consumer pays the whole cost of the service line on his premises, the actual cost of supplying him, without allowing any margin for profit, reserve fund or for plant the output of which is unsold, is, in Grimsby:

	£	s.	d.
(*a*) Fixed charges on service and mains per consumer	2	12	6
(*b*) Standing charges on plant, etc., per kW of maximum demand	5	10	0
(*c*) Cost of generation (average) per unit ..	0	0	2

These figures apply to short-hour consumers only, and the cost of generation is therefore taken as the average; it would be more correct, and would be necessary if long-hour consumers were being considered, to divide (*c*) into running and standing charges; this would decrease (*c*) but would increase (*b*). To illustrate the incidence of the above figures I give Fig. B, which shows the units that must be purchased by a consumer for a given maximum demand in order to cover fixed and standing charges. This figure also shows that, however small the consumer's installation, the undertaking will not be able to cover these charges unless he consumes about 100 units per annum. In 1920, 235 consumers, or 9 per cent of the total number connected in Grimsby, used less than 100 units; and out of 131 consecutive consumers in a main street, 51 (39 per cent) used less units than are shown on the diagram to be necessary to cover the fixed and standing charges. Considering "short-hour consumers" as distinct from "small consumers," it will be found that, with early closing and Summer Time, the time during which artificial light is required, in premises where daylight is not obstructed unduly, is approximately:

Offices: 100 to 150 hours per annum.
Shops: 200 to 250 hours per annum.

Fig. C has been drawn from the figures given above, and shows the relation between the cost per unit and the number of units sold per annum, (*a*) with a varying maximum demand, and (*b*) a varying number of hours' use of maximum demand. In considering this figure, it is to be noted that 28½ per cent of the total number of consumers in Grimsby use less than 250 units per annum. The curves show that the cost of supplying a consumer using light for 100 hours, and consuming 250 units per annum, is 18d. per unit, and that this rises to 21d. per unit if the consumption is 100 units per annum, as against a maximum statutory charge of 10d. per unit. The shape of the curves shows the

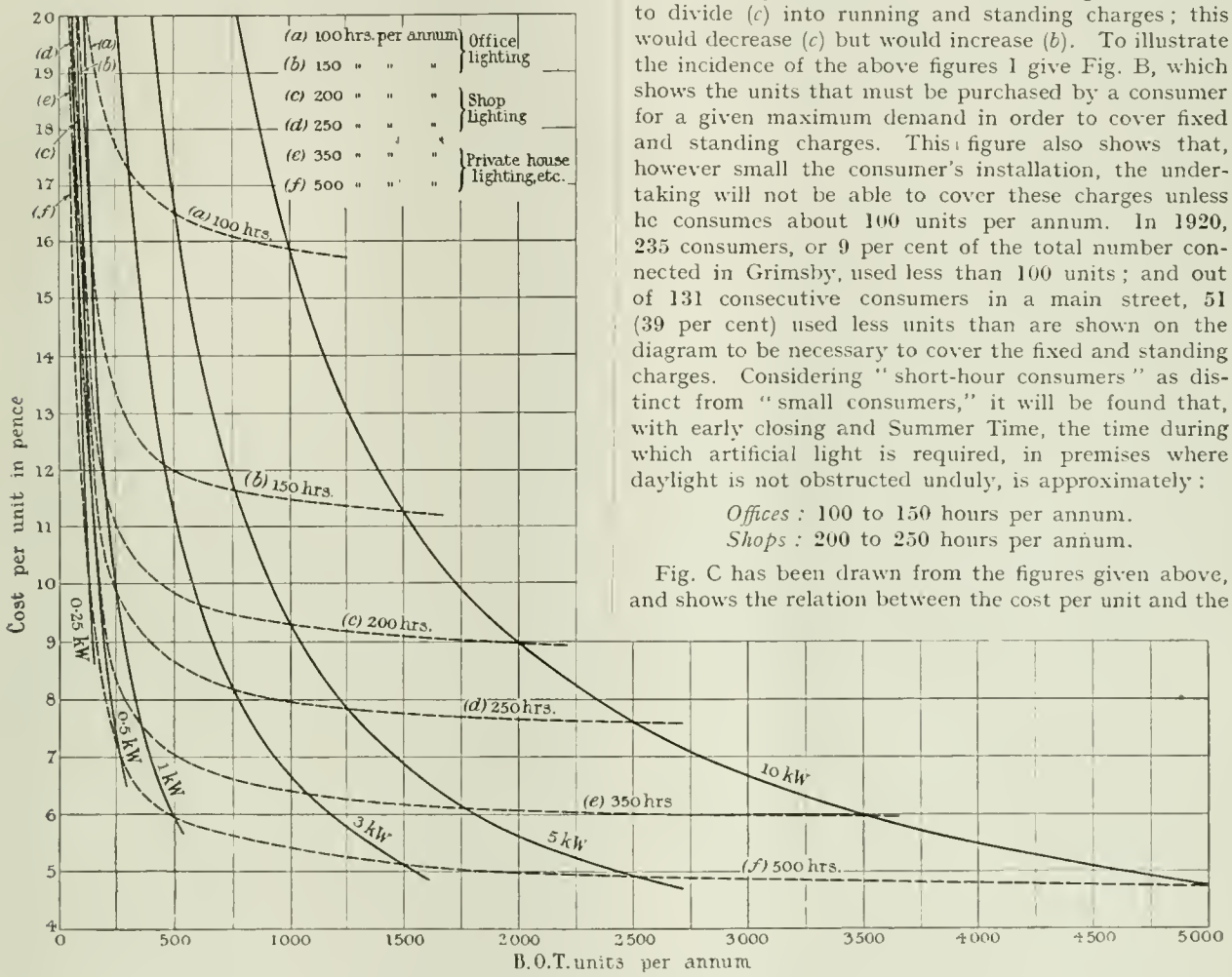

FIG. C.—Curves showing the cost per unit of supplying electricity to consumers at varying rates of maximum demand.

The dotted curves are derived from the others and show the cost incurred for any particular number of units consumed for maximum demands varying from 0·25 kW to 10 kW and maintained for the periods shown.

impossibility of fixing any rate per unit that would be fair to all classes of consumers; a two-part tariff appears to be the only solution. The figures make it clear that a loss is being made on the supply to certain consumers, which loss has to be made up by profits obtained from other classes of consumers. The general position in the electricity supply industry appears to be that charges for electricity supplied to the long-hour consumer cannot be further increased; that we have reached, or in some cases have passed, the price which the long-hour consumers are willing to pay, and that it is of the greatest importance that the small and short-hour consumers should cease to be a burden on the consumer who is the mainstay of supply undertakings.

Mr. J. R. Blaikie (*in reply, communicated*): Mr. Wright asks for a more detailed explanation of the proposal to divide consumers into 6- and 7-day groups. Perhaps the historical account of this development will prove to be of interest. During four consecutive war years at Bedford the steaming conditions were fairly uniform, but remarkable changes in the annual consumption of coal and units sold were observed. These changes could not be accounted for by variations in load factor, and an explanation seemed to lie with the alterations in the character of the demand brought about by war conditions. In the first year there was an abnormal demand for lighting, owing to large numbers of troops being quartered in the town. This was followed by a severe suppression of lighting, both public and private, as a precaution against air raids, and at the same time there was a rapid expansion in the demand for power for munition works. The actual figures of coal consumption and output are as follows:—

	Year	Units Sold	Tons of Coal Consumed
(1)	1914–15	3 065 682	11 429
(2)	1915–16	3 357 392	10 634
(3)	1916–17	4 115 733	11 565
(4)	1917–18	5 462 911	15 650

As explained in the paper, experiments were made with simultaneous equations; and a subdivision of the output into two groups, viz. 6- and 7-day demands, gave values of coal consumption per unit from which the coal bill could be reconstructed to within $2\frac{1}{2}$ per cent for each of the four years. A similar result was found by experimenting roughly with the figures from other undertakings, and is also observed in the early history of the Bristol undertaking many years prior to the war. The case was developed chiefly with the object of obtaining an intelligent idea of the rates of increase to be applied to various classes of consumers, necessitated by the enormous rises in the price of coal per ton. With the facts fairly well established, the hypothetical case was constructed, which I think explains the matter in a satisfactory manner and proves that broadly similar results must occur in every steam-driven undertaking. It is pointed out that in early days with cheap coal this effect was negligible, or did not arise when the supply was entirely for lighting. Mr. Dick agrees that this is so, and that at the present time it is desirable to subdivide the standing coal. Having divided the output in this manner to suit the coal it was found that, incidentally, the same grouping was suitable for subdivision of other charges and capital charges, and, further, that protective clauses could be based on the price per ton of coal. Both Mr. Wright and Mr. Dick question the justification in my application of the figures showing that at the present time capital charges in existing undertakings are about 35 to 40 per cent of the whole fixed charge, instead of 50 per cent or more as in pre-war days. They point out that this is a temporary effect. I quite agree, but in these disturbed times, when it may be necessary to correct tariffs every 6 months, it is important to have a clear conception of the costs at the moment. It is also probable that a large volume of pre-war capital will remain active for perhaps 10 years, and tariffs might be revised, say, once in 3 years under normal conditions. With this general explanation, I will now take the comments of speakers in the order in which they occur.

Mr. Wright makes a series of observations which provide material for a lengthy discussion in themselves. With regard to (*a*), I take it that Mr. Wright desires to emphasize the fact that small intermittent users contribute a great deal to the diversity factor, and should therefore share a larger proportion of the diversity-factor benefit than steady long-hour consumers. This might be effected by adjusting the fixed-charge rate to benefit the deserving classes. Observation (*b*) appears to require fuller detail. I presume that (*c*) is in the form of a meter or service rent, and I suggest that the policy must be governed to a large extent by the practice of the local gas competitor. With regard to (*d*), if, as Mr. Wright suggests, the profit should be chiefly derived from the running cost, surely the good-load-factor consumer is unduly taxed. I think that, if the traffic will bear it, it is more satisfactory to aim at the same percentage profit on the total cost of the units supplied to each consumer or to groups of similar consumers. I understand Mr. Wright to mean by (*e*) that a power-factor clause should apply to the power factor measured at the time of maximum demand. With this I quite agree. If a consumer is charged on a load-factor basis and runs his plant intermittently, he will have a low load factor and a low average power factor. He pays for the low load factor and, in doing so, hires the use of the plant serving his maximum demand. If he occupies this plant at intervals with wattless current he only tends to lower the diversity factor of the whole system, and the loss in diversity factor is usually shared by all consumers.

Mr. Purse criticizes the 6- and 7-day grouping chiefly from the point of view of capital. I gather that he agrees from the coal standpoint, or at any rate he appreciates the conclusion that lighting consumers require a larger share of the stand-by coal than the load factor indicates. With reference to shop lighting, I am convinced that it is not nearly so bad as the Hopkinson analysis leads us to suppose. The capital outlay on

mains is very largely influenced by the density of the demand. In the figures contributed by Mr. Fedden it will be seen that the distributing system costs about £60 per kilowatt of demand in a residential area, as against about £7 in an industrial area. I think it will be agreed that in a shopping area the density of the demand is much higher than in a residential area, and therefore the cost of mains will be considerably lower per kilowatt demanded. During the war, in many cases, shop lighting was suppressed and the plant was available and used for power purposes. If shop lighting were really as disastrous as is usually supposed, there should be a marked improvement in the finances of the year when it was removed and replaced by power. In Bedford no such result was observed and I shall be much surprised if any undertaking can trace any benefit from the omission of the shop-lighting load. Mr. Purse draws certain conclusions from the Bedford figures. I should point out that these figures are peculiar to the area and the character of the demand. They are not given as typical, but merely as a practical example carried through to a balance sheet. I may say, however, that this system applied to a series of Bedford accounts appears to fit the financial results uncommonly well, showing, for example, that the tariffs for power were a little too close to the cost of production and, therefore, that the financial result for the year depended on the fortunes of the lighting section. With reference to traction, it is probable that a traction load should be treated as a combination of the two groups; as a 7-day consumer, the average Sunday output would be multiplied by 365 (in which case it would have a very high load factor), and the balance would be considered to be a 6-day consumer. As the meters are read daily at the generating station, there would be no difficulty in this subdivision. It is also possible that some adjustment should be made on account of the number of units sold being the same as the units generated. In the majority of cases meters are placed at the supply end and not at the delivery end of the feeders.

Mr. Cowan, I am glad to find, generally endorses the views expressed in the paper as to the value of service, as a principle in framing tariffs. The points of difference appear to be my tendency towards profiteering, and the disposal of profits. The charge of profiteering is met by a paragraph on page 711 commencing "Having regard to the statutory limitations." The limitations in mind are those contained in the Provisional Order, section 55, headed "Application of Moneys received," which provide for a reduction in the price charged after certain specified profits have been obtained. From the same section I also draw the conclusion, in the case of public authorities, that the profits should be distributed amongst the ratepayers as a whole, and not amongst consumers, chiefly because the ratepayers are responsible for any loss on the undertaking. Incidentally, I believe that a strong financial position in the undertaking, having thereby no anxiety in extending mains or replacing obsolete plant, is also in the best interests of the consumer.

Mr. Minton draws attention to the inaccuracies that arise from the custom of expressing small differences as plus or minus percentages. I quite agree that a correction factor is better for instruments and precise work, but as employed in this paper I think that the popular method is more effective.

Mr. Cooper's criticism shows much careful consideration of the paper. After studying his comments I feel that there is really a very small difference in opinion. He says: "There is no question as to the soundness of Hopkinson's basis for a tariff, provided that commonsense modifications in detail are introduced." I thought that I had expressed precisely the same opinion, and it is therefore only a question as to what the modifications should be. With reference to the fixed coal, Mr. Cooper finds many difficulties, but he shows that a resultant can be obtained from quantities that vary hour by hour, and I feel sure he will agree that over long periods the fixed coal can be ascertained with some confidence. On the question of the figures used in the hypothetical case, perhaps Mr. Cooper has overlooked the practice used throughout of dealing with units sold and not with units generated. In this case the units used for iron losses in transformers, meter shunts and other continuous losses, all augment the fixed coal items. For a station having the loads suggested, I think that an overall consumption of 6 lb. per unit sold is not unreasonable. However, other figures may be substituted and broadly similar results will be found. The point I particularly wished to make is that the 7-day load, which is chiefly lighting, requires more fixed coal per kilowatt of demand than the 6-day load, which is chiefly power. Perhaps this will be apparent by considering the shape of the daily load curves. The familiar lighting-load curve resembles mountain peaks, whilst a purely power load is in rectangular blocks. It is possible to have two such load curves each having the same load factor, but there will be little doubt as to which requires more fixed coal. A tramway load has both characteristics and is therefore likely to require an amount of fixed coal somewhere between the two. The use of simultaneous equations is doubtful, I admit, and this is acknowledged in the paper. The results must be checked by other means. The coal, at Bedford, is checked by the observed Sunday consumption and also by the close reconstruction over 4 consecutive years. The wages item is checked by an entirely different form of analysis. Only as regards capital charges is there no check, but the quantities have large variations year by year, so that the results may be more reliable. Mr. Cooper agrees that some differentiation in regard to capital is reasonable. With regard to station equipment, I suggest that if lighting were the only load the development of the station would be marked by the addition of comparatively small units over lengthy intervals. With power the advance takes place in larger steps and justifies the installation of large units. The power load, therefore, is the reason for installing large units and deserves an advantage in the price per kilowatt of plant. On the point Mr. Cooper makes by asking any station engineer if he would prefer a tramway load for 6 days a week or 7, let us first suppose that the station supplies tramways only. If the station runs 7 days a week, undoubtedly the load factor will be higher. Now as to the three items of the total cost.

(1) Will 6- or 7-day load in tramway service give the lowest cost of coal per kilowatt-hour? *Answer*, 6-day.

(2) Will 6- or 7-day load give the lowest charge in wages per kilowatt-hour? *Answer*, 6-day.

(3) Will 6- or 7-day load give the lowest charge in capital per kilowatt-hour? *Answer*, 7-day.

It depends, therefore, on the relative costs of coal, labour, and capital where the advantage lies. In existing stations, under the conditions prevailing at the moment, coal and labour predominate. If the supply is from a general system the advantage may depend on the sizes and numbers of units of plant that the tramway load may require. That is to say, how much additional coal and how much additional labour. Mr. Cooper is under a misapprehension in reading 40·7 lb. of coal per kilowatt-hour at West Ham. This figure is only a ratio, assuming that the 6-day load was 10 lb., which figure was adopted as a common basis. I have much pleasure in giving the details of one of the examples of simultaneous equations.

	6-day Units Sold	7-day Units Sold	Tons of Coal
1915	1 871 456	1 194 226	11 429
1916	2 383 756	973 636	10 634
Average	2 127 606	1 083 931	11 031
1917	3 309 075	806 658	11 565
1918	4 493 140	969 771	15 650
Average	3 901 107	888 214	13 607

$$2\,127\,606x + 1\,083\,931y = 11\,031 \times 2\,240$$
$$3\,901\,107x + 888\,214y = 13\,607 \times 2\,240$$
$$y = 13{\cdot}5 \text{ lb.}, \quad x = 4{\cdot}73 \text{ lb.}$$

Note: 6-day = power and heating
7-day = private lighting, public lighting, and sewage pumping.

Mr. Dick considers that the principle adopted in the paper is sound, but that the methods used may be open to question. That is so, but the object of the paper is fulfilled in showing that the Hopkinson principles require revision, and in evolving some method of producing what may be called a Hopkinson balance sheet. There are undoubtedly other methods of segregating the data and of forming two or more groups with a separate fixed charge for each group. This sort of thing has been done for years in a private and unsatisfactory manner in arriving at low flat rates for special contracts. In such cases when the price of coal or rates of wage change there is no general method of revising the rates, and no certainty as to whether one particular consumer is profitable or not. Mr. Dick is mistaken in supposing that I fear that the effect of capital in the fixed charge will diminish; what I do say is that at the present time it may be about 30 per cent of the fixed charge instead of 50 per cent as in pre-war days, and that owing to the large amount of pre-war capital still active the rise in the capital component will be slow.

Mr. Sayers finds the division into 6- and 7-day groups fairly logical, but he raises questions very similar to those of Mr. Cooper. Perhaps the reply to Mr. Cooper will cover some of these points. I am glad to hear that Mr. Sayers has been very successful with the use of simultaneous equations. Provided that there are material differences in the quantities, and the results can be approximately checked by some other method, this method may be very useful. I think that Mr. Sayers will agree, on further consideration, that the modern practice of installing a small number of large units shows an advantage both in capital cost and also in steam consumption, notwithstanding the large waste of kilowatt-hours run that may occur in some districts. With reference to the term "running plant load factor," I presume that this is the same as plant load factor. Mr. E. Tremlett Carter in "Motive Power and Gearing," published in 1896, defines it as follows:—"The plant load factor, or the ratio of work done by any unit in any given time to the total work it could have done had it worked at full capacity during the time without intermission." Various applications of plant load factor are used in many ways, but chiefly, I believe, in daily records to see how the plant available fits the load, and to watch this influence on the coal consumption. The factor I had in mind is over the whole year, as in the usual case of load factor, and is the ratio of the units sold to the maximum possible output of the plant during the hours it is running. By the way, many of these terms, including load factor, seem to be lacking definition by a standard authority.

Mr. Woodhouse confirms the view that tariffs should not be based too rigidly on costs, but I take it that he approves the fullest possible inquiry into costs, for the use of the office in the first place, and for the advancement of general knowledge in the subject after practical testing.

Mr. Phillips points out that the simultaneous equations have been applied in cases selected on account of large differences, and therefore likely to be correct; also that the principle of grouping with different standing charges is of more importance than the merits of the grouping selected, as by such means we are enabled to get a real idea of the costs.

Mr. Ritter seems to find some anomaly in two groups having different fixed charges working out at different rates, each on 100 per cent load factor. I do not quite understand what the difficulty is. To my mind the whole question is the justification of assessing widely different fixed charges, but, if these are accepted, the results at any load factor must also be accepted. The only test I should apply is the value of service. Suppose, for example, that a form of analysis resulted in a charge of, say, 4d. per unit for a 25 per cent load factor. I should ask: Can the consumer generate current for himself at a lower rate? If it appears that he can, then this form of analysis is of no practical value. A case in point is the sewage pumping at Bedford. It is actually a 7-day load; it is classed as a 7-day load, and has a good load factor, but it is a comparatively small demand. The rate obtained is reasonable and

it is very doubtful whether any other form of power could do the work at a lower rate. In these circumstances there can be no objection to the rate. If the sewage pumping were a much larger user, then the fixed charge of the 7-day group would be lower and the rate per kilowatt-hour with the same load factor would also be lower.

Mr. Bentham's case of a power consumer who found it too costly to run his own plant on Sundays is a valuable illustration of a point I have been trying to make. If the consumer ran his plant on Sunday his load factor would be higher and, according to general belief, therefore his cost per kilowatt-hour should be lower. Apparently, this is not so. I believe that something of the sort is experienced in most power houses. Does not Sunday running tend to spoil the records? It is a common argument to say that "a power house has to run on Sundays anyhow, and the extra cost of taking on load is so small that we can do it at a specially low rate." There might be something in this argument if a low rate for power on Sundays attracted consumers, but I think it will be admitted that in practice nobody runs his works on Sundays if he can avoid it. If he must run his plant on Sundays he would pay a little more for the current. With plant running at uneconomical loads and extra wages it will probably be admitted that the costs per kilowatt-hour are higher on Sundays than on any other day, and in such case it seems absurd to sell current at the same or even a lower rate and charge the losses to all consumers. The reason that the power house has to be run is because certain consumers require a supply, and it seems logical to try to make such consumers pay the extra costs. Mr. Bentham says it is not practical in a factory. That is probably correct; it might cost more to ascertain the Sunday consumption than it would be worth. On the other hand, tramway loads and bulk supply can be easily allocated; also large groups of residental consumers and public lighting can be classified as 7-day loads. If an important volume of supply can be fairly charged on a 7-day basis there is no occasion for what Dr. Johnson calls "needless scrupulosity" in the case of repairs in factories, which are essentially 6-day consumers. It is also possible to construct tariffs that do not follow the costs too rigidly, by putting all the profit on the running charge. In this case the extra load factor due to Sunday running gets very little advantage. In Mr. Bentham's case with only 2 per cent of the total load used for lighting, some other form of grouping might be better. It depends, of course, on what his Sunday load is, and the possibility of identifying a substantial amount. I am glad to see that he finds similar proportions in the fixed charge to those given in the paper. With reference to heating, etc., the proposals put forward have no pretence to be perfect. I think that Mr. Bentham appreciates the idea of studying the fixed charge and, possibly, of establishing groups having different fixed charges, with a view to discovering the costs and the effects of changes in the price of coal and rates of wages. As for tariffs, I think I have made it clear that they should not be based too rigidly on the costs. The construction of a Hopkinson form of balance sheet is the principal object of the analysis.

Mr. Fedden's communication and figures are a most valuable addition to the discussion. With an overwhelming amount of power, which is probably nearly all in the neighbourhood of 20 per cent load factor, differentiation on a load-factor basis becomes almost unnecessary. That is to say, if we can assume that each consumer has a load factor of 20 per cent we can at once quote a flat rate. But it requires too much imagination to suppose that small domestic users have a load factor of 20 per cent or anything like it, and, unfortunately, the costs increase very seriously with decreasing load factor below this point. Still, with a very large bulk of power it may be justifiable to temper the extreme penalties of low load factor, and perhaps Mr. Fedden has this in mind in disregarding standing coal. With the remaining figures I have drawn up a comparison with the findings of the Bedford case. Broadly speaking, the 7- and 6-day groups in Bedford are very similar to the residential and industrial areas of Sheffield.

	Bedford.				Sheffield.			
	7-day Group		6-day Group		Residential		Industrial	
kW Demanded	762 kW		1 038 kW		500 kW		10 000 kW	
	Total	Per kW of Demand	Total	Per kW of Demand	Total	Per kW of Demand	Total	Per kW of Demand
	£	£	£	£	£	£	£	£
Coal	3 493	4·58	634	0·610	—	—	—	—
Other charges	5 424	7·125	1 412	1·362	3 937	7·86	9 218	0·922
Capital	4 530	5·94	3 072	2·959	3 653	7·3	19 914	1·991
Total	13 447	17·645	5 118	4·931	7 590	15·16	29 132	2·913
Ratio	10 to 2·8				10 to 1·91			

Comparing the results it is interesting to note that the relative rates between the two groups are more pronounced in Sheffield than in Bedford, both in "Other charges" and in "Capital." The greatest difference is in capital charges, but the methods employed are entirely different. Mr. Fedden takes the aggregate capital cost in each case and charges 10 per cent on the capital. In order to obtain a Hopkinson balance sheet I took the actual capital charges for the year, which of course do not include charges on loans that have been extinguished. But probably the difference is due to the very much greater divergence in the size of consumers of the two classes in Sheffield, in which case there appears to be a satisfactory comparison between the two undertakings. Other features in Mr. Fedden's figures are very astonishing and of great interest; for example, it will be seen that the capital cost of mains, services and meters amounts to about £60 per kilowatt of maximum demand in the residential area, and about £7 per kilowatt of maximum demand in the industrial area. In view of the fact that the two areas are of a fair size it may be justifiable to regard them as representative of the mains system in Sheffield, and to deduce values for the two principal factors governing the cost of mains, viz. length and current capacity. Let C = capital cost; L = length of street frontage in miles; D = maximum demand, and let a and b be constants.

$$\text{Then } C = aL + bD$$
$$£30\,035 = 13{\cdot}7a + 500b$$
$$£69\,144 = 9{\cdot}3a + 10\,000b$$

from which we obtain

$$a = £2008{\cdot}4, \quad \text{and} \quad b = £5{\cdot}046$$

a represents £1·14 per yard of street frontage, and b represents £5·046 per kilowatt of maximum demand on the system.

If these figures are reliable, there is a basis for a multi-part tariff which takes into account the very important factor of length of mains. The consumer might be charged

	£	s.	d.
On account of frontage, at per yard ..	—	—	—
On account of maximum demand, mains and generating plant, at per kW ..	—	—	—
On account of distribution and administration, per consumer	—	—	—
On account of electricity, at per unit ..	—	—	—
Total ..	—	—	—

that is, adopting Mr. Fedden's treatment of generating charges, etc. Bills presented in this form may appear to be complicated, but if there is any merit in educating the consumer it is surely worth the trouble in making out the bill in this form, and advantageous to tell him the same story at least four times a year. In the happy event of multi-part tariffs being legalized, it might be convenient to raise any one of the items as required by publishing a short notice.

Colonel Vignoles's communication hardly comes within the scope of my paper, which is chiefly concerned with the analysis of costs. I regret, therefore, that he omits the particulars on which the data used are founded. I think, however, he will agree that if any logical system can be devised for increasing the portion of the charge which is due for service, and consequently for decreasing the charge per kilowatt of demand, such charges would offer greater inducements for consumers to install small heating apparatus. The form of tariff sketched in the reply to Mr. Fedden will illustrate the point. The effect of a heavy direct tax, per kilowatt of demand, as in the original Hopkinson tariff, is referred to in the paper.

Mr. J. W. Beauchamp (*in reply*): With regard to Mr. Arthur Wright's remarks, rebates have been given in relation to total consumption, and frequently have been too large, e.g. where an increased consumption has not indicated an increased load factor. The Hopkinson theory with the amendments proposed by Mr. Wright is generally accepted as a sound basis of charging, but are we bound or even able to continue to apply it to each individual consumer or must we be content if it applies to groups of small users, leaving us free to charge individuals by simple methods which may in practice conform to Hopkinson principles over the average results obtained? If it be agreed that for any further large advance in electrification we shall require "salesmanship" in the widest sense of the term, including not only educative advertising but the supply of wiring and apparatus on easy terms, such provisions are not encouraged by the principle of selling on a purely cost basis. The idea that the "cost of supply" must fix the "charge for supply" has been largely responsible for the sluggish development of the selling side of the electricity business. "Value of service" is an admissible factor in any business that is not at the same time an absolute necessity to the public and a monopoly to the providers. Shop-window lighting is only poor business because we are obliged to sell electricity within a certain figure per unit, irrespective of its value to the user. Certain shops would pay double the present rates rather than give up electric lighting. We used to offer low prices for basement lighting in order to acquire a day load; now we find that basement rents have increased immensely because electric lighting and ventilating have made the basements almost as useful as naturally lighted rooms. A basement with a rental of £50 per annum can be well lighted and ventilated for £5 to £7 per annum. Without the ability to use electric light its rental might drop to one-half or less. Electricity converts a cellar into a room.

In reply to Mr. Couzens, the actual form and method of operating a supply tariff is probably of less importance than the energy and initiative of those in charge of it. So far as the domestic business is concerned, neither a low average rate nor a perfectly balanced tariff alone can bring success; it must be supported by help for the consumer in regard to the more expensive apparatus and its upkeep.

In reply to Mr. Tuckett, there has always been a marked difference between the competitive value of the return from a unit of electricity as used for

light and heat. This is accentuated by improvements in lamps, and entails increasing differences between the price of energy for lighting and for heating. These differences are not too easy to explain to consumers, and could disappear under a sound form of subscription tariff.

With regard to Mr. Purse's remarks, the full advantage of a single-meter tariff in regard to cost of wiring can only be enjoyed in the case of buildings wired with the knowledge that such a tariff will be available; it is then possible to make very substantial savings. There appear to be two schools of thought with regard to cooker practice: (1) To produce an infallible cooker, hand it to the consumer, and leave both the utensil and the customer alone. (2) To produce a more or less fallible cooker designed for quick replacement and repair with standard parts, and to keep in regular touch with the user in regard to maintenance and instruction in use. I believe we can only reach the first by way of the second; success to-day in electric cookery depends on the establishment of organizations to care for the apparatus and lead the users to understand and appreciate what is really a new method in the home.

In reply to Mr. Cowan, if it be admitted that the increment cost of any moderate increase in the number of connections (marginal consumers) is small, one must remember that each new connection is bringing nearer the day when a further unit of machinery, etc., will need to be installed and in its turn made remunerative by the gradual building up of still more connections. A transaction which though not yet a source of loss might become one if multiplied beyond a particular degree, is in the nature of "dumping" rather than "selling."

Mr. Minton referred to inaccurate meters. Such inaccuracies on large power supplies are often met by an arrangement of three meters and an agreement to accept the average readings of the two meters whose readings are most nearly alike.

I agree with Mr. Rye that, much as a multi-part tariff can help to build up a domestic load, it yet requires to be supplemented by every possible means of introducing to the consumer the advantages of general electrification. It is by using electricity for purposes other than lighting that we realize the simplicity and economy of this method of charging. It was noticeable in many instances that when circumstances led suppliers to increase their two-part tariffs, often by doubling both the fixed and running charges, the system tended to fail on purely lighting businesses, whilst the consumers who used electricity for a number of different purposes were much less dissatisfied and still obtained advantages from this method of charging.

In reply to Mr. Gillott, I have suggested for consideration that a multi-part tariff should provide for increasing the fixed charge in relation to large increases in connected load (i.e. potential increase in demand), because I feel that there is a weakness in basing it on lighting only; it may lead to the running rate being made too high, or to the tariff not allowing sufficient for liberal expenditure on distribution works.

With reference to Mr. Cooper's remarks, there is an evident reluctance on the part of suppliers to quote special rates for so-called 100 per cent load-factor apparatus. The composite tariff takes care of such load automatically, even if no further concession is contemplated for "restricted hour" use.

In reply to Mr. Dick, I look to a basis for the fixed charges broader than that for lighting only, to enable copper to be laid for the growing domestic load; and to future development of heat-storage apparatus to obtain the maximum duty from that amount of copper.

The contributions to the discussion by Messrs. Madgen, Elvy, Sayers, Woodhouse, Phillips, Ritter, Bentham, and Lieut.-Col. Vignoles are in the nature of valuable additions to the matter and arguments contained in the paper and call for no reply by the author.

Mr. Fedden has put on record arguments which he does not advocate and which, moreover, have probably caused him a considerable amount of work; they are of interest as summing up a minority opposition, which arises from time to time in different districts, to the recognized basis of charging. Perhaps in time to come we may sell electricity for all purposes at a single flat rate; at present it is necessary to consider only what would happen if such a principle were suddenly introduced into a large mixed industrial and residential area. The increase in the cost of power and the decrease in the charge for lighting would probably soon result in destroying most of the industrial load and leaving the public with its lighting at a rate which, though prohibitively high, could not save the undertaking from bankruptcy.

These theories when honestly held arise from failure to appreciate the economics of the electric supply business. The idea of treating all consumers alike as a matter of equity seems to make it difficult for some to recognize that in buying electricity (i.e. sharing the upkeep cost of the undertaking) it is only by highly differentiated rates and charges that the burden can be equitably adjusted to each consumer in accordance with the expense to which his particular service commits the supplier.

ABNORMAL PRESSURE-RISE IN TRANSFORMERS, AND ITS REMEDY.*

By R. Torikai, Associate Member.

(*Paper received* 11 *January*, 1921.)

Summary.

In a high-tension transformer, abnormal potential gradients likely to cause a breakdown appear usually at the foot of the bushing and at the end coils. The author describes briefly a theory which is applicable to both these cases and from which he concludes that these dangers may be due to the incorrect arrangement of electrostatic capacities. Following this theory, he shows that, by properly grading the capacities, it is possible to avoid to a great extent the dangers in both the bushing and the end coils. The correct gradation of the capacities is shown to be as follows:—

(1) The shunt capacity (C_s) must be graded so as to increase with the distance from the earthed end of the coil, or from the top of the bushing.
(2) The capacity (C_g) to earth, or to the high-tension conductor, should be graded so as to decrease with the distance.
(3) The ratio C_s/C_g should be as great as possible.
(4) It is preferable to use a resistance in series with the capacity C_s.

Experiments show that where a transformer is exposed to excessive stress a potential gradient of about 20 times the normal value may occur at the end coil, and this can be reduced to one-third this value by the device proposed. The author's method also prevents breakdowns due to other causes, such as the decrease in the natural frequency of the coil.

I. Introduction.

In transformers, especially in high-tension testing transformers, accidents take place most frequently at the bushing and coils adjacent to the end terminals.

(*a*) *Accidents at the bushing.*—These may be of two different kinds; one is the puncture of the insulating material, and the other electric discharge along its surface, i.e. surface creeping. Both may be due to either of the following two causes: (1) An abnormal high potential of many times the normal value may appear at the end terminal. (2) Even in the case of comparatively low terminal potentials, owing to the irregular distribution of the potential gradient a very intense electric field may be set up at a certain point on the surface or in the bushing.

The abnormal rise of terminal potential may be caused by circumstances in the external circuit; for example, the propagation of electric waves of high potential from the external circuit, electric oscillations taking place in the transformer coils or in another circuit connected to the bushing, etc., or the electrostatic effect of the bushing in conjunction with the electromagnetic action of the circuits connected to it, and it is never due to the bushing alone.

Irregular distribution of the potential gradient, however, depends on the construction of the bushing itself.

In either case, the problems of puncture and surface leakage are electrostatic, although the electrostatic capacities may vary according to the frequency of the applied potential difference.

The most popular remedy for these defects is the so-called condenser-type bushing. In its design the internal and external field distributions may be made uniform by suitably arranging the capacities of concentric cylindrical condensers, and by aid of the static shield; this type of bushing is very ingenious. In the author's opinion, however, the effect of the electrostatic shield upon the control of the external field of the bushing is less than the inventor claims. The author made several experiments on the action of this shield, and found that its effect is very small, even when its radius is greater than the height of the bushing. This can easily be explained as follows: The air space offers a high resistance to the flow of lines of force. Then the static shield cannot affect the distribution of the electric field at the foot of the bushing, being too far from it. If the shield can act on the field near to the wall of the tank, the high-potential conductor to which the shield is connected, and whose potential is equal to that of the shield, will act on the lines of force at the foot of the bushing, because the conductor is much nearer than the shield. This explains the small effect which the shield has.

In the light of these considerations, the author suggests a new method which will almost entirely avoid the surface creepage at the bushing.

(*b*) *The problem of end-coil short-circuiting.*—The theory of the distribution of potential and current in the coils of a transformer has been studied by various authorities, usually in the same manner as that of long transmission lines, that is, by considering only the self-induction and capacity to earth and by neglecting the capacities between adjacent turns or coils. Such methods, however, do not explain why the end coils should be subjected to the greatest stress. Some investigators consider this effect to be due to the action of the initial steepness of the travelling wave, which becomes less as the wave travels further from the end coils. But the time during which a wave traverses a few turns or coils is exceedingly small. In some cases the wave-front may destroy end coils, but this cannot be regarded as the general explanation.

K. W. Wagner * and O. Böhm † proposed new theories, taking capacities between adjacent turns into account, and ingeniously explained the reason for the

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* *Electrotechnik und Maschinenbau*, 1915, vol. 33, p. 89.
† *Archiv für Elektrotechnik* 1917 vol. 5, p. 383.

dangerous potential differences which occur at the end coil.

About the same time as Wagner, the author was engaged on a similar problem, without being aware of the former's work, and obtained some interesting results, which differ in some respects from those given by Wagner. In this paper the author describes his new designs of bushing and transformer coils, and gives the theory which he followed.

II. New Method of Design.

(*a*) *Bushing.*—As shown in Fig 1, outside the ordinary bushing (whether plain or condenser) a number

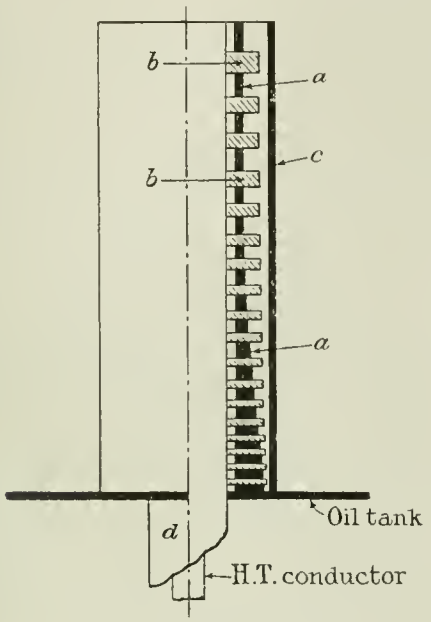


Fig. 1.

of conducting rings and insulating discs are arranged on the same axis. In this figure, *a* is a conductor ring; *b* is an insulating disc, which must be such that it completely insulates the adjacent conducting rings; *c* is a tube of glass (or other insulating material), which is filled with oil, so that the rings and discs are completely immersed in oil. In this respect, the author's bushing is somewhat similar to the so-called oil-bushing; but it is unnecessary to use oil except in the case of very high potentials; *d* is an ordinary bushing, around which *a* and *b* are built up. The shapes of the rings and the distances between adjacent rings are such that—

(1) The ratio of the capacity (C_s) of the condenser formed by any ring and the adjacent ring, to the capacity (C_g) of the same ring and the high-tension conductor, is as great as possible.

(2) The capacity (C_s) shall have the smallest value at the top of the bushing, the greatest value at the bottom, and at intermediate positions vary between these two extreme values.

(3) The capacity C_g (see Fig. 2) shall have the greatest value at the top of the bushing, and the smallest value at the bottom, and vary between these two extreme values at intermediate positions.

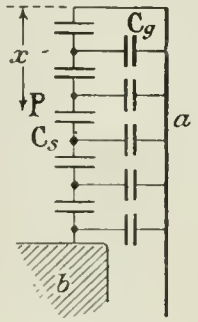


Fig. 2.

(*b*) *Coils.*—Of the total number of coils, about one-quarter or one-third, counting from the end terminal, are shunted individually, by means of a capacity and resistance in series, as shown in Fig. 3. The capacities shunting individual coils are graded from the end coil to the inner coils so that the greatest capacity is at the end coil. The coils are to be wound and mounted in such a manner that the capacities (C_g) formed by coils against the core are as small as possible, and that they are graded from the end coil to the neutral

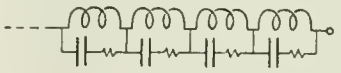


Fig. 3.

end, so that the smallest capacity is formed by the end coil, i.e. at the opposite end to the smallest shunt capacity.

III. General Theory.

We shall now consider a system of condensers connected as in Fig. 2.

Let *a* and *b* be conductors, the potentials of which are V_1 and V_2 respectively, and assume that the potential difference between them is $V_2 - V_1 = V_0$.

Let x be the distance (or the number of condensers connected in series between the point "*a*" and the point P in the C_s group) between the points P and "*a*."

Let V be the potential difference of the point P above V_1,
C_s be the capacity of the C_s group per unit length of x, and
C_g be the capacity of the C_g group per unit length of x.

Then the capacities of the element of length dx are C_s/dx and C_g/dx respectively. Therefore we have

$$\frac{d}{dx}\left(\frac{C_s}{dx}dV\right)dx = C_g V dx,$$

i.e.
$$C_s\frac{d^2V}{dx^2} + \frac{dC_s}{dx}\cdot\frac{dV}{dx} - C_g V = 0 \quad . \quad . \quad . \quad (1)$$

C_s and C_g are both dependent on x.

The general solution of Equation (1) is difficult ; but our purpose is only to investigate the change in distribution of potential according to the values of C_s and C_g or the mode of their dependence upon x, and therefore we shall give C_s and C_g any convenient values to enable us to solve Equation (1) easily and investigate sufficiently the relation of C_s and C_g to V.

(a) *The case where C_s and C_g are both constant.*— This case was the only one studied by Wagner and Böhm.

We have $$\frac{dC_s}{dx}\cdot\frac{dV}{dx}=0,$$

So that Equation (1) becomes

$$C_s\frac{d^2V}{dx^2}=C_gV. \quad . \quad . \quad . \quad . \quad (1a)$$

The solution of this equation is

$$V=A_1e^{\sqrt{(C_g/C_s)}x}+A_2e^{-\sqrt{(C_g/C_s)}x} \quad . \quad . \quad (2)$$

when $x=0$, $V=0$; and when $x=l$, $V=V_2-V_1=V_0$

so that $$0=A_1+A_2,$$

$$V_0=A_1e^{\sqrt{(C_g/C_s)}l}+A_2e^{-\sqrt{(C_g/C_s)}l}$$

therefore $$\left.\begin{aligned}A_1&=\frac{V_2-V_1}{e^{\sqrt{(C_g/C_s)}l}-e^{-\sqrt{(C_g/C_s)}l}}\\A_2&=-A_1\end{aligned}\right\} \quad . \quad . \quad . \quad (3)$$

$$V=-\frac{V_1-V_2}{e^{\sqrt{(C_g/C_s)}l}-e^{-\sqrt{(C_g/C_s)}l}}(e^{\sqrt{(C_g/C_s)}x}-e^{-\sqrt{(C_g/C_s)}x}) \quad (4)$$

Let E be the potential at the point P, then

$$E=V_1-\frac{V_1-V_2}{e^{\sqrt{(C_g/C_s)}l}-e^{-\sqrt{(C_g/C_s)}l}}(e^{\sqrt{(C_g/C_s)}x}-e^{-\sqrt{(C_g/C_s)}x}) \quad (5)$$

When the conductor b is earthed we have

$$E=V_1-\frac{V_1}{e^{\sqrt{(C_g/C_s)}l}-e^{-\sqrt{(C_g/C_s)}l}}(e^{\sqrt{(C_g/C_s)}x}-e^{-\sqrt{(C_g/C_s)}x}) \quad . \quad . \quad (5a)$$

The potential gradient along x is

$$-\frac{dE}{dx}=\sqrt{(C_g/C_s)}A_1\left(e^{\sqrt{(C_g/C_s)}x}+\frac{1}{e^{\sqrt{(C_g/C_s)}x}}\right) \quad . \quad (6)$$

From (6) we know that:

(1) The potential gradient along x increases with the coefficient $\sqrt{(C_g/C_s)}$,

(2) The maximum value of the potential gradient $\left(\frac{dE}{dx}\right)_{max.}$ occurs when $x=l$, and its value is

$$\sqrt{(C_g/C_s)}A_1\left(e^{\sqrt{(C_g/C_s)}l}+\frac{1}{e^{\sqrt{(C_g/C_s)}l}}\right)$$

$\left(\frac{dE}{dx}\right)_{max.}$ increases with $\sqrt{(C_g/C_s)}$.

Curves 1 to 6 in Fig. 4 represent potential distributions at various values of $\sqrt{(C_s/C_g)}$, taking V_2 at zero, i.e. earthing the conductor b, and taking l at 5.

Curve	Value of C_s/C_g
1	4
2	2
3	1
4	$\frac{1}{2}$
5	$\frac{1}{4}$
6	$\frac{1}{8}$

From these curves we see that the potential gradient, and consequently its maximum value which appears at the end point $x=l$, decrease with $\sqrt{(C_g/C_s)}$, and in the limit, when C_s becomes so great that C_g may be considered to be zero compared with C_s, the potential distribution curve approximates to a straight line, i.e. the potential gradient is uniform.

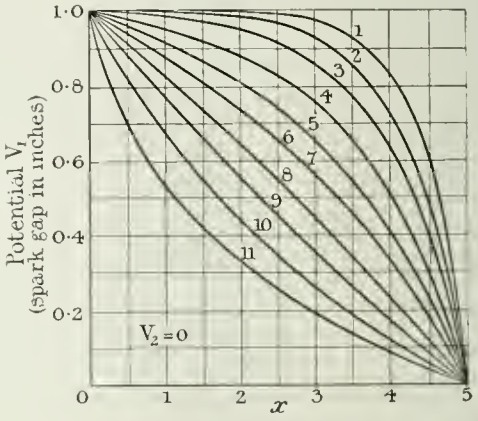


FIG. 4.

From the above we know also that, in the case of a system of condensers as shown in Fig. 2, with constant values of C_s and C_g, we cannot have a uniform distribution of potential unless we make C_s large compared with C_g.

(2) *The case where C_s and C_g are both functions of x.*—

We have $$C_s=f(x),\ C_g=g(x).$$

Now consider the cases where

$$f(x)=ax^n,$$
$$g(x)=bx^{n-2}.$$

These assumptions are convenient for solving Equation (1), and also for investigating the variations of potential distribution with those of C_s and C_g.

With the above assumptions Equation (1) becomes

$$ax^n\frac{d^2V}{dx^2}+anx^{n-1}\frac{dV}{dx}-bx^{n-2}V=0 \quad . \quad (1b)$$

Then $$V = A_1 x^{m_1} + A_2 x^{m_2}$$

where
$$\left.\begin{aligned} m_1 &= -\frac{n-1}{2} + \frac{1}{2}\sqrt{\left(\frac{4b + a(n-1)^2}{a}\right)} \\ m_2 &= -\frac{n-1}{2} - \frac{1}{2}\sqrt{\left(\frac{4b + a(n-1)^2}{a}\right)} \end{aligned}\right\} \quad . \quad (7)$$

If we let $a = b = 1$, then

$$\left.\begin{aligned} m_1 &= -\frac{n-1}{2} + \tfrac{1}{2}\sqrt{[4 + (n-1)^2]} \\ m_2 &= -\frac{n-1}{2} - \tfrac{1}{2}\sqrt{[4 + (n-1)^2]} \end{aligned}\right\} \quad . \quad (8)$$

In Equations (7) and (8), m_2 is always negative for any value of n. Assume the absolute value of m_2 to be m', then, when $x = 0$,

$$V = 0 = A_1 x^{m_1} + \frac{A_2}{x^{m'}}$$

In order that the last equation may hold good, A_2 must be an infinitesimal of the second order, or, generally speaking, the term whose power index is negative must vanish ; consequently, the potential is expressed by

$$V = Ax^m \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where m is positive.
Then, when $x = l$, $V_2 - V_1 = Al^m$ from which we get

$$A = \frac{V_2 - V_1}{l^m} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Therefore
$$V = -\frac{V_1 - V_2}{l^m} x^m \quad . \quad . \quad . \quad (11)$$

and
$$E = V_1 - \frac{V_1 - V_2}{l^m} x^m \quad . \quad . \quad . \quad (12)$$

When the conductor b is earthed,

$$E = V_1 - \frac{V_1}{l^m} x^m \quad . \quad . \quad . \quad . \quad . \quad (12a)$$

and the potential gradient will be

$$-\frac{dE}{dx} = \frac{mV_1}{l^m} x^{m-1} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

From Equation (13) we have :

(1) When m is greater than unity the potential gradient is a maximum at the end point, $x = l$;
(2) When m is equal to unity the potential gradient is uniform along x ; and
(3) When m is less than unity the potential gradient is a maximum at the end point, $x = 0$.

Now let us investigate various values of n.

(*a*) When $n = 0$; $m = 1 \cdot 62$, $C_s = 1$, $C_g = 1/x^2$, i.e. C_s is constant, and C_g is inversely proportional to x^2.

(*b*) When $n = 1$; $m = 1$, $C_s = x$, $C_g = 1/x$, i.e. C_s is proportional to, and C_g inversely proportional to, x.

(*c*) When $n = 2$; $m = 0 \cdot 62$, $C_s = x^2$, $C_g = 1$, i.e. C_g is constant and C_s is proportional to x^2.

(*d*) When $n = 3$; $m = 0 \cdot 415$, $C_s = x^3$, $C_g = x$, i.e. both C_s and C_g are proportional to x.

These various cases are shown in Fig. 5, taking $l = 5$.

From the above we can easily deduce that the potential distribution along x can be varied by altering C_s and C_g.

When $a = 1$, $b = 1$, and $n = 1$, then $C_s = x$, $C_g = 1/x$, and the potential gradient is uniform ; in the cases where C_s and C_g are constant we cannot have a uniform potential gradient unless we make C_s very large compared with C_g, which is practically impossible. In order to obtain a uniform potential gradient in a given system of condensers as shown in Fig. 2, the best plan is to grade C_s and C_g.

We shall now proceed to discuss more fully how C_s and C_g may be graded. Generally, the method depends upon the values of a and b. The results

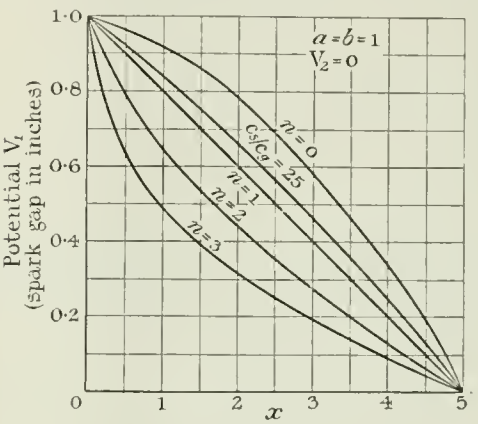


Fig. 5.

obtained above are only true when a and b are both unity.

In Equation (7), m_1 is always positive whatever the value of n (positive or negative) and m_2 is always negative, so that, in order that the potential gradient may be uniform, m_1 must be equal to unity. Therefore

$$-\frac{n-1}{2} + \frac{1}{2}\sqrt{\left(\frac{4b + a(n-1)^2}{a}\right)} = 1,$$

From which we obtain

$$n = \frac{b}{a} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

From this we see that n must always be positive and equal to the ratio of C_g to C_s at the point $x = 1$. If b is greater than a, or, in other words, C_g at the point $x = 1$ is greater than C_s, n must be greater than unity, and if b is less than a, n will be smaller than unity.

Therefore, C_s must be graded in such a way that it will increase with x, so that the potential gradient may be uniform. But it is not necessary to grade C_g in one fixed direction. For example, when $b/a = 1$,

$n = 1$; in this case, C_s is to be graded so as to be proportional to x, and C_g so as to be inversely proportional to x, in other words, C_s and C_g will be graded in opposite directions. Next, when $b/a = 2$, $n = 2$; $C_s = ax^2$, and $C_g = b$, i.e. C_g is not graded. When $b/a = 3$, $n = 3$; $C_s = ax^3$, $C_g = bx$, i.e. both C_s and C_g are graded in the same direction, but differ in degree. In any case, the grading direction of C_s is fixed; the grading of C_g is to be in a direction opposite to that of C_s for all values of b/a less than 2, and in the same direction for values greater than 2.

When the number of condensers is finite and all their values are known, we can easily construct the potential distribution curves by a well-known elementary method as follows: Let the resultant capacity at the junction points I, II, III, etc. (in. Fig. 6), be C_I, C_{II}, C_{III}, etc., and let the potential differences at the same points be V_I, V_{II}, V_{III}, etc. Then

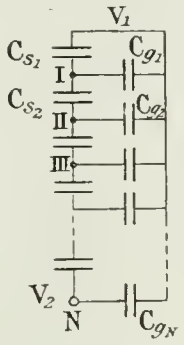


Fig. 6.

$$C_I = C_{s1} + C_{g1},$$

$$C_{II} = \frac{1}{1/C_I + 1/C_{S2}} + C_{g2}.$$

$$C_N = \frac{1}{1/C_{N-1} + 1/C_{Sn}} + C_{gn}.$$

and $V_N = 0$,

$$V_{N-I} = \frac{C_{N-I}}{C_{N-I} + C_{Sn}} V_1,$$

$$V_{N-II} = V_{N-I} + \frac{C_{N-II}}{C_{N-II} + C_{Sn-1}}(V_1 - V_{N-I}).$$

Some of the curves in Fig. 4 are drawn by this method. In this figure, C_s and C_g have the following values:

x	Curve 7		Curve 8		Curve 9		Curve 10		Curve 11	
	C_s	C_g	C_s	C_g	C_s	C_g	C_s	C_g	C_s	C_g
1	1	1	1	1	1	1	1	1	1	1
2	2	1	2	0·75	2	0·75	3	0·75	4	0·75
3	3	1	3	0·5	4	0·5	6	0·5	9	0·5
4	4	1	4	0·25	6	0·25	10	0·25	16	0·25
5	5	1	5	0	8	0	15	0	25	0

In practice it is not a good plan,* and in some cases it may be practically impossible, to make n or b/a very large, because with greater values of n the degree of gradation of C_s must be high, therefore various condenser capacities must be selected. Even if we make n small, we can make the potential distribution uniform, so it is convenient to make n less than 2.

In some cases it is necessary to modify the potential distribution to a much higher degree than is necessary to obtain a uniform distribution of the potential gradient, or, in other words, it is desirable to shift the point of maximum gradient from the end towards the centre, as shown by curves 9, 10 and 11 in Fig. 4, and the curves in Fig. 5.

One example of this is when the capacity of a condenser is graded by changing the distance between the electrodes, keeping the size of the electrode constant. In this case, when the potential differences across the condensers are all equal, the insulation of each condenser is not equally strained, and the condenser having the greatest capacity must be where the maximum stress per unit length of dielectric occurs; in order to make the stress per unit length uniform, it is necessary to keep the potential differences of large-capacity condensers at values lower than in the case of uniform distribution.

For the purpose of uniform distribution of potential gradient along x, C_s and C_g are to be taken as follows:

(*a*) The greater the value of the ratio C_s/C_g, the better.
(*b*) C_s is to be graded so as to increase with x.
(*c*) C_g is to be graded so as to decrease when x increases.

IV. Bushing.

With plain bushing the corona appears at far lower voltages than that at which puncturing or surface arcing-over occurs, and it gradually climbs to the upper end when the voltage is raised. On account of this surface corona, bushing cannot be used at potentials corresponding to its puncture voltage, and its use must be restricted to far lower voltages. The ideal bushing is one with which the surface corona never appears at any voltage lower than its puncture voltage, but in which surface arcing-over and insulation puncture occur at the same time, without a local corona having taken place.

This abnormal potential gradient at the foot is easily explained by the previous theory and by curves 1 to 6 in Fig. 4. Conductor "*a*" in Fig. 2 corresponds to the high-tension terminal of the bushing, and conductor "*b*" to the oil tank of the transformer, the maximum potential gradient occurring near the end point $x = l$, or near the oil tank. In the author's design of bushing, as already mentioned, if capacities C_s and C_g are graded according to the law explained in Section III, the potential gradient along the surface can be modified at will. For the purpose of grading C_s, we may vary the thickness of the insulating discs, or the size of the rings. If we grade C_s and C_g by

* The author's new method of design of bushing and transformer coils is in accordance with this theory.

varying the form of the rings, the heights of the rings will be decreased from top to bottom, and their widths will be increased as in Fig. 1.

The calculation of C_s and C_g is quite easy, because it is not necessary to take their absolute values; we need only the ratio C_{sn}/C_{sn-1} or the form of grading. For example, height of bushing = 35 cm, diameter of oil-filled tube = 10 cm, number of rings = 25, and C_s is graded by changing the thickness of insulating discs.*

The author connected the above bushing in parallel with another plain bushing of similar construction, but without rings. On applying the voltage, in the case of the plain bushing a corona started at its foot at 40 000 volts, and at over 80 000 volts about three-quarters of the surface was covered with corona, arcing-over taking place at 90 000 volts. At this pressure there was no corona on the author's bushing. The bushing was then disconnected from the circuit and tested alone, no corona appearing until a pressure of 180 000 volts was applied.

V. Coils.

We shall now study the electrical behaviour of transformer coils, neglecting resistance. Since there is electrostatic capacity between adjacent turns as well as to earth, the coils are electrically equivalent to the arrangement shown in Fig. 7, in which

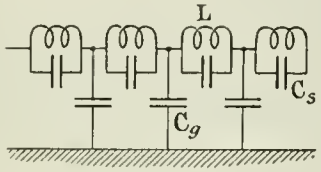


Fig. 7.

L = coefficient of self-induction per unit length,
C_s = electrostatic capacity between adjacent turns per unit length, and
C_g = electrostatic capacity to earth per unit length.

In this case C_s and C_g are considered to be constant. Let us first consider the effect of the parallel connection of C and L. The resultant impedance is $\omega Lj/(1 - \omega^2 CL)$, and it acts merely as an inductance at frequencies lower than those given by $1 - \omega^2 CL = 0$, and as a condenser at higher frequencies. In the former case, the equivalent inductance is $L_e = Lj/(1 - \omega^2 CL)$, and in the latter the equivalent capacity is

$$C_e = (\omega^2 CL - 1)/\omega^2 L.$$

Now, let f be the frequency of the applied voltage, and f_k the frequency which satisfies the equation $1 - \omega^2 C_s L = 0$. Then the equivalent inductance of L and C_s is

$$L_e = \frac{L}{1 - \omega^2 C_s L}$$

and the equivalent capacity will be

$$C_e = \frac{\omega^2 C_s L - 1}{\omega^2 L}$$

* *Denki-hōron*, vol. 7, No. 5.

At frequencies lower than f_k, the transformer coil acts as L_e and C_g in Fig. 8, that is, in the same way as a transmission line whose inductance and capacity to earth per unit length are L_e and C_g respectively.

For frequencies higher than f_k the coil can be considered to be a combination of C_e and C_g, as in Fig. 9. The values of C_e and C_g vary with the frequency.

We shall now consider in more detail the case where one terminal is earthed.

(*a*) *The case where* $f < f_k$.—Here the coil may be considered to consist of L_e and C_g, and, therefore, if alternating current is applied to it electrical oscillations may take place, and consequently the coil may be damaged by the abnormal potential gradient due to resonance. In this case, the natural frequency of the coil is

$$f_0 = \frac{k}{2} \cdot \frac{1}{l\sqrt{(C_g L_e)}}$$

where l = length of the coil measured from the earthed end, and $k = 1, 2, 3, \ldots$ etc.

Inserting the value of L_e we have

$$f_0 = \frac{k}{2l} \cdot \frac{1}{\sqrt{\left[C_g L\left(1 + \frac{\pi^2 k^2}{l^2} \cdot \frac{C_s}{C_g}\right)\right]}} \quad . \quad . \quad (15)$$

Put $C_s l = C_{st}$, $C_g l = C_{gt}$; then C_{st} is the total capacity of C_s between the open end and earth, and C_{gt} is the total capacity of C_g. Then

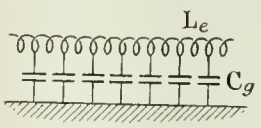


Fig. 8.

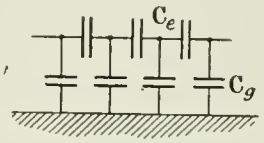


Fig. 9.

$$f_0 = \frac{k}{2l} \cdot \frac{1}{\sqrt{[C_g L(1 + \pi^2 k^2 C_{st}/C_{gt})]}} \quad . \quad . \quad (15a)$$

In this equation, when C_{st} is very small compared with C_{gt}, the natural frequency, that is, the frequency most dangerous to the transformer, is determined by the value of the capacity to earth, in other words, in the determination of f_0 the effect of C_s need not be taken into account.

(*b*) *The case where* $f > f_k$.—Consider next where a constant potential is applied to the open end. The transformer coil is considered to be composed of C_e and C_g, as in Section III. Conductor "*a*" in Fig. 2 corresponds to earth in this case, and conductor "*b*" to the open-end terminal, that is, the opposite to the case of bushing, and C_e is substituted for C_s.

In this case, if C_e and C_g be uniform, a dangerous potential gradient may occur at the end point $x = l$, as shown in Fig. 4. The magnitude of this potential gradient depends upon the value of C_g/C_e. In this case

$$\frac{C_g}{C_e} = C_g \frac{\omega^2 L}{\omega^2 C_s L - 1}, \quad \text{and} \quad \omega^2 C_s L - 1 = 0 \text{ at } f_k;$$

therefore the strain upon the end coil is a maximum

when the frequency of the applied potential is equal to the critical frequency. From the above considerations we may draw the following conclusions:—

When $f < f_k$, the danger of breakdown in the case of a transformer is due to electric resonance, that is, to the coincidence of the frequency of the impressed wave with the natural frequency of the coil, or, in other words, to the abnormal potential gradient of the stationary wave. In this case the maximum potential gradient, and consequently the breakdown of the coil, does not take place necessarily at the end coil; in most cases it occurs at an inner point.

Next, when $f > f_k$, if C_s and C_g be uniform (this assumption holds good in an actual transformer) the breakdown will occur at the end coil, and the magnitude of the abnormal potential gradient will be a maximum at $f = f_k$. There are generally two dangerous frequencies for transformers, one is a frequency which approximates to the natural frequency of the coil, and the other is one near the critical frequency.

The question then arises: What causes these dangerous electric disturbances? There may be many causes, but the arcing to earth that sometimes takes place at the terminal of the transformer or at one point of the circuit connected to the transformer is considered to be the most frequent and dangerous.

Let us next consider the case of a transformer connected to an external circuit, for example, to a transmission line. At the terminal of the transformer there will be a concentrated capacity C_0 with its bushing, in which case the arrangement is the same as with the induction coil (Fig. 10). Therefore, when arcing takes place at any external point, the frequency of oscillation may be considered to be determined by the constants L_1, C_1 and C_0, that is, by the constants of the external circuit, and not by those of the transformer itself as in the case of an induction coil, L_1 and C_1 being the inductance and capacity respectively per unit length of the external circuit. If this frequency is near to the natural frequency of the coil there may be danger due to resonance, and if it is near to and higher than the critical frequency the end coil will be damaged. Therefore, the extent and position of the breakdown depend not only on the construction of the transformer itself but also on the conditions prevailing in the external circuit. One transformer may break down internally, and another may be damaged at the end coil, even though they are of the same construction and size, if the conditions in the external circuit are different.

The natural frequency varies with the length of the conductor, and is very high in low-voltage transformers, sometimes several hundred thousand per second, while in very high-voltage testing transformers it is very low.

In the case of high-voltage power transformers the natural frequency may be in resonance with that of the wave produced by arcing in the external circuit. But with testing transformers of very high voltage the danger is rare, because the natural frequency of the coil is very low and the external circuit is very short; consequently the frequency of the wave produced in the external circuit is very high, so that there can be no resonance. Therefore if any resonance occurs in the testing transformer, it would be due to the higher harmonics in the alternator wave or to some other cause arising on the low-voltage side of the coils.

The author's method of protection against abnormal potential gradient has already been described in Section II. This method is chiefly for use with testing transformers, but it also protects against resonance.

(*a*) Let us now consider in more detail the theory briefly outlined in Section III.

As mentioned above, the dangerous potential gradient in testing transformers is mainly due to the wrong distribution of C_s and C_g; and it will therefore be avoided by the adoption of the author's method.

In grading capacities, it is desirable that the coils should be arranged so as to conform with the conditions outlined, in addition to adding shunt capacities. The method of doing this is to employ near the open end thin, deep windings of large internal diameter and thick windings of small depth and less internal diameter near the earthed end, as shown in Fig. 13.

We shall now discuss the three reasons for inserting resistance in series with the capacity.

The first is the damping of waves; the second the division of the current-flow into two paths, that is,

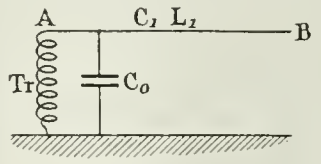


Fig. 10.

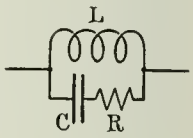


Fig. 11.

through the coil and the capacity (artificially added); and the third the prevention of a decrease in the natural frequency of the coil, which may be caused by the addition of shunt capacities. The second and third reasons are intimately connected. It is evident that resistance damps electrical oscillations and lessens the time during which the coil will be strained. The second reason is somewhat interesting.

Consider the case shown in Fig. 11. The division of the current into two branches L and C depends not only upon the values of L and C but also upon R, consequently the resultant capacitance (or inductance) varies with the value of R for given values of L and C.

Let the effective inductance, resistance and impedance in Fig. 11 be L_e, R_e and Z_e respectively, then

$$\left.\begin{aligned} L_e &= L\frac{\omega^2C^2R^2 + (1-\omega^2CL)}{\omega^2C^2R^2 + (1-\omega^2CL)^2} \\ R_e &= R\frac{\omega^4C^2L^2}{\omega^2C^2R^2 + (1-\omega^2CL)^2} \\ Z_e &= \frac{\omega L\sqrt{[R^2 + (1/\omega C)^2]}}{\sqrt{[R^2 + (1/\omega C - \omega L)^2]}} \end{aligned}\right\} \quad . \quad . \quad (16)$$

If $\omega^2CL - 1 < \omega^2C^2R^2$, the circuit shown in Fig. 11

will act as a combined inductance and resistance, and its inductance will be

$$L_e = L\frac{\omega^2C^2R^2 + (1 - \omega^2CL)}{\omega^2C^2R^2 + (1 - \omega^2CL)^2},$$

and, if $\omega^2CL - 1 > \omega^2C^2R^2$, the circuit will act as a combined capacitance and resistance, and its capacity will be

$$C_e = \frac{\omega^2C^2R^2 + (1 - \omega^2CL)^2}{\omega^2L\{\omega^2C^2R^2 + (1 - \omega^2CL)\}},$$

that is, the effective capacity depends on the value of the resistance. Therefore, if we insert resistance in

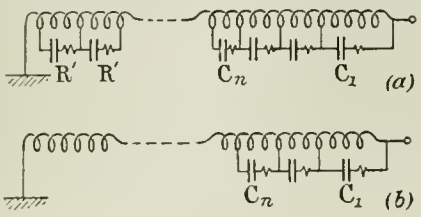


Fig. 12.

series with capacity, so long as C_e is not converged into L, we can vary the effective value of the capacity, not only by varying the protecting capacity but also by varying the resistance R; that is, the grading of C_e may be effected by varying the resistance.

Now let us take the third reason. If every coil of the transformer is shunted with capacity, even though the capacities are graded, the natural frequency must be lowered and, as a result, there may be a breakdown due to resonance of the higher harmonics in the alternator wave. As shown in Equation (15), the effect of shunt capacities upon the natural frequency is considered to be replaced by one resultant capacity C_{st}.

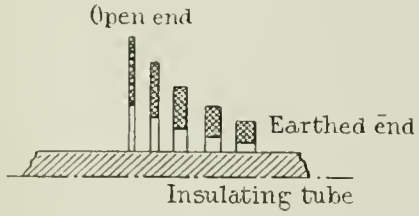


Fig. 13.

Therefore, even in the case where every coil is shunted with capacity, if the resultant value of C_{st} be kept small, the natural frequency of the coil will not be affected. If we put high enough resistances in series with capacities, the desired effect will be obtained. But for this purpose it will be better if some of the coils near the earthed end are left without shunting capacities, as in Fig. 12 (*b*), for breakdown does not usually occur here. In Fig. 12, C_1, C_2, . . . C_n will protect the individual coils if the resistance in series is not very large, but their combined action, i.e. C_{st}, may be reduced to a great extent by series resistances R' as shown in Fig. 12 (*a*), and by the inductance of the unshunted part of the coil in Fig. 12 (*b*). In the author's opinion it is preferable to shunt about one-quarter or one-third of the total number of coils in testing transformers with capacities, and to leave the rest without protecting capacities, then, as shown later, the natural frequency will not be affected at all.

If, however, we leave some coils without shunt capacity, the series resistance is very necessary, because at some frequencies the shunted part acts as C and the unshunted part as L, and electric oscillations may be set up; in this case there must be some damping resistance.

(*b*) We shall now consider the effect of the device with various wave-forms.

Let us first take a rectangular wave. If there are

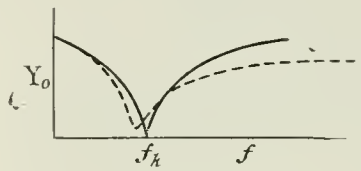


Fig. 14.

not any branch circuits of C, the wave will travel along the conductor at a certain speed. For this reason it may occur that at the instant under consideration the front of the wave has already reached a certain point of the coil; between this point and the next there will be a potential difference corresponding to the height of the wave.

In the case of capacity circuits, the wave is divided at the entrance into two parts, one of which travels along the capacity circuit, and the other the coil circuit; hence the height of the wave traversing the latter is reduced.

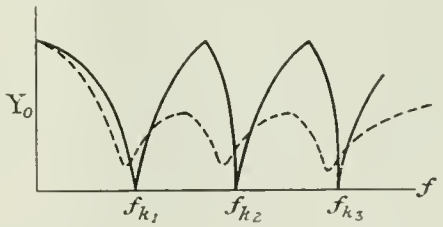


Fig. 15.

The grading of capacities is also important from the point of view of breakdown caused by the wave front. If all capacities are of equal value (the inductance of each coil is assumed to be equal) the division of the current is the same for each section; each part of the wave passes along the coil and capacity individually, without interfering with each other, that is, the part which enters the coil at the entrance passes through this circuit only, and the same applies to the part which enters the capacity circuit. In this case, there is a danger of breakdown caused by the wave flowing in the coil circuit, although much smaller than in the case where there is no branch capacity circuit. But if the capacities are graded, the division of current is

different for each section, therefore two divisions of current, after traversing every section, meet again at each junction point and are divided again in a different proportion. Thus at every junction point two parts of the current will join. The capacity current can be considered to pass instantaneously from one junction point to the next, while the current in the coil circuit takes a certain time to pass between the same two points. Therefore all the junction points will be

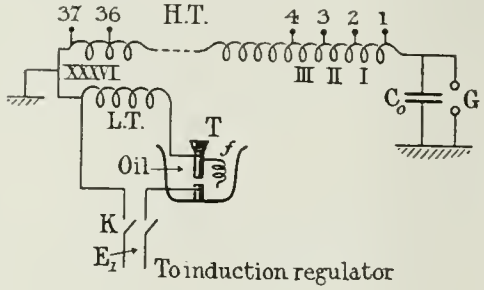


Fig. 16.

charged at once by the capacity current before the coil current can reach them. In other words, the coil is charged from several points at the same instant. It is evident that when this happens there is less danger than when it is charged from one end point.

(*c*) Let us now consider the effect of the device on resonance. In the case of testing transformers, if the natural frequency of coil is very small, there is no effective device for preventing resonance. But in the case of power transformers, if the natural frequency is high and is comparable with that of the current

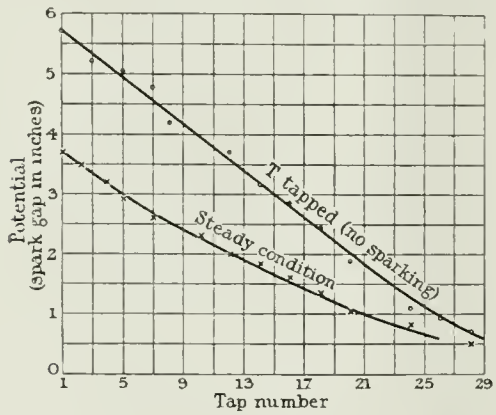


Fig. 17.

set up in the external circuit by an arc to earth, resonance can be prevented by this device. As shown in Equation (16), the impedance of the circuit composed of L, C and R in parallel is

$$Z_e = \frac{\omega L\sqrt{[R^2 + (1/\omega C)^2]}}{\sqrt{[R^2 + (1/\omega C - \omega L)^2]}}$$

and this value varies with the frequency.

Now suppose $Y_e = 1/Z_e$. The variation of Y_e* with f is shown in Fig. 14.

If $R \neq 0$, Y_e becomes zero when $f = f_k$.

If $R = 0$, Y_e does not vanish at any value of f, but, if R is not very large, Y_e becomes very small at points near $f = f_k$.

When an alternating current of constant voltage and variable frequency is applied across such impedance the amount of current passing through the

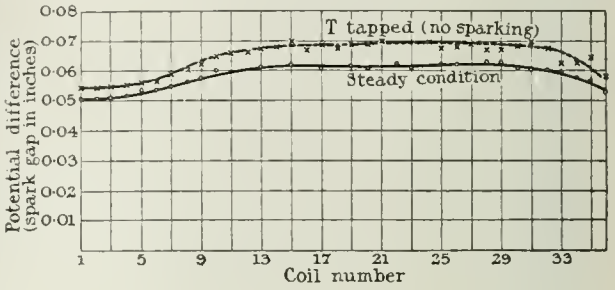


Fig. 18.

impedance is as shown in Fig. 14. If many such impedances are connected in series and their critical frequencies, i.e. the frequencies satisfying the relation $\omega^2 CL = 1$, are all different, the current passing through

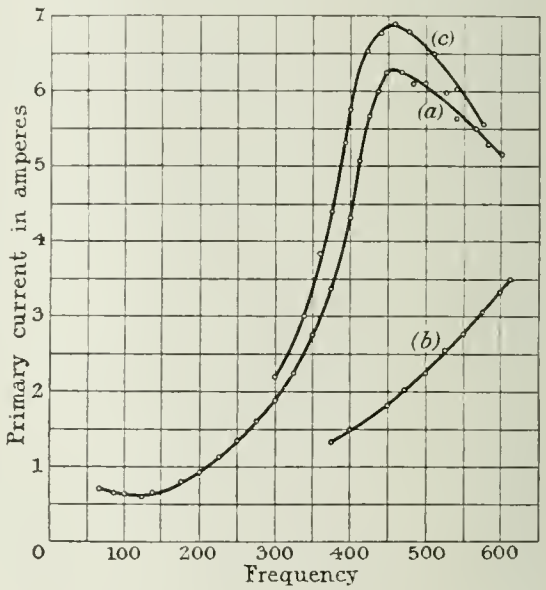


Fig. 19.

these impedances in series is as shown in Fig. 15. The lowest points correspond to the critical frequencies.

In the author's device the capacities are graded and each section has a different critical frequency, and if these are equal to the natural frequency, or multiples of it, any disturbing current from the external circuit is checked. Consequently, the resonance can be damped out.

* *Denki-hioron*, vol. 8, No. 9.

It is easy to select sets of coils which will do this without nullifying the protective effects.

VI. Experiments.

The details of the transformer tested were as follows:

Number of coils = 36,
Number of turns per coil = 1 800,
Resistance per coil = 530 ohms,
High-tension conductor, B. & S. No. 32,
Number of turns on low-tension side = 126,
Resistance of low-tension coil = 0·055 ohm,
Low-tension conductor = 14·02 sq. mm,
Cross-sectional area of the core (gross) = 36·4 sq. mm.

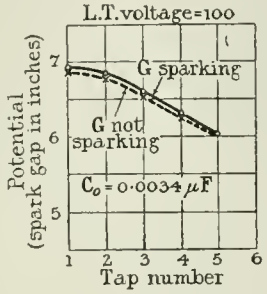


Fig. 20.

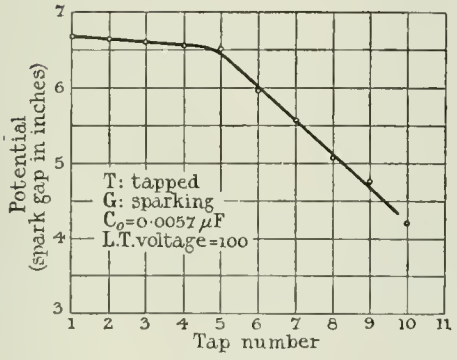


Fig. 21.

The connections for the test are shown in Fig. 16, where

1, 2, 3, . . . 37 = taps,
I, II, III, . . . XXXVI = coils,
K = switch,
T = oil switch.
f = spring of moderate strength. If the upper terminal of the oil switch is pressed down by hand, the circuit is made, and on removing the hand the switch is opened by means of the spring. By tapping the upper terminal of T, we can make and break the circuit at a moderate speed.
G = spark gap, the electrodes of which are spherical balls of 2 cm diameter,
C_0 = glass condenser of capacity C_0.

The voltage of the supply circuit was kept constant by means of an induction regulator, and its frequency was practically constant.

After adjusting the length of the gap G so that it just sparked over when the switch T was made or broken, the upper terminal of T was tapped about 200 times per minute, and the potentials or potential

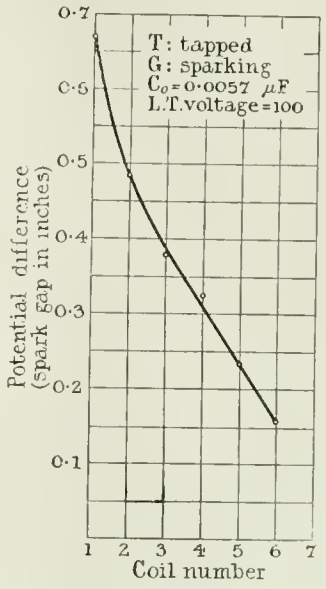


Fig. 22.

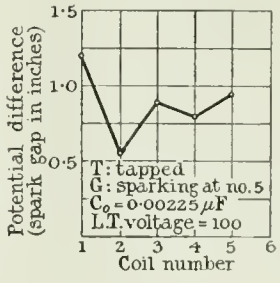


Fig. 23.

difference at various points were measured by means of a needle gap. The distances between the needles were accurately measured by means of a screw.

Fig. 17 shows the potential distribution of the coil without G and C_0 when E_1 = 100 volts.

Fig. 18 shows the potential gradient in the same case.

Fig. 19 (*a*) shows the resonance curve, that is, the relation of the primary current to the frequency of the supply circuit when E_1 = 40 volts. We see that the natural frequency of the transformer was about 450 periods per second.

Fig. 19 (*b*) shows the resonance curve when the number of coils was reduced to 26. For the purpose of maintaining the natural frequency at a higher value we must make the voltage per turn high and reduce the length of the coil.

Fig. 19 (*c*) shows the resonance curve with the author's device in circuit, as explained later.

Fig. 20 shows the potential curve when T was tapped, and $C_0 = 0{\cdot}0034\ \mu F$. From Fig. 20 we know

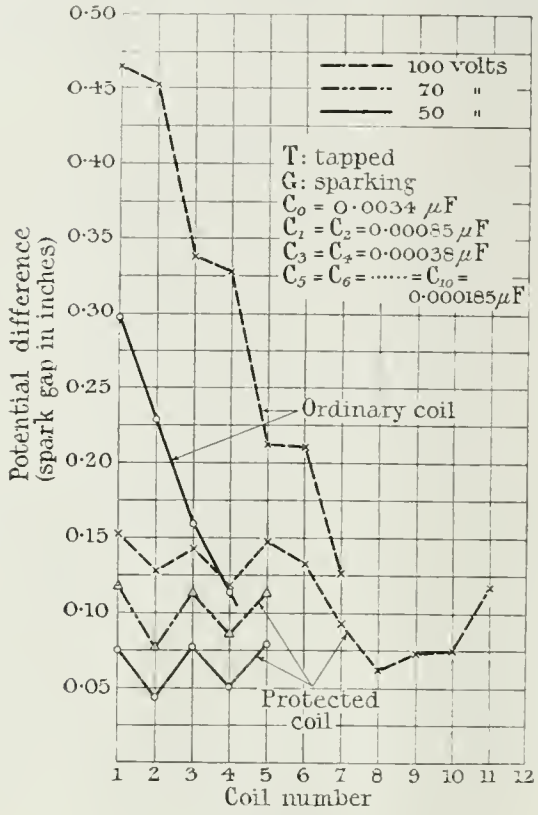


FIG. 24.

that the absolute potential is affected only to a very small extent by sparking at the terminal.

Figs. 21 and 22 show the potential and potential gradient respectively, when $C_0 = 0{\cdot}0057\ \mu F$. The end coil is severely strained.

Fig. 23 shows the potential-gradient curve when G sparked at tap No. 5, instead of at the end point.

These data show that there was a dangerously high potential gradient of about 20 times the normal value.

Fig. 24 shows the potential gradients of the original coil and a coil of the new design when G sparked at $C_0 = 0{\cdot}0034\ \mu F$. In the latter case, the 10 coils had the following capacities in shunt:

$$C_1 = C_2 = 0{\cdot}00085\ \mu F,$$
$$C_3 = C_4 = 0{\cdot}00038\ \mu F,$$
$$C_5 = C_6 = C_7 = C_8 = C_9 = C_{10} = 0{\cdot}000185\ \mu F.$$

We see from this that the potential gradient is much reduced by the adoption of the author's device. The next table shows steady potential gradients in inches when resonance occurs and $E_1 = 50$ volts.

Coil No.	Without Shunt Capacity, $f_0 = 450$	With Shunt Capacities (as in Fig. 24), $f_0 = 437$
I	0·0575 inch	0·0585 inch
V	0·0363 ,,	0·0367 ,,
IX	0·0237 ,,	0·0250 ,,
XIII	0·0345 ,,	0·0337 ,,
XVII	0·0463 ,,	0·0600 ,,
XXI	0·0780 ,,	0·0820 ,,

As shown by the above table and Fig. 19, by adding capacities according to the author's method, the natural frequency and the potential gradient at resonance are affected to only a very slight degree. The author desires to express his thanks to Dr. K. Ogura for help in the investigation.

DISCUSSION ON

"HARMONIC ANALYSIS BY SELECTED ORDINATES."*

Dr. A. Russell (*communicated*): I have read Mr. Clayton's paper with interest, but desire to record my dissent from his conclusions which, in my opinion, are misleading. Although the ground has been gone over many times before, it will be useful to restate the problem as, owing to the injudicious claims of many inventors of schedules, the manufacturers for whose benefit they claim to write are fighting shy of the whole method of harmonic analysis. Few recognize the enormity of the assumption that is made when it is supposed that the effect of the harmonics whose order is greater than n can be neglected. Let us suppose for example that we make the assumption that the wave is symmetrical and that the fifth and higher harmonics can be neglected. In this case we have

$$y = a_1 \cos x + a_3 \cos 3x$$

Hence $a_1 + a_3 = y_0$ and $y_{\pi/6} = a_1(\frac{1}{2}\sqrt{3})$, and hence a_1 and a_3 can be found. Some writers would say that the solution was exact in this case, and therefore superior to the methods given by mathematicians. However, when we remember that the solution applies only to one of the infinite number of curves which pass through the ends of the ordinates y_0 and $y_{\pi/6}$, we may well think that it is of little value. Let us now consider the curve given on page 510. Its equation is known to be of the form

$$y = a_1 \cos x + a_3 \cos 3x + \ldots + a_{13} \cos 13x \quad . \quad (1)$$

These constants can be determined by the given schedule. They can also be very readily determined by the method of ordinates. We have, for example,

$$\begin{aligned} a_1 + a_3 + \ldots + a_{13} &= y_0 = 1\,090 \\ a_3 + a_9 &= \tfrac{1}{3}(y_0 - y_{\pi/3} + y_{2\pi/3}) \\ &= \tfrac{1}{3}(1\,090 - 395 - 395) = 100 \end{aligned}$$

$$a_5 = \tfrac{1}{5}(y_0 - y_{\pi/5} + y_{2\pi/5} - y_{3\pi/5} + y_{4\pi/5}) = -50,$$

and so on. We thus with very little labour, beyond measuring the ordinates, find the exact values of the constants. Silvanus Thompson showed the writer, 11 years ago, quick mechanical methods which he had invented for summing the odd and the even ordinates and subtracting the one sum from the other. If, however, we try to apply a schedule to the harmonic analysis of a given curve we at once meet with difficulties. Assuming that the 15th and higher harmonics are absent, we see that we can make the curve (1) pass through only seven points on the given curve and that the values of the constants will in general vary, depending on which points we choose. Now Fourier showed that the coefficients of the cosine terms in the Fourier series were constants for a given curve, just as its area or the distance of its centre of gravity from a given line is constant. Hence if we limit the number of harmonics our solutions are only approximate for a given wave. On page 506 the author says: "Many of the waveforms met with in electrical practice, although containing only harmonics of comparatively low orders, have quite a large number of points of discontinuity. . . ." I do not know how it is ascertained that only harmonics of low orders are present. If there are points of discontinuity in the wave-shape—where the ordinate suddenly leaps from one value to another or where the gradient of the wave instantaneously changes—then these discontinuities must be due to abnormal action on the part of the oscillograph. The waveform also would have an infinite number of harmonics. In practice we are rarely justified in making the assumption that all harmonics whose orders are greater than n can be neglected. I know of no criterion and the author gives none for determining this. If we assume that in a simple alternator the reluctance of the magnetic field is an absolute constant in all positions of the rotor and that the magnetic flux is sine-shaped, we might get a sine wave on open circuit. In any given case it is quite certain that neither of these assumptions is strictly true and therefore the open-circuit wave can be expressed only by a doubly infinite series of harmonics. For example, if we have n slots in the polar step we get an infinite series of harmonics the orders of which are $2ns + p$ and $2ns - p$ respectively, where p and s are any odd integers. Under load conditions, Lyle's theory* of the electric generator shows that even if no iron is used in its construction there is always an infinite number of odd harmonics present in the armature current, and an infinite number of even harmonics in the exciting current. For an E.M.F. wave, therefore, we have to assume that there is an infinite number of harmonics. Methods which proceed on the assumption that there are no harmonics present higher than the nth may conceivably give approximate values of the harmonics. The answers given may also be extremely inaccurate. In my opinion, unless a definite criterion be given which will indicate the complete absence of higher harmonics it is waste of time to use a schedule. The ordinate methods and the methods —not discussed by the author—founded on measuring the areas of strips of the given wave are of value, as they give simple relations between the harmonics which are useful in checking the values calculated by other methods. The writer thinks that the best and most accurate practical methods are those founded on Fourier's solutions. Fourier showed that the problem of finding the amplitude and phase of the

* Paper by Mr. A. E. Clayton (see page 491).

* *Proceedings of the Physical Society*, 1909, vol. 21, p. 702.

nth harmonic is completely solved when we have found the values of a_n and b_n which are given by

$$a_n = \frac{1}{\pi}\int_0^{2\pi} f(x) \cos nx \, dx ;$$

and

$$b_n = \frac{1}{\pi}\int_0^{2\pi} f(x) \sin nx \, dx,$$

where $y = f(x)$ is the equation of the wave to be analysed. These values can always be found by drawing the curves

$$y = f(x) \cos nx \text{ and } y = f(x) \sin nx \quad . \quad . \quad (2)$$

and finding their areas between the limits 0 and 2π either by the planimeter or by the method of graphical quadrature used by the writer. The curves (2) will each contain $2n$ loops the areas of which have to be determined. In the graphical method the operator has to determine the number of ordinates per loop he has to measure so as to make reasonably certain that he gets the area correctly. This number varies with the shape of the wave. In everyday practice 10 ordinates are generally drawn and measured in order to find the area of a curve. Five would be too few and two would be ridiculous. We must not, for instance, analyse a pure sine wave—as the author does on page 508—by drawing 12 ordinates per half period and then proceed to find the amplitude of the 11th harmonic. This operation would be just as sensible as finding the area of a wave by measuring the length of one ordinate and a fraction of another ordinate. It will be seen that this method is perfectly definite and applies to wave-shapes of every form. A complete account of it is given in the second edition of Russell's "Alternating Currents" (vol. 2, chap. 3). We can in most cases determine by its use the first few harmonics without difficulty. The method shows that the accurate determination of a high harmonic is almost an impossibility, except in very special cases. In the special case of a rectangular wave the measuring of the ordinates presents no difficulty, and thus, supposing that the height of the wave is unity, that $12n$ ordinates are drawn, and using Weddle's rule of graphical quadrature, we get

$$a_n = 0 \; ; \; b_n = 1{\cdot}2732/n$$

where n is an odd number. The value of b_n is given correctly to the last figure. It may be said that the old-fashioned way of assuming that the given wave can be expressed as follows :

$$y = a_1 \cos x + a_3 \cos 3x + \ldots + a_{2n+1} \cos (2n+1)x + b_1 \sin x + b_3 \sin 3x + \ldots + b_{2n+1} \sin (2n+1)x$$

and determining the constants by drawing $2n$ ordinates, gives the approximate values of the Fourier coefficients. Remembering, however, that an infinite number of waves can be drawn through the $2n$ points on the curve determined in this way, we see by Fourier's solutions that the constants can have widely different values for various members of this family of curves. We have, therefore, no security, beyond the common-sense of the operator, that his results are correct.

My conclusions are :

(1) That unless we have some criterion which will tell us the order of the highest harmonic in a given wave, it is never safe to use a schedule.

(2) That the values of the first few harmonics can always be accurately found without much difficulty.

(3) That it is always advisable to use the various relations between the amplitudes of the harmonic, the ordinates, and particularly the areas between the ordinates, in order to check our results.

An urgent question at present is to find a criterion which will tell us how closely the form of a wave approximates to sine shape. I would suggest that a first rough test would be to determine the amplitude of the first harmonic of the wave and compare it with the effective value of the wave multiplied by $\sqrt{2}$. I am aware that this neglects the phase differences between the harmonics of the wave, but I fail to see on what principle these differences are to be taken into account. The "deviation factor" as defined in the "Standardization Rules" of the American Institute of Electrical Engineers does not seem to rest on a sound theoretical basis.

Mr. A. E. Clayton (*in reply, communicated*) : I quite agree with Dr. Russell that there has been a tendency for manufacturers to lack confidence in the results obtained when wave-forms are analysed. So far as the Runge method of analysis is concerned this state of affairs has been brought about partly by the misguided attempts to reduce too much the time necessary to complete the analysis by the use of too limited a number of ordinates, and partly by an absence of knowledge as to the exact effect of the presence of any particular harmonic beyond the normal scope of the schedule, upon the values computed for the various harmonics. For example, I have little doubt that the opinion was commonly held that the presence of any harmonic higher than the nth would introduce errors in the values computed for every harmonic. Actually, as shown in the paper, the values computed for the amplitudes of the various sine and cosine terms are represented by series (4) and (6) on pages 494 and 496, and each such higher harmonic can affect the value computed for one, and one only, of the harmonics within the scope of the schedule. The importance of employing a sufficiently high value for n—the number of ordinates per half period—is thus apparent, and the advantage gained by using such a high value is due as much to the fact that only one in every n of the total possible orders of higher harmonics can influence the computed value, as to the fact that more harmonics are thereby included within the scope of the schedule. It is therefore simply necessary to employ a sufficiently high value for n to reduce to a negligible amount the error in the value computed for any harmonic.

It is stated in the paper and re-stated by Dr. Russell that, by using the Perry method or its Runge simplification, the computed values for the various harmonics correspond to one particular wave out of an infinite number that can be conceived to pass through the measured points, and that to obtain correctly the amplitudes, etc., of the various harmonics it is necessary to obtain the values of the Fourier integrals on

page 492. If it were possible to draw accurately the curves $x = y \sin p\theta$ and $x = y \cos p\theta$, and to determine accurately their areas by any of the known methods, the correct amplitudes of the various terms could, of course, be obtained. In actual practice the accurate construction of these curves would be a matter of great difficulty even for the lower harmonics, and soon becomes impossible as the order of the harmonic rises, owing to the need of measuring the values of too large a number of ordinates. Taking $12p$ ordinates per half period of the wave, so that the pth harmonic can be determined by applying Weddle's rule, without actually constructing the graph, it is at once obvious that, unless the wave-form is available on a very large scale, it is useless to attempt to determine the values of any except the lowest harmonics. Further, in the case of the higher harmonics the curves cross the axis many times, so that extreme accuracy would be required in order to obtain a reliable value for the total area. With the oscillograph records usually available, and the wave-shape by no means smooth * but containing a number of points of inflexion and possibly also many turning points, the determination by this method of even the fifth harmonic would be a matter of considerable difficulty.

It being admitted then, as indeed Dr. Russell does, that, for the type of wave-shape met with in electrical practice the accurate determination of a high harmonic is impossible by this method, it remains to utilize some other method, for example the schedule on pages 499 to 502, which, although not giving in every case mathematically exact results, in my opinion gives results which are accurate enough for practical purposes and as accurate as the data will warrant in the great majority of cases. What accuracy, then, are we entitled to expect in practice? Assuming that the oscillogram is a true record of the wave-shape—in itself sometimes a rather big assumption—it is obvious that the degree of accuracy must depend primarily upon the accuracy with which the various ordinates can be placed and measured, and that we cannot expect our results to be correct to five significant figures. Rather must it be realized that an error amounting to a certain percentage of the crest value of the wave is inevitable, quite apart from all consideration as to the theoretical accuracy of the method. The amount of this error depends upon the wave-length and amplitude of the oscillogram, and, in the case of any particular ordinate, upon the slope of the curve at the measured point. In the majority of cases, therefore, when analysing oscillograms I have assumed that this error may be expected to be about 0·5 per cent of the crest value, and that the computed results may have an error of that amount, so that any amplitude less than 0·5 per cent or, in certain cases, 1·0 per cent, of the fundamental must be disregarded.

It remains then to see how the possible theoretical error introduced by the use of the Runge method compares with the inevitable errors accompanying all methods of harmonic analysis involving the measurement of a number of equidistant ordinates. The extreme case of the rectangular wave is worked out in Table 2 (page 495) and shows that with 26 ordinates per half period the error in the computed value of the 13th harmonic is only 1·6 per cent of that of the fundamental, and that to a rough approximation the error is proportional to the order of the harmonic. Compared with wave-shapes that may be expected in alternating-current working, the rectangular wave is abnormally rich in higher harmonics. A rectilinear wave in which the higher harmonics are present in more normal proportions is the triangular wave. P. Kemp * has tabulated the error obtaining when the triangular wave is analysed by the Runge method, using 18 ordinates per half period; for all the harmonics within the scope of the analysis, i.e. up to the 17th, the error amounts to less than 0·4 per cent of the value of the fundamental, the values computed being too high, whereas with the rectangular wave the values computed are too low. By increasing the number of ordinates to 26 the amplitudes computed for the harmonics up to the 17th are correct to within 0·2 per cent of the amplitude of the fundamental. A still further increase in the number of ordinates to 60, the number suggested by Dr. Russell for determining the amplitude of the 5th harmonic by means of Weddle's rule, still further reduces the error, and even in the case of the rectangular wave the error in the computed value, in terms of the fundamental, is only about 0·3 per cent for the 13th, and 0·74 per cent for the 29th harmonic; for the triangular wave the error would not be measurable even on a very large-scale wave-form. It would therefore appear that, without using an unduly large number of ordinates, the theoretical accuracy of the Runge method is certainly likely to be greater than that with which the values of the required ordinates can be measured.

With regard to the choice of the number of ordinates per half period, the schedule on pages 499 to 502 is drawn up for 26 ordinates, as this represents the largest convenient number to utilize with oscillograph records of normal dimensions. If the record is such as to permit, and circumstances warrant the use of, a larger number of ordinates, say about 60 as suggested above, this larger number should be used, not because it permits of the analysis being carried farther, but because the theoretical error of the method of analysis would be rendered quite negligible up to harmonics of very high orders for the wave-shapes met with in normal practice. The example in which 12 ordinates per half period were used was an extreme case to illustrate the fact that, if the highest order of harmonic present is less than the number of ordinates per half period, the Runge method gives correct results in all cases, whereas, as shown also on page 505, the indiscriminate application of Weddle's rule may result in error. In the example no attempt was, of course, made to determine the area of each loop referred to by Dr. Russell, and, indeed, no such attempt is made in the formulæ put forward by Dr. Russell † in which 24 ordinates per period are used for the determination of the amplitude of the third

* In this connection I have to thank Dr. Russell for calling attention to the error on page 506, where "points of discontinuity" should read "points of inflexion."

* P. Kemp: "A Practical Method of Harmonic Analysis," *Journal I.E.E.* 1919, Supplement to vol. 57, p. 87.

† A. Russell: "Alternating Currents," 2nd edn., vol. 2, p. 132, eqns. (19) and (20).

harmonic. I think it is extremely dangerous to employ any method of harmonic analysis in which various of the ordinates are loaded, unless the wave-shape is smooth; in practice, pressure-waves which have to be analysed are unlikely to be smooth.* Further, I regard it as essential that any schedule or formula shall be expected to give correct results when applied to the analysis of a wave-form containing only a limited number of relatively low harmonics. That such wave-forms are of common occurrence in electrical practice does not admit of dispute, although, as stated on page 502, it is quite impossible to predict from a mere inspection of the curve that harmonics above any given order are absent. When using the schedule given on pages 500 and 501 for the analysis of pressure-waves met with in practice, it is usual to find that, with the exception of tooth-ripple harmonics, the values computed for the harmonics higher than about the 11th are negligible. Whilst this is admittedly no mathematical proof that the higher harmonics are negligible, it is surely sufficiently conclusive, especially if it be remembered that there are very good reasons for the assumption that, excepting the tooth ripples, the very high harmonics are negligible. Dr. Russell says that with a simple alternator the open-circuit pressure-wave can only be represented by a doubly infinite series of harmonics, and that under load conditions there is always an infinite number of even harmonics present in the exciting current, and of odd harmonics in the armature current. For a simple alternator this is true, but even in this case it is more than possible that the amplitudes of the terms in the infinite series will be so rapidly convergent as to render any error introduced by the Runge method, with 26 ordinates per half period, negligible in comparison with the probable errors of experiment and operation. Further, in practice the case of the simple alternator does not arise; we have rarely to deal with single-phase or two-phase machines, but chiefly with three-phase machines. With a normal alternator, the flux-distribution curve may deviate considerably from the sinusoidal form with salient-pole machines, and to a reduced extent with distributed field windings as on turbo-alternators. In any case, however, the flux wave approaches the sinusoidal form much more closely than the rectangular wave, and by spreading the coil side of the armature winding over an appreciable portion of the polar pitch the higher harmonics in the E.M.F. wave are reduced very considerably. In the case of a surface winding the corresponding reduction factor is $(\sin p\beta)/(p \sin \beta)$, where β is the spread of the coil side in electrical radians, and p is the order of the harmonic. With the coil side spaced in slots the factor is dependent upon the number and spacing of the latter, but, with certain minor exceptions, the effect is the same, i.e. the importance of the higher harmonics is very much less in the wave of induced E.M.F. than in the flux-distribution wave. With slotted armatures experimental evidence indicates that there are no appreciable pulsations in the total value of the flux, but that ripples in the induced E.M.F. may be established due to flux-swinging over the pole-face. These tooth ripples may be very important in the case of faulty designs, and even in normal designs are often by no means negligible. The orders of these harmonics are $2MQ \pm 1$; in addition there may be a spacing ripple represented by harmonics of the orders $2MQ_t \pm 1$, where M is any integer. Normally only the lowest pairs of these harmonics, as shown by the shape of the ripples, are of importance. On load it is common experience, except in the case of leading currents, that the importance of any higher harmonics in the pressure-wave is reduced, and the shape of the terminal pressure-wave improved. Single-phase alternators are almost invariably provided with amortisseurs, so that the exciting winding is screened from the pulsating effects of armature reaction, and with three-phase machines the very slight pulsations in armature reaction are invariably damped out without the provision of any special damping devices. Therefore in the most extreme case, i.e. with the machine on short-circuit, the fluctuations in the exciting current are relatively small in modern single-phase machines, and in modern three-phase machines the exciting current is quite steady.* As a result it may be taken that secondary actions consequent upon the periodic changes in the magnetizing action of the armature currents are certainly negligible so far as the production of important series of high harmonics is concerned. It may then, I think, fairly be concluded that with alternating-current pressure-waves there is ample justification for the assumption that, with the exception of tooth ripples, the high harmonics are relatively unimportant, and certainly it may be taken that any error introduced by such a series of high harmonics will only be a very small fraction of that occurring in the case of a rectangular wave. Therefore, when the Runge method of analysis is used with a sufficient number of ordinates, excepting only the effect of tooth-ripple harmonics, the theoretical error of the method will be less than that inevitably due to the limitations of the oscillogram and the measurement of the necessary ordinates, even in the case of harmonics well towards the end of the schedule. For the very great majority of cases 26 ordinates per half period will suffice.

To sum up:

(1) In analysing alternating-current wave-shapes, the accuracy of the results is limited primarily by the accuracy attainable in the placing and measurement of the necessary ordinates.

(2) As a result, it is useless to attempt to determine even approximately the value of any harmonic having an amplitude less than about 0·5 per cent of the value of the fundamental, the actual figure depending upon the nature and dimensions of the oscillograph record. With exceptionally large and good records this figure might be slightly improved. Further, quite apart from considerations of the theoretical accuracy of the method, the amplitudes computed can only be expected to be correct within the above limits.

* S. P. Smith and R. S. H. Boulding: "The Shape of the Pressure Wave in Electrical Machinery," *Journal I.E.E.*, 1915, vol. 53, p. 205.

* A. E. Clayton: "The Wave-shapes obtaining with Alternating-current Generators working under Steady Short-circuit Conditions," *Journal I.E.E.*, 1916 vol. 54, p. 84.

(3) With schedules based upon Runge's method, the theoretical error can be reduced to any desired amount by the use of a sufficient number of ordinates. Normally, with 26 ordinates per half period the theoretical errors introduced may safely be assumed to be negligible in comparison with the inevitable errors in measurement, etc., except in the case where pronounced ripples of high orders greater than the nth are present.

(4) To allow for the possible effect of pronounced high harmonics beyond the scope of the schedule, the tables showing the effects of the higher harmonics should always be used.

(5) When the data warrant an attempt to obtain greater accuracy, a schedule drawn up for a larger number of ordinates should be used.

(6) In cases of doubt the results can be checked by the use of a schedule drawn up for a different number of ordinates.

For very abnormal wave-shapes, such as condenser charging currents when the wave of potential difference is rich in high harmonics, the values computed by the Runge method and 26 ordinates could of course only be regarded as rough approximations. Otherwise, the results obtained may be expected to be as accurate as the data will warrant.

DISCUSSION ON "THE MEAN ERROR OF AN ELECTRICITY METER."*

Mr. O. Howarth *(communicated)*: I do not agree with the author's statement on page 336 that the disturbing influence of solid friction is approximately constant at all loads for commutator meters, otherwise the compensating coil on the watt-hour meter would make it possible to get a uniform error between 1/20 and $\frac{1}{4}$ loads, which we find to be impossible. Even a well-designed induction meter will have a greater negative error on 1/10 load than on $\frac{1}{4}$ load, and on 1/20 load a smaller negative error than on $\frac{1}{4}$ load owing to the small constant forward torque produced by the friction compensation device, and in an induction meter the effect of friction is reduced to within very small limits. In dynamometer motor meters the friction is liable to vary to such an extent that errors below $\frac{1}{4}$ load will be unstable and, therefore, of little value in endeavouring to obtain the mean error by any method which takes them into consideration. Because many large undertakings have a three-phase system of supply and base their charges on the maximum demand as well as on the energy consumed, the errors on a three-phase meter and maximum-demand indicator are perhaps the most interesting in view of their importance. In these meters, errors due to friction are not of great importance, as the meters are usually so well compensated that the errors on low loads are very small, also the meters are rarely used below $\frac{1}{4}$ load. In addition to the sources of error mentioned in the paper, there are errors introduced by combining two or three single-phase meters into one, thus introducing an error due to the different loading of the two elements in the case of a 3-phase 3-wire meter or a 2-element 3-phase 4-wire meter; this error will be determined by the relative errors on the separate elements and also by any interaction between the elements. The errors due to either of these causes will obviously vary if the phase rotation is altered. Current transformers are almost invariably used in the case of meters rated at a capacity of 100 amperes and above, thus introducing an error due to the ratio not being the same at all loads and also an error due to the phase relation between the primary and secondary currents not being the same at all loads, this latter having the same effect on the error as if the angle α in the formula at the bottom of page 337 varied when the current varied. It appears, therefore, to be quite impracticable to endeavour to arrive at any formula which will determine the load at which the error on the meter will be the same as it has been over any particular period in dispute. I think the simplest way of dealing with a disputed meter is carefully to calibrate a set of single-phase meters (two for a 3-wire and three for a 4-wire load) and connect them in series with the disputed meter during a typical period. Single-phase induction meters working with current transformers can easily be calibrated to register correctly within 1 per cent on all power factors between unity and 0·5 lagging, and on all currents from $\frac{1}{4}$ to full load. An advantage of this method is that it can easily be explained to the consumer, and the meter in dispute need not be tampered with until he is satisfied. Demand-indicator attachments introduce errors in their mechanisms which are not constant and are commonly of the order of 2 per cent. As the demand charge is commonly the major item in the consumer's account, it seems to me that considerable improvements are required in these instruments. Referring to the author's definition on page 338 of the "mean inaccuracy" of the meter, I think it is rather too complicated, involving as it does the insertion of a sine meter in series with every alternating-current watt-hour meter (it must

* Paper by Mr. G. W. Stubbings (see page 335).

be there during any period which may be in dispute) and I suggest the following:—"The mean inaccuracy of the meter shall be taken as the weighted mean error between 1/10 load and full-load amperes and shall be calculated from a curve plotted from results of tests taken at a power factor of 0·8 lagging." This would not give special consideration to the consumer's actual working load, but would not involve any inquiries about the running of the consumer's plant. It could be a standard clause in all agreements, with possibly a modification of the power factor in special cases. With modern alternating-current meters I think it would give an error not differing, even in extreme cases, by 1 per cent from that obtained from the author's definition. It would be quite a simple matter to make tests at six or eight loads between 1/10 and full load, plot a curve, integrate it graphically, and abstract the mean error. After all, it is only when one party insists on his legal rights that these definitions matter. In most cases the insertion of a set of check meters by the supply authority is a method of determining the error which is satisfactory to both parties.

Mr. G. W. Stubbings (*in reply*): Mr. Howarth objects to the assumption that the retarding frictional torque of an inaccurate meter can be taken as constant throughout the working load. When friction is sufficient to be more than a negligible source of error, this assumption is usually sufficiently near the truth for the purpose of the investigation in the paper. In the case referred to by Mr. Howarth, in which errors under $\frac{1}{4}$ load were inconsistent, it is obvious that, strictly speaking, no method of testing could give the error of registration over any period other than that of the actual test. Instrument-transformer errors are certainly difficult to allow for in an investigation of the kind attempted in the paper. If, however, current transformers supplying integrating meters are of ample capacity, and are not used to operate any other instruments or gear, the errors should be very small for normal circumstances. Mr. Howarth gives a figure of 1 per cent for these errors over a wide range. Mr. Howarth's alternative definition of the term "mean inaccuracy" is interesting. The suggestion to test a disputed meter by connecting a calibrated check-meter in series, appears however to be preferable to the use of this definition. It may be noted that the method of testing by a check meter can be used in conjunction with the definition of "mean inaccuracy" given in the paper, by taking simultaneous revolution tests of the two meters at a time when the load is approximately the average required by the definition, any difference in power factor being corrected for by data obtained from the original test of the meter.

PROCEEDINGS OF THE INSTITUTION.

14TH MEETING OF THE WIRELESS SECTION, 25 MAY, 1921.

(Held at the Institution of Mechanical Engineers.)

Dr. W. H. Eccles, F.R.S., Chairman of the Wireless Section, took the chair at 6 p.m.

The minutes of the meeting of the Wireless Section held on the 16th March, 1921, were taken as read, and were confirmed and signed.

The Chairman announced the list of members nominated to serve on the Wireless Section Committee for the year 1921–22 (see *Institution Notes*, No. 26, page 22, May 1921).

A discussion was held on "Long-distance Wireless Transmission," with an introductory paper (see page 677) by Mr. C. F. Elwell, Member.

The meeting terminated at 8 p.m.

668TH ORDINARY MEETING, 26 MAY, 1921.

(Held in the Institution Lecture Theatre.)

The chair was first taken at 6 p.m. by the President, **Mr. Ll. B. Atkinson,** and later by **Mr. J. S. Highfield,** Vice-President.

The minutes of the Ordinary Meeting of the 5th May, 1921, were taken as read, and were confirmed and signed.

The President announced that the scrutineers (Messrs. F. B. O. Hawes, P. M. Baker, and E. Nash) appointed at the Ordinary Meeting of the 28th April had reported to him that they had examined the ballot papers for the election of new Members of Council and that the following persons had been found to be duly elected :—

President : J. S. Highfield.

Vice-Presidents : Professor E. W. Marchant, D.Sc., and C. C. Paterson, O.B.E.

Honorary Treasurer : Sir James Devonshire, K.B.E.

Ordinary Members of Council : S. Evershed, Lieut.-Colonel F. A. Cortez Leigh, T.D., R.E., Sir A. M. Ogilvie, K.B.E., C.B., A. Page, T. Roles, and A. W. Tait, C.B.E.

On the motion of the President a vote of thanks was passed to the scrutineers of the ballot.

After the President had referred to the reinstatement of the Institution in its building, a Lecture was delivered by Messrs. Alfred Johnsen and Knud Rahbek entitled "A Physical Phenomenon and its Application to Telegraphy, Telephony, etc."

A vote of thanks to the lecturers, proposed by the Chairman (Mr. J. S. Highfield), was carried with acclamation, and the meeting adjourned at 7.35 p.m.

49TH ANNUAL GENERAL MEETING, 31 MAY, 1921.

(Held in the Institution Lecture Theatre.)

Mr. Ll. B. Atkinson, President, took the chair at 5 p.m.

The notice convening the meeting was taken as read.

The minutes of the Ordinary Meeting of the 26th May, 1921, were taken as read, and were confirmed and signed.

The **President,** after summarizing the Annual Report of the Council for the year 1920–21 (see page 343), moved : —"That the Annual Report of the Council for the year 1920–21 as presented be received and adopted."

The resolution was seconded by **Mr. G. L. Addenbrooke** and, after being put to the meeting, was unanimously adopted.

Sir James Devonshire, K.B.E. (Honorary Treasurer) moved the following resolution : "That the Statement of Accounts and the Balance Sheet for 1920,* as presented, be received and adopted." In doing so, Sir James Devonshire said :

Income and Expenditure.—As will be seen by the printed Report which has been issued to members, there is a surplus on the Revenue Account for 1920 of £673 7s. 1d. after making the provision of £3 000 for the Reserve Fund, and this amount which has been carried to the General Fund, as compared with £1 495 3s. 3d. in 1919, shows a decrease of £821 16s. 2d. The total expenditure for 1920 is £28 010 8s. 10d., as compared with £24 097 16s. 1d. in 1919, an increase of £3 912 12s. 9d., the principal items occurring under the heads of "Management" which is up by £1 355 4s. 8d., the *Journal* which is responsible for an increase of £3 343, and the Territorial Centres of £680, Meetings, etc. On the other hand, there are savings under sundry heads, notably *Science Abstracts* £493, Special Grants £255, Conversazione £365, Research £345, all together amounting to £2 093. On the other side of the Account the Income shows a gratifying expansion by reason of the fact that the subscriptions for the year were £22 319 16s. 6d., as against £19 532 1s. 4d. for the previous year, an increase of £2 787 15s. 2d. The Life Compositions show a slight decrease of £6 4s., viz. £315 2s., as against £321 6s. a year ago, and as was also to be expected, in view of the larger sums invested, the dividends on investments have gone up and now

* See page 354.

stand at £1 775 9s. 4d., as against £1 387 5s. 0d. in the previous account, an increase of £388 4s. 4d. Another pleasing increase is the rent from tenants, viz. £6 615, as against £6 240 5s., an increase of £374 15s. This is due to the fact that the upper floors of our Building have been advantageously re-let at an increased rent. The net rental from our Tothill-street property has also improved from £529 14s. 4d. to £557 11s., an increase of £27 16s. 8d. due to improved rentals.

Balance Sheet.—The first item is Economic Life Assurance Society, which last year stood at £30 198 5s. 5d., and is reduced to £29 284 15s. 0d., by £913 10s. 5d., the amount of the obligatory repayment to the Assurance Society. The Reserve Fund (Mortgage Redemption and Contingencies) which last year stood at £11 000, now stands at £14 000, an increase of £3 000. The General Fund now stands at £82 370 16s. 10d., as compared with £80 834 12s. 8d. on the 31st December, 1919. On the Assets side the amount of the Institution Building and Lease is shown as £69 727 5s. 5d., as against £70 068 18s. 6d., the difference, viz. £341 13s. 1d., being larger on account of the fact that the surrender values of the Sinking Fund Policies have now been brought in for the first time in lieu of the Sinking Fund premiums paid in respect of the Policies of Redemption which have been taken out to secure the principal sum of £75 000 at the termination of the lease of the Institution Building in 1984. The only other item I need call attention to is the General and Reserve Funds Investments. Last year these stood at £26 985 1s. 7d., whereas this year they amount to £28 635 13s. 5d., an increase of £1 650 11s. 10d. Attention may be called to the Statement on page 351 of the Annual Report, relating to the Annual Accounts. The surplus of Assets over Liabilities is shown in a slightly amended form. This amended form is a departure from the method adopted in past Reports and substitutes the value of the actual properties shown on the Assets side, less the Liabilities, for the several Funds appearing on the Liabilities side of the Balance Sheet. I think members will find it clearer to follow the figures presented to them in this way, and I suggest that this method should be adopted in future Reports. In conclusion, I think the Institution has every reason to be gratified with its financial position as disclosed by the Accounts which are now submitted for adoption.

The resolution was seconded by **Mr. A. H. Allen** and, after the Honorary Treasurer had replied to a question by **Mr. F. W. Purse,** was unanimously adopted.

The following resolution, moved by **Mr. J. R. Cowie** and seconded by **Mr. J. Coxon,** was carried unanimously :—" That the best thanks of the Institution be and are hereby accorded to the Council of the Institution of Civil Engineers for the kind hospitality extended by them to the Institution in placing their meeting rooms and library at their disposal."

Mr. C. C. Paterson, O.B.E., then moved : " That the best thanks of the Institution be accorded to the following officers for their valuable services during the past year : The Honorary Secretaries of the Territorial Centres and the Local Honorary Secretaries and Treasurers abroad ; the Honorary Treasurer of the Institution, Sir James Devonshire, and the Honorary Solicitors, Messrs. Bristows, Cooke & Carpmael."

The resolution, having been seconded by **Mr. Charles Alfred Baker** and supported by the President, was carried with acclamation.

Mr. J. S. Highfield moved : " That Messrs. Allen, Attfield be appointed Auditors for the year 1921–22."

The resolution was seconded by **Mr. F. W. Crawter** and carried unanimously.

The meeting terminated at 5.45 p.m.

SPECIAL GENERAL MEETING, 31 MAY, 1921.

(Held in the Institution Lecture Theatre.)

Mr. Ll. B. Atkinson, President, took the chair at 5.45 p.m.

The notice convening the meeting was taken as read.

The President : I have to propose the following resolution :

" That the Council of The Institution of Electrical Engineers be requested to apply to the Privy Council for a Royal Charter of Incorporation and that the draft Petition and draft Charter therefore which have been initialled for purposes of identification by the Chairman of this meeting be approved subject to such alterations as the Council of the Institution may deem it necessary or expedient to make therein prior to the application to the Privy Council, and to such alterations as may be made therein by the Privy Council and approved by the Council of the Institution."

The necessity of this meeting and of the resolution may be explained briefly as follows. The corporate existence and constitution of the Institution are founded on the fact that we are a Company and that with the permission of the Board of Trade we can omit the word " Limited," as we are not trading for profit. If we desire to have any other constitution, it will be necessary first to obtain the permission of the members. What we propose to do is to present a petition for a Royal Charter to the Privy Council and with that petition to submit a draft of the form of Charter. All the Members, Associate Members, and Associates have already received a copy of the petition and of the proposed Charter. It may be said : What is the advantage of having a Charter and to what extent does it carry us farther than we are at present ? So far as our operations or our standing are concerned, the advantage is this. As long as we remain under the Company Law we are merely a self-appointed body ; and however valuable the work, and however powerful the membership of a self-appointed body, it can never get away from the suggestion that it is somewhat of the nature of a club and therefore has no public standing other than that which it may achieve by its work. If, however, we are successful in getting a Royal Charter, we are in the position that the State has acknowledged that this Institution is what it stands for, viz. the representative of the electrical profession and the electrical industry. That may not seem very much,

but I think it is really a great step in the opinion of the public. In the Charter we are asking that Honorary Members and other members shall be entitled to the exclusive use after their names of the initials "Hon.M.I.E.E." and "M.I.E.E.," and so on for the different grades; but in addition we are asking for the power to give to our Corporate Members the exclusive use of the designation "Chartered Electrical Engineer." Several of the other professional Institutions already possess Charters, and have adopted these or similar words; for instance, Chartered Accountant, Chartered Patent Agent, and so on. Consequently we selected the word "chartered," partly because it is one that the public is more or less accustomed to attach to members of a recognized body, and partly because the Institution of Civil Engineers is asking for additional powers under its Charter to use the words "Chartered Civil Engineer"; and possibly a useful practice may thereby be extended. We asked the members to send in their opinions on a postcard, and I will explain the reason why we did so. It may be that at some time during the course of our efforts before the Privy Council we may be asked to what extent this application for a Charter voices the wishes of our membership. Sometimes the attendance at Annual General Meetings and Special Meetings is not very large, and it would have been inconvenient if we had had to reply to an inquiry by the Privy Council that there were only, say, 50 members present at the Meeting who voted for the resolution and that we did not know the opinion of the remainder of the members. We therefore asked the members to reply on a postcard stating whether they were in favour of the resolution, and no less than 63½ per cent of our members have replied, all in the affirmative, with the exception of three. This will be sufficient answer to any question as to whether the membership as a whole is in favour of a Charter being applied for. With these few remarks I will now ask Mr. Roger Smith to second the resolution, which will then be open for discussion.

The resolution was seconded by **Mr. R. T. Smith.**

Mr. C. H. Wordingham: During my term of office as President I brought very prominently before the Institution the question of obtaining a Royal Charter and prepared a draft scheme in order to have something definite to talk about. I went to most, if not all, of the Territorial Centres; and there was a Special General Meeting called to consider the matter. Probably many members have wondered why nothing more has been heard of the proposal, but there was a very good reason for it, namely, that the Institution of Civil Engineers had introduced a Bill which had an important bearing on the question of the status of this and other Engineering Institutions, and until the fate of that Bill was settled it would have been altogether inadvisable to raise the question of a Royal Charter for our Institution. That Bill was dropped and is not likely to be revived. Therefore that clears the way for the reconsideration of a Charter. The Charter for which we are asking, however, is very different from the one which I outlined in my addresses to the Territorial Centres. I hope that it will later on be found possible very greatly to extend the scope of the Charter we are now asking for; but expediency has always to be considered in these matters, and the Committee considered that it would be better, at all events at this stage, to ask for a Charter which did not extend the sphere of the Institution. I therefore strongly commend the present scheme put forward by the Council.

After **Mr. Charles Alfred Baker** and **Mr. F. P. Sexton** had spoken, the President put the resolution and declared that it had been carried unanimously.

The meeting terminated at 6.20 p.m.

669TH ORDINARY MEETING, 31 MAY, 1921.

(Held in the Institution Lecture Theatre.)

Mr. Ll. B. Atkinson, President, took the chair at 6.30 p.m.

A list of candidates for election and transfer, approved by the Council for ballot, was taken as read and was ordered to be suspended in the Hall.

Messrs. H. Craske and A. Kirk were appointed scrutineers of the ballot for the election and transfer of members and, at the end of the meeting, the result of the ballot was declared as follows:—

ELECTIONS.

As Members.

Gridley, Sir Arnold Babb, K.B.E.
Howard, John Whiteley, M.Sc.
Jackson, Charles Albert.
Robins, William Harry.
Roche, Thomas.
Shaw, James Henry.

As Associate Members.

Bennett, Thomas Cyril.
Dawson, Alfred Ernest.
Dewar, Gordon Duncan, B.Sc.(Eng.).
Eves, Walter John.
Ferguson, William.
Gadsby, George James.
Gibbins, Frederick Edison.

As Associate Members—continued.

Grepe, Sydney Francis.
Hollinghurst, William.
Johnson, Edward.
Johnson, John.
Jones, Robert Griffith.
Macmillan, Frank Shaw.
Midgley, Harold, B.Sc. (Eng.).
Mills, Ernest A.
Payton, George Alfred.
Peterson, Ivan Emille Kerr.
Raworth, Arthur Basil.
Rhodes, Thomas Imrie.
Street, William Arthur.
Tillekeratne, Aloysius Gordon.
Warnock, James Wallace.
Webster, James.
Whitehead, Herbert Alfred.

As Graduates.

Basil, Bishen Duo.
Baster, Norman.
Bell, Herbert Harry.
Bradshaw, Arthur John.
Chambers, David Joseph.
Chatterji, Satyendra Narayan.
Coveney, Henry Percival R.
Damodar, Bhaskar.
Fletcher, Mark Noble.
Grant, Ridsdale Henry.
Hainsworth, Harold Percival.
Kay, Henry Herbert.
Keogh, Louis Joseph.
Lowe, John.

As Graduates—continued.

Lucas, Herbert James.
Mead, Reginald Henry.
Reuben, Enoch A.
Sharp, Donald Claude.
Still, Ernest Henry.
Stinchcomb, Ernest Arthur.

As Students.

Andrews, Robert.
Barson, Albert Clifford.
Bates, Harold Thomas.
Beynon, John Henry.
Bobby, George Edgerton.
Boyd, Henry Bamford.
Briggs, Arthur Longton.
Brown, Arthur MacArthur.
Cameron, Kenneth Albyn.
Clarke, Stanley Archibald.
Cooper, Arthur.
Cooper, Wilfrid Varley.
Crosbie, Sydney.
Davies, William Richard.
Edwards, William John.
Everett, Arthur George.
Gosling, Arthur John B.
Gurney, George Frederick.
Hamer, Joseph Bishop.
Hendry, William Masson.
Hewett, George Nele.
Hill, Douglas St. Clare.
Howe, Reginald Percy.
Keay, James Tosh.
Lawton, Charles Robert.
Lye, Rupert Graham.
Marlowe, Thomas.
Mitchell, Benjamin William, Junr.
Moseley, Ernest Gower.
Nicholas, James Douglas.
Pankhurst, Frank Arthur.
Parsons, Albert.
Paver, Thomas Righton.
Pinkney, William Halewood.
Pitt, Frank Ernest.
Pocock, Cecil John D.
Pooley, Leonard Arthur.
Roach, John Carlyle.
Russell, Carlton John C.
Scaling, Thomas Nawton.
Scrafton, Christopher Dixon.
Simpson, Frank.
Sleath, Frank Albert.
Stretch, William, Junr.
Towndrow, Frederick Francis.
Tucker, John Potterton.
Turner, Reginald.
Underwood, Thomas George.
Wheelwright, Geoffrey Whitfield.
Williams, John Dinwoody.
Wright, George.
Wylie, Thomas Oldham K.

TRANSFERS.

Associate Member to Member.

Bartholomew, Sydney Charles.
Blennerhassett, Richard Francis.
Cameron, John.
Cardrey, Ashby George.
Dunsheath, Percy, O.B.E., M.A., B.Sc.
Furlong, Harold George.
Gibbs, Arthur.
Glen-Bott, Carl Lotherington, O.B.E., M.A.
Greenwood, Edward Joseph B.
Henley, Frank Leete.
Hill, Henry, B.Sc.(Eng.).
Horner, Frank Henry.
Hughes, William Walter.
Kingston, John Rudolph, O.B.E., Major R.E.
Kitchen, Henry.
MacDermott, Claude Neville.
Richards, Henry Walter H.
Richardson, James.
Rycroft, Percy Edward, M.B.E.

Associate Member to Member—continued.

Shearing, George.
Spicer, Frank Parkinson.
Terras, James Sinclair.
Thomas, John Pattinson.
Waring, Richard.
Whillis, Charles.
Williamson, Arthur Frederick.

Graduate to Associate Member.

Carey, Theophilus Mattingly.
Edgar, George Thomas.
Mellonie, Stanley Richard.
Puttick, Harold Walter.

Student to Associate Member.

Sharpley, Forbes Wilmot.
Wright, John Edwin, Lieut. R.N.V.R.

Student to Graduate.

Heaton, Reginald George.
Hillman, Reginald Victor.
Ross, Victor.

The following donations were announced as having been received, and the thanks of the meeting were accorded to the donors :—

Benevolent Fund.—F. W. D. Adcock, W. Aitken, A. C. Baker, S. Baldwin, H. P. Bearcroft, C. A. Beaton, A. E. Bennett, A. W. Bennett, J. M. Birrell, W. Braithwaite, W. Brown, W. C. Brown, W. S. Carr, W. Carter, R. L. Cleaver, H. S. Clifford, H. Colebrook, A. W. Cox, C. T. E. J. Darbyshire, A. Douglas, G. H. Davies, The Electrical Engineers' Ball Committee (per Mr. A. M. Sillar), G. J. Evans, W. Eynon, S. Francis, R. D. Gifford, S. F. J. Gilliver, F. H. G. Goodwin, W. H. Hanks, C. Haywood, G. P. Henderson, E. A. Hick, W. Higson, F. S. G. Hinings, J. Holmes, H. J. Howard, G. Hunter, H. H. Jacques, W. H. M. Kelman, W. Kidd, E. M. Lacey, F. A. Lawson, A. Lingard, D. S. Maclachlan, T. H. Matthews, H. J. O'Dell, B. Paul, D. Pender, S. F. Philpott, H. V. Postlethwaite, C. Redman, C. E. Rickard, F. E. Rowland, A. Sevalho, Herbert Smith, J. A. Stokes, W. C. P. Tapper, H. Taylor, R. St. G. Terry, E. O. Turner, F. Walker, H. P. Walker, J. Walker, J. Waring, and D. Wilson.

Dr. F. B. Jewett delivered an Address entitled "Research Work in the United States," and was followed by Sir Frank Heath, who gave a brief account of Government research work in this country.

A vote of thanks to Dr. Jewett and Sir Frank Heath for their Addresses was proposed by Mr. C. H. Wordingham, C.B.E., seconded by Mr. C. C. Paterson, O.B.E., and carried with acclamation.

The meeting terminated at 7.55 p.m.

15TH MEETING OF THE WIRELESS SECTION, 8 JUNE, 1921.

(Held in the Institution Lecture Theatre.)

Dr. W. H. Eccles, F.R.S., Chairman of the Wireless Section, took the chair at 6 p.m.

The minutes of the meeting of the Wireless Section held on the 25th May, 1921, were taken as read, and were confirmed and signed.

The Chairman announced the award of Premiums for papers read before the Wireless Section (see *Institution Notes*, No. 26, page 22, May 1921).

A paper by Professor J. S. Townsend, F.R.S., entitled "Electric Oscillations in Straight Wires and Solenoids" (see page 771), was read and discussed, and the meeting terminated at 8 p.m.

BENEVOLENT FUND.

23RD ANNUAL GENERAL MEETING, 26 MAY, 1921.

(Held in the Institution Library.)

Mr. Ll. B. Atkinson, President, took the chair at 5.45 p.m.

The notice convening the meeting was taken as read.

The minutes of the 22nd Annual General Meeting held on the 20th May, 1920, were taken as read, and were confirmed and signed.

The Report of the Committee of Management (see below) and the Statement of Accounts for the year 1920 (see page 359) were presented and, on the motion of the President, seconded by Mr. F. W. Crawter, were unanimously adopted.

It was proposed by the President, seconded by Captain R. J. Wallis-Jones, and unanimously agreed, that Mr. J. Attfield, F.C.A., be elected Honorary Auditor for the year 1921–22.

The President reported that under Rule 10 the Council had appointed the following Committee of Management for 1921–22 :

The President (*ex-officio*),

Mr. H. J. Cash,
Mr. H. W. Clothier,
Mr. J. R. Cowie,
Sir James Devonshire, K.B.E.,
Dr. A. Russell,
Mr. J. Sayers, O.B.E.,
} Representing the Council ;

Mr. W. B. Esson,
Mr. J. E. Kingsbury,
Colonel H. C. Sparks, C.M.G., D.S.O., M.C.,
} Representing the Contributors ;

and the Chairman of each Territorial Centre in the United Kingdom.

The meeting then terminated.

REPORT OF THE COMMITTEE OF MANAGEMENT FOR THE YEAR 1920.

Capital.

The Capital Account stood on the 31st December, 1920, at £5 709 9s. 6d., all of which is invested.

Receipts.

The Income for last year from dividends, interest and annual subscriptions was as follows :—

	£	s.	d.
Dividends on investments ..	296	14	4
Interest	12	0	8
239 Annual subscriptions ..	211	18	6
	£520	13	6

In addition to the foregoing the Fund benefited during the year by the following, many of which are non-recurring donations :—

	£	s.	d.
City of Glasgow R.E. Volunteers	197	0	11
R.E. Volunteers	31	9	6
Warwickshire R.E. Volunteers	30	0	9
Midland Electrical Engineers' Ball Committee	30	0	0
W. T. Henley's Telegraph Works Co., Ltd.	25	0	0
Twenty-five Club	15	15	0
Incorporated Municipal Electrical Association	10	10	0
A. O. Buckingham	10	10	0
T. M. Harvey	5	5	0
J. D. Dallas	5	0	0
R. E. Grime	5	0	0
and 611 non-recurring donations of under £5	243	4	1
	£608	15	3

The accumulated Income now amounts to £2 533 1s. 9d., of which £2 149 17s. 9d. has been invested.

Donors and Subscribers.

Lists of the names of donors and subscribers during 1920 have been published in the *Journal*.

A special appeal for subscriptions to the Fund was made by the Committee at the end of 1920.

The Committee of Management desire to acknowledge their indebtedness to the donors and subscribers, and they venture to urge upon members the pressing need for a generous support of the Fund. Apart from donations, the Committee will be grateful for annual subscriptions even of small amounts.

Grants.

Twenty-eight applications for assistance were received in 1920, and the Committee, after due consideration, made grants in all the cases. One grant of £30 was made, three of £25, two of £20, one of £16 5s., one of £11 1s., eleven of £10, two of £9 15s., and one each of £9 9s., £9 3s., £6, £5, £4, £3, and £2 5s., a total of £340 13s.

Offer from Mr. E. M. Hughman.

An offer has been received from Mr. E. M. Hughman, of Bombay, of a donation of £250 to the Benevolent Fund, on condition that 19 similar donations are received before 30th June, 1921. The Committee have obtained Mr. Hughman's consent to the sums of £250 not being contributed solely by single donors, but by group subscriptions. The London members of the

Committee and the Territorial Centres are taking steps to organize such group collections, which will doubtless be supported by members of the Institution of all classes.

It may be well to recall briefly the position of the Fund.

The Fund was founded over 30 years ago (1890) and up to the present time the accumulated funds only amount to a little over £8 000. The revenue from this sum is about £310, and the normal donations and subscriptions amount to about £500 a year. The outgoings at present are not very large. The total expenditure during the last two years has averaged £325 per annum, and all deserving cases have been reasonably helped.

The electrical profession and industry are yet young, its members have in the past been fairly successful, but all experience shows the probability of much larger expenditure by the Fund in the near future.

The necessity of strengthening the Fund has been fully realized of late and has led to Mr. Hughman's generous offer.

Wilde Benevolent Fund.

The Capital Account stood on the 31st December, 1920, at £2 586 19s. 2d., the whole of which is invested and brings in an annual income of £89 12s. 5d.

The balance standing to the credit of the Income Account, from which full members only can benefit, on the same date was £97 2s. 7d.

No grant was made from this Fund during the year.

DEVELOPMENT OF ARMY WIRELESS DURING THE WAR.

By Lieut.-Colonel A. G. T. Cusins, R.E.

(*Lecture delivered before the* Wireless Section of The Institution, 3 *April*, 1919.)

(1) Historical, up to an Early Period of the War.

The first connection of the Army with wireless telegraphy was in 1898, when this development was more or less in its infancy. An officer then accompanied Signor Marconi on experiments in the Solent, and we read with interest now his report that "communication could still be kept up with the 'Osborne' from Newton Bay, a distance of over seven miles." And in 1899 the fact that overland communication was found quite feasible in addition to oversea communication adumbrated the possibilities of the use of wireless telegraphy for Army purposes.

In 1901, trials were actually made in the field in South Africa, but failed owing to difficulties in erecting suitable aerials and also in connection with earths.

In the Army Manœuvres of 1903, however, the 1st Army Corps had attached to it two stations supplied and manned by Messrs. Marconi, and the 2nd Army Corps two by Messrs. Muirhead, and officers and men were attached for tactical supervision and for instruction. Later on, in addition to experiments in producing mobile wagon-sets, trials were carried out with pack sets in 1906 on the Lodge-Muirhead and Telefunken systems, and later with sets on the Marconi system. Trials with the last-named were somewhat delayed because the sets on order for the Army were handed over under some misapprehension to an enterprising Yeomanry officer. As a result of trials, however, three new sets were procured in 1910.

By this time, sets were being reduced to a form in which they could be used in the field, and definite wagon and pack sets were evolved, and in 1913 wireless telegraphy was definitely accepted as an integral portion of the British Expeditionary Force. During the manœuvres of that year, which closely approximated to the conditions expected on real active service, many defects were discovered and rectified, and the theory of the tactical application of wireless telegraphy had more or less crystallized. As an adjunct to a highly organized Signal Service on a line-telegraphy basis, it was very naturally considered to be better adapted to working with cavalry, on account of the difficulty of accompanying this mobile arm by other means. Hence, on the outbreak of war in 1914, wireless telegraphy was organized and equipped entirely on this basis, its function in the general scheme being to provide communication between the cavalry and G.H.Q. of the Expeditionary Force.

This, of course, represented only the accepted official view of the matter as applicable to Army Establishments. But experimental work had wandered further afield, and the questions of the more general application of wireless telegraphy, and in particular the problem of communicating with aircraft, had been considered. A good deal of experimental work had been carried out with the Royal Flying Corps, and the practical utility of wireless telegraphy in this connection had been definitely established. It is a trite saying that success in war depends on the perfect co-ordination of all arms, and it is not too much to say that without this experimental work the Royal Flying Corps would have been severely handicapped.

In addition, the use of wireless telegraphy for intelligence purposes had been considered, and progress had been made with a motor set. The experiments with this, carried out by Captain Blandy, had shown that wireless telegraphy had still other possibilities from a military point of view. However, war is a very serious matter, and, although the soldier is often accused of being conservative, to ask him to accept a new weapon practically untried is to ask a great deal. Therefore, wireless telegraphy started on the great adventure as "possibly a useful adjunct to visual and line signalling" and, in spite of the valuable work of the Experimental Section at Aldershot and of certain Territorial wireless units, notably the London, Leeds and Scottish sections, it was still an adjunct and had no separate organization and but a half-recognized position.

In this connection I would mention that the one wireless telegraph lorry, after great difficulty, was sent abroad in charge of one officer, Captain Lefroy, and two N.C.O.'s, and had to beg personnel from the Service Wireless Section at Boulogne. And yet we find this set, on 19th August, 1914, working in the garden of the Town Hall at Le Cateau, engaged in intercepting enemy messages and giving valuable information to the Intelligence Department concerning his dispositions and movements.

Thus from the outset we find Army wireless telegraphy in a hitherto unrecognized rôle, performing duties the subsequent extensions and ramifications of which have been among the outstanding features of war by land, sea and air.

Meanwhile, the sets of the authorized establishments carried out their normal duties, the pack sets with Cavalry Brigades, the wagon sets with Cavalry Divisional Headquarters and General Headquarters. They took their part in the dramatic events of the early days of the war of movement, undergoing many vicissitudes, including the escape of a station during the retirement from a point actually behind the German line. About the time of the Battle of the Marne, in the early part of September, more motor sets began to arrive, and

gradually replaced the wagon sets in the line, but the new sets were still looked upon as cavalry adjuncts, although we find them used for purely administrative work and engaged in relieving the congestion of signals traffic on lines of communication, and also maintaining the service of interception.

During this period, and particularly during the move of the British Army bodily to the North of the line, wireless telegraphy proved its utility. But by the middle of October the war had settled down to the long-drawn weariness of those siege operations which lasted, with a few convulsive intervals, until 1918.

(2) Subsequent Developments in the Field.

The second phase of the war revealed totally different conditions. The mobile cavalry force, calling for a free and independent system of signals, disappeared. The contact line became more or less fixed, a whole cable and air-line service came into being, and the original tactical use of wireless telegraphy was no longer its primary function. But the value of what had been its secondary function was by this time established, and for some considerable time it became almost exclusively an Intelligence accessory. And as such it was undoubtedly a success.

At the same time, the settling down of the war into a stationary condition opened the way to a series of experiments to the end of perfecting wireless telegraphy as a supporter of the ordinary Signal Service.

Apparatus at this time was not available in large quantities and consisted, in all, of one wagon set which had gone all through the campaign, five lorry sets of somewhat similar pattern designed at Aldershot, and four lorry sets made by Messrs. Marconi since the outbreak of the war. All were $1\frac{1}{2}$-kW spark sets.

December 14th, 1914, marked the commencement of a new epoch. The first set making use of the 3-electrode valve, a Marconi direction-finding set under Lieut. Round, was shipped at Southampton. Although direction-finding had previously been carried on with some considerable success by the older method of crystal reception, the comparative lack of sensitiveness of this system had greatly limited its utility; but the introduction of the valve with its extraordinary sensitivity and powers of amplification revolutionized the whole science and led to the inauguration of a widely extended locating and intercepting system which from small beginnings grew until it constituted an important department of the Intelligence system, not only on the Western front but wherever an enemy wireless station could be heard and located.

It is common knowledge that aircraft, particularly at night, can navigate with any certainty only by wireless means, and it is to the credit of the Army intercepting stations that they succeeded in solving the navigation code of the German "Zeppelins" simultaneously with its solution in England. To digress for a moment, it may be pointed out that the Intelligence Service lives on scraps of messages in code and cipher. Fragments of intercepted messages, however small, picked up by wireless telegraphy, may be of value, for what one station misses another may catch, and every bit may possibly form an integral portion of the puzzle on which the Intelligence Service has to be ceaselessly engaged. During the war we used the old terms brought up to date, and the word "surprise," which a century ago applied to periods of time reckoned in minutes, now has a wider significance. It is a surprise nowadays if one can put in 10 days' undiscovered work on an offensive which will take two months to prepare. It is of the utmost importance therefore to pick up the earliest indications of movements of troops, concentration of stores, etc., incidental to action on a large scale. And it is in this work of what amounts to reconnaissance behind the enemy's line that wireless telegraphy has been of so much value. Wellington said that the art of war was to know what was happening on the other side of the hill. The aeroplane now gives us that information. What we have to know is what is going to happen some weeks ahead.

As a contrast, warnings to areas about to be shelled in the next few minutes may be cited as an instance of the varied uses to which wireless interception may be put. By subjecting the wireless activities of enemy aircraft and the liveliness of enemy batteries to a process of comparative analysis it became possible to connect the two, and to discover which particular aeroplane "flights" normally observed for any individual batteries. Knowing the targets likely to be fired on by these batteries (for the Boche was nothing if not systematic), the detection and location of the wireless messages of enemy aircraft enabled warnings to be sent to the areas most likely to be shelled. In addition, the location of the observing aeroplane could be notified to the Royal Air Force "for necessary action."

It is a mere step in another direction to point out the extreme value of this work in the early days of Zeppelin attacks on England. A ship at sea navigates by certain constants; the horizon is a fixture, the tides are fairly regular, the heavens revolve as laid down in the Nautical Almanac and, in territorial waters at any rate, the soundings and the nature of the bottom are charted in detail. The mariner, therefore, should know where he is to within a mile or so. But the aviator occupies a point in three-dimensional space with no reference marks, and at the commencement of the war his only means of navigation was to "make his number" by means of wireless telegraphy, receive his magnetic bearing from two or more fixed stations, and resect his position accordingly. Therefore, as soon as his compass code was discovered, and there is practically nothing that cipher experts will not solve, our Intelligence Department was able to follow with the greatest accuracy the movements of attacking aircraft. As a definite case, I would mention that two of our stations in France, tracing the course of a Zeppelin across the North Sea, gave the point at which it crossed the coast line of England, to within a mile of the point reported by the local police. I need hardly mention the relief of strain on an Anti-aircraft Service which is effected by such localization of the probable danger area.

This portion of Intelligence work became a definite

part of the routine work of Army wireless telegraphy, and continued so throughout the war. It was naturally of the utmost value, and was spread over every theatre of land warfare. Of what the Navy did in this direction we heard but little, but what we did hear made us think very much more. The Germans themselves certainly gave them full credit.

Now we come to a new development, not indeed wireless telegraphy but of somewhat similar nature, and important not only technically but as supplying one of the links which attached wireless work to the fighting zone as distinct from the comparative calm of interception and compass work. I refer to the overhearing of enemy telephone and buzzer signals in the actual trench line. The instruments employed were operated by personnel who handled the earlier forms of trench wireless apparatus. I mention the subject here, as it is another branch of Intelligence work, and marks the introduction of valve work into the trench line.

There is no doubt that the Germans were ahead of us in this branch. In 1915 there were frequent reports that they were aware of what was happening on our side. Battalions relieving others in the line in secrecy and under cover of darkness would be greeted in the morning with shouts of welcome from the enemy's trenches. On one occasion a battalion took over the line to the strains of its own regimental march played on a German cornet. Other examples could be cited in conditions that ruled out the possibility of spies being at work. The trouble was eventually pinned down definitely to the indiscreet use of our own telephones. Now, this state of affairs was extremely dangerous. The presence of a strange battalion would perhaps indicate the arrival of a new division from another part of the front.

The principle of the listening sets is simply that of picking up minute earth currents, and magnifying these, which are normally too weak to detect, by means of a valve amplifier. The collecting loops and earths were laid out in "No Man's Land," sometimes being buried actually in the enemy's parapet, and cabled back along the communication trenches to the listening posts. This work called for the enlistment of special interpreter operators, and produced some rather heterogeneous drafts, one including a clergyman, two conjurers and a Russian chef.

The introduction of this system was of the greatest value in picking up enemy telephone conversations and buzzer signals, and also had a most salutary effect on those of our own people who happened to be indiscreet. The first sets used by us were provided by the French, who had been carrying out for some time a series of systematic experiments with valves. Since that time, of course, the construction of valves for military purposes has become a regular function of many of our British incandescent-lamp firms.

It will be understood that the whole of the work was in the highest degree confidential, as regards both its nature and the messages handled. Being conducted in the forward zones it was also a service involving all the risks incidental to the territory lying between the reserve line and the enemy's wire.

The early days of trench warfare, however, were marked by one great development. Held up by the extraordinary resisting power of the trench system, recourse was had to the use by both sides of ever-increasing forces of artillery. One of the effects of this was to render line communication more and more precarious, and a very fair estimate of how serious it became can be formed by examining the honours list of the Signal Service.

So we find that early in 1916 wireless telegraphy was beginning to be tried in two forms, (1) transmission through the earth, and (2) ordinary ætheric transmission. The former system, which though not technically wireless telegraphy was operated by the same personnel, was adapted from the French "*telegraphie par sol*," and from their instrument known as the "*parleur*" was evolved the British "power buzzer" in which the primary circuit, actuated by a 10-volt battery, induces in the secondary circuit a vibrating current which is passed to earth along an insulated wire connected with two groups of earth pins. It is desirable with this class of instrument to have a long base between earth groups, up to about 600 yards. Reception on a 3-valve amplifier was good at from 2 000 to 5 000 yards, according to the nature of the soil.

In later types, such as the power buzzer amplifier, the set was arranged for both-way signalling, and fitted with a commutator to enable the instrument to be connected to as many as six bases so as to get good directional results. Such an instrument naturally proved of great value in forward trench work. It was very unobtrusive and hard to locate, and it held an honourable position up to the cessation of hostilities.

In ætheric transmission one of the earliest forms was a 30-watt spark trench set. This set was designed in France in the latter part of 1915 and was perfected in the Inspection Division at Woolwich and supplied to the Front in large numbers, beginning about March 1916. It was simple and easy to manipulate. It came into use in the trench warfare leading up to the Somme offensive.

Later came a smaller set, extremely portable, designed in 1916 and making its appearance in numbers in the autumn of 1917. This set was known as the Loop set, or Forward Spark 20-watt "B," and was especially intended for use by unskilled operators. The wave-lengths of this set are fixed at 65 and 80 metres, and are not alterable. The forward transmitter is in two parts, the "upper" unit containing the spark-gap and condenser and carrying the loop antenna. The whole clips on to a bayonet which supports it. The "lower" unit consists of a high-tension unit and sending key, and can be taken underground. In the rear transmitter the two units are combined and the aerial is carried on two tripods. The receiver is the same in each case and consists of a 2-valve receiver with adjustment for filament heating only.

Another and more powerful type of spark set, consuming about 130 watts, is the Wilson set which was mainly used by Corps H.Q. for control purposes, and also by the Royal Air Force for ground station work. This set was actually one of the earliest and is still one of the best. In it a motor-driven interrupter

was introduced, interruption taking place at a good musical frequency, and the circuits being so arranged that the interrupter frequency is such a factor of the spark frequency that both make and break occur at the zero of battery current, thus obviating commutator sparking, and also that the amplitude of oscillation during the period of "make" is a maximum. It is a very interesting and useful set.

A more recently designed 120-watt set was, at the termination of hostilities, in the form of a finally completed model. It is not likely to be reproduced in bulk, but detail work on some of its parts provided valuable material in other directions, notably in the design of "high-tension units." This is the Service designation of apparatus for producing high-tension current from portable accumulators, very necessary adjuncts of forward instruments.

The design of the first continuous-wave transmitter was commenced in France in 1916, and it is interesting to record that the first sets were made at Havre by German prisoners of war. Knowledge of valves was still somewhat scanty, and the transmitting valve as now used was not yet evolved. French receiving valves were used at first, and the sets were tried in the field. Hitherto, supplies of spark sets had been mainly absorbed by the Infantry, and, as artillery observation had begun, in common with other services, to suffer from interruption of wired communication by artillery fire, the new continuous-wave sets became at first more particularly used for forward observation work, and continuous-wave working was for a long time almost peculiarly an Artillery monopoly.

Another set, designed in England in 1916 and modified after trial in France, became W.T. set, Trench, C.W. Mk. I. Many of these are still in use.

A further development, Mark II, was found, when the W.D. Signal Factories were established, to be unsuitable for rapid reproduction, and existing stocks were taken over by the Tank Corps, in which service they did excellent work towards the end of the war.

The present Mark III is the up-to-date outcome of these earlier types. It is definitely a trench or forward set, power being supplied to the transmitter by accumulators and high-tension units. The receiving group consists of the tuner, the heterodyne wave-meter and a selector which may be used or not as desired, its function being to enable the full range of wave-lengths, i.e. 350 to 2 000 metres, to be obtained in any aerial from 40 to 150 feet in length, with a considerable amount of freedom from spark interference.

A later type of receiver has been designed to reduce jamming from spark stations to a minimum by means of a particularly loose coupling forming part of the instrument itself.

A more recent set, designed for a special purpose, and having a range of wave-length from 2 000 to 3 000 metres, is the 60-watt continuous-wave set. The conditions imposed as regards the type of aerial to be used necessitate the employment of a highly sensitive type of receiver, the drawbacks of which can, however, be countered, in the case of more generous aerial conditions, by the employment of tuned high-frequency amplification and frame reception.

Of larger sets, also of recent date, we have the 120- and 500-watt sets. A 250-watt set has also been produced in small numbers, to meet special demands, but this type is not likely to be made in any quantity.

The 120-watt set, which is of the semi-portable type, is designed for general use on lines of communication, and has justified its existence during the general dislocation of wired communications brought about by the rapid movements of the Army during the period just previous to and succeeding the Armistice. Its safe range of working may be taken as being about 200 miles, and its effective range of wave-length from 600 to 6 000 metres, though its range of action is somewhat reduced on the higher wave-lengths. The whole consists of one case of instruments, the batteries for operating, and the portable aerial gear, and can be conveniently carried in a 20-cwt. motor van. The instruments consist of a transmitter complete with a rotary converter for supplying high-tension alternating current, a rectifier which, by utilizing two special valves, can reconvert this into almost pure high-tension direct current, and a receiver fitted with a separate heterodyne. This latter is so arranged that it can be used as a wave-meter for tuning the transmitter. At the back of the case is a switchboard specially arranged for facility of testing. A T-aerial of two wires 300 feet long is employed, supported on two portable 48-ft. masts.

The 500-watt set is also fitted in a case, the rotary converter, however, being separate. In this set a rectifying unit for transmission is not provided. Its effective range may be taken as being about 800 miles and its range of wave-length up to 2 500 metres for transmission and 5 000 metres for reception. The same aerial is used as for the 120-watt set, but is carried on 70-ft. instead of 48-ft. masts.

In addition to this work of producing sets for pure signalling, the design of a field compass station is practically complete. A 2-ft. square frame constitutes the receiving aerial, which will perhaps give a better idea of the efficiency of modern reception than I could convey in words. Much of the interception work done during the war was carried on with the roughest of apparatus, but it is now desirable that a definite type should survive which embodies the best of the results so obtained.

(3) Necessity of Central Research.

Before the war there had been a small experimental staff at Aldershot at work since the year 1903. The personnel of this staff was absorbed in the mobilization of the Expeditionary Force, and the duty of providing wireless telegraph apparatus devolved on the Inspector of R.E. Stores at Woolwich. It was obvious, however, that this officer could not be in touch with Service requirements which were growing day by day and, moreover, his normal work, like that of everyone connected with the supply of stores for the Army, increased and multiplied both in volume and in kind at an enormous rate. However, experiments had to go on, and early in 1915 a definite Section for this work was brought into being under an officer on sick leave from France. This Section in its turn grew in size and in the scope

of its activities, until in the autumn of 1916 it broke away and started an independent existence as the Signals Experimental Establishment under Colonel A. H. Bagnold.

That is what happened, produced by the logic of circumstances. But the object of this portion of my lecture is to point out the necessity for such an Establishment and, though the necessity is thrown into more immediate relief by the stress of war, it is almost equally existent in the affairs of peace.

The object of those entrusted with the supply of apparatus for use in the field was, first, to ascertain what was required and to keep touch with the growth of opinion of officers in the field, and at the same time to endeavour to keep those at the Front cognizant of the developments in science at home; and second, and most important, to make production keep pace with both requirements and developments, and to deliver the goods to the troops in the field.

Now, research naturally could not be carried on conveniently in an actual theatre of war. Even at a base, repair of existing gear would elbow experimental work out of the workshops, to begin with. But, quite apart from this, it is necessary for anyone engaged in research to be in touch with the work of others similarly engaged, even though their objects in view be widely different. It was therefore essential that experimental and research work should be housed in conditions which would allow those working for the Army to maintain a close liaison with those working for the other Forces, and at the same time to keep touch with the manufacturing possibilities both of Government factories and of private firms. Those who have been in any way connected with the supply of munitions will know how minutely manufacturing details had to be considered; how even the number of screws in the lid of a box was a matter of concern. In addition to the design of actual apparatus, moreover, there is what may be called incidental design of such instruments as wave-meters, inductance and capacity bridges, and many other laboratory aids to design, manufacture and inspection. These call for the very highest skill and care, which can hardly be readily at disposal with an army in the field; and all such work must therefore be relegated to home activities.

An outline description of the organization of design and supply may be of interest. Monthly meetings were held, attended by representatives from the Signals Service in the field, from the War Office, both General Staff and Supply, from the Signal Factories, the Inspection Branch and the Experimental Staff. At these meetings everything from general policy to manufacturing details was discussed, and the proceedings were of the greatest value. A definite procedure governed the production of apparatus from inception to final inspection, in which care was taken that all concerned were consulted and kept posted.

Apart from this, lists of experiments and records of all research were kept up, and pamphlets were prepared giving full details and working instructions for all instruments. In addition, a monthly report was drawn up, giving an account of progress made. This report was circulated to the other Services and to our Allies, and we in exchange received copies of similar literature issued by them. It is a matter of great satisfaction to be able to say that relations between the research branches of the Services and of our Allies were throughout of the most cordial nature. To the French we owe a great deal, more particularly in connection with early work on valves; and the Americans entered whole-heartedly into the coterie in the same spirit. Co-operation was by no means confined to paper, whether in the form of official productions or direct correspondence, but personal touch was kept up by fairly frequent visits between the various establishments. Nor can we exclude the private firms, whose ideas and discoveries were equally at our disposal.

This was one of the better memories of the war; the devotion of all effort to the one object, and the sinking of personal and trade interest in the common cause.

All of this was very necessary; it was only by a staff immediately connected with the Army—they were by no means all soldiers—that research could be directed to the production of apparatus for field use in land warfare. There is a tendency to treat research as a matter by itself, but it is not, and cannot be, any more than mathematics can be. Each Service in the war had to work out its own problems along its own lines as limited by the conditions under which it had to work, and the same is true of peace organizations. By all means co-operate and establish what we call liaison, but do not trust to spoon-feeding. We are a race of individuals, not a herd, and if we are to succeed in the future in the higher branches of industry it will be rather by the cumulative effect of local and sectional effort working in a general fraternity than by the ponderous action of a centrally directed mass. It has always been so. Our best work has been the result of the activities of individuals and small groups. The "thin red line" is more than a soldier's fairy story. It is the general expression of our national character.

(4) Evolution of Service Apparatus, as Influenced by Extension of Application and Development of Organization.

I have very often heard it urged that the Army is much too particular about detail in its stores, and have been asked why we do not simply use a trade article straight from the shelf. The first answer in nearly every case is that this practice would in the first place throw out of gear all our packing arrangements, and anyone who has had anything to do with Army packing, whether in the channels of supply or as part of field organization, will realize what this means. The second answer is generally that the inevitable variations in design of minor parts would create difficulties in repair.

I have indicated to a certain extent how types of apparatus were evolved from the Service point of view, but this portion of my lecture is devoted to the more technical considerations which govern design.

It is obvious that, as the need for greater numbers of instruments became felt, the strain on the producing

organization increased, and the necessity for standardization of design and the definite adoption of policy became more and more insistent. It was naturally the case at first that matters in this respect were somewhat chaotic. Sets were procured from various sources for trial and comparison, very often on their merits as sets alone, the designers not having much, if any, knowledge of the conditions under which they would have to be used. Undesirable features were perpetuated because something was wanted at once and there was not time to re-design, and so on. The lack of general policy with regard to the use of wireless telegraphy, which had to establish itself in the confidence of those who would have to rely on it before any such policy would be formulated, in itself produced a vicious circle from which escape was difficult.

It is of interest to note that as late as 1917 a conference was held by the German Staff at which Hindenburg gave his opinion that the advantages of wireless telegraphy were outweighed by the dangers attending its use, and that it should be treated as a subsidiary branch of Flying Corps communications—in fact as a necessary evil connected with that Service. He was over-ruled, however. This shows that the enemy must have passed through their early ailments much as we did. Hindenburg's argument, by the way, was that his compass stations had been able to follow the intentions of the Russian Staff who were using wireless telegraphy freely throughout the battle of Tannenburg, and that the Germans won the battle by this means.

But as better results were obtained opinion was better formulated, policy began to crystallize and, as a result, method of design and direction of research took definite shape in their turn. In fact, the vicious circle slowed down and began to revolve in the opposite sense. It therefore became necessary to organize production and to speed up design. Everything possible is standardized, reducing draughtsmen's work to the minimum. Moreover, the system of production ensures that by the time the final model has been tested and approved, every detail has been examined from a manufacturing point of view.

As a definite case of the working of this system, the necessity for a certain class of set was conveyed to us in the middle of May, 1918. The laboratory experiments lasted until the end of July. Had the work in our factories proceeded at war pressure, the bulk supply would have been completed early in March, 1919. This set involves, as entirely new work, a high-tension unit, transmitter, tuner, high-frequency amplifier, and heterodyne wave-meter.

It would, of course, be unfair to the Service in the field to load it heavily with large numbers of a new type of apparatus with which the operators were unfamiliar. The practice has arisen of continuing to make by hand, in the experimental workshops, a certain number of instruments or sets for instructional purposes to bridge over the gap between the appearance of the approved experimental pattern and the delivery of the manufactured article in bulk.

As regards types, there are three main categories :— Stationary for permanent fixed stations; Semi-portable for stations normally stationary but capable of being moved by ordinary road transport; and Forward sets in a form that enables them to be carried and used in the actual battle line.

With the stationary type we have not been very much concerned, but in semi-portable work we have evolved the 60-watt, 120-watt and 500-watt sets already referred to.

But it is when we come to the design of forward sets that ingenuity is taxed. In the first place we require a source of power, and it is this above all that limits efficiency. Various attempts have been made at providing manual power, but we have never come across any device that can be called really satisfactory. So in the main we have to adopt storage batteries with all their drawbacks. They require constant re-charging, which means constant traffic to and from the Front in stationary warfare, and relays in moving warfare, for naturally a charging set, as a key portion of the whole organization, must be fairly protected. Protection is a matter of distance and power is a matter of lead, and the ampere-hour in the front line can be expressed in foot-tons. Experiment has, however, resulted in the design and adoption of a really unspillable cell.

This limits the power of transmission, and therefore the range, not so much of transmission as of reception, as the receiver must be fairly close to the weak transmitter so as not to be jammed by more powerful sets at a greater distance. This is of course the main reason why the tendency has been to rely more and more on continuous-wave working, on account of the very small range of frequency through which the heterodyne is audible, and the consequent possibility of elimination of undesired signals by a comparatively small change in the wave-length.

It has also led to the partial abandonment of what is known as the auto-heterodyne, which would be highly convenient if sets were very few and far between, but which, as it causes the receiving set to oscillate back in its own aerial, causes interference with other sets in the neighbourhood. It remains to be seen whether it will be possible to employ the auto-heterodyne method by reception on a revolving frame of limited transmitting power but having also selective properties due to the directional nature of its reception.

(5) Present Position and Trend of Possible Advance.

Our present position is that wireless telegraphy and telephony have steadied down from the early days of more or less haphazard trial and introduction, and have inspired sufficient confidence to be given a definite place in the general scheme of communication. Unless this growing confidence can be completely justified there is just the danger of a set-back. The problem we have to solve is somewhat complex. In the nature of things, wireless telegraphy and telephony are likely to be specially required in the forward zones of warfare. It is here that the intense effect of modern artillery fire has practically ruled out the use of wired communication. It is also in this zone that instruments have to be most numerous, and have by their nature and position

to contend with the largest amount of jamming. The best instruments will be those with the greatest selectivity. It must be remembered that an Army Corps Headquarters, say some 15 miles behind the battle line, controls three to four Divisions each covering about four miles of front, and each Division has three Brigades in line. Thus in an area about 15 miles square there will be a Corps H.Q. communicating with neighbouring Corps, and with three Divisions, three R.G.A. Brigades, R.A.F., Tanks, etc., each Division linked with its neighbouring Divisions, its own Brigades and its Artillery, and finally each Brigade linked to its Battalions in the line. If we add an enemy on the other side of the line doing something similar, and the rival Air Forces overhead, we get a state of affairs which is, to say the least of it, congested. Much, of course, can be effected by the use of continuous-wave working, which not only gives higher power for a given weight, but also lends itself to selectivity as regards wave-length. But heavy spark-jamming is very difficult to deal with, even with the loosest of coupling. It is not too much to say that the German wireless sets in the 1918 offensive put down what can only be described as a wireless barrage.

Another enemy to working is that known as "atmospherics."

Our experiments in the future must therefore be directed towards producing good low-power sets, proof as far as possible against both spark and atmospherics. There have been many attempts, more or less successful, in both directions. The French in particular have been experimenting with a system in which a continuous-wave signal is first heterodyned to a supersonic frequency (i.e. a frequency above that of audibility), and then passed through a selective system in which damped oscillations produce what is called a free wave, while the undamped oscillation produces a stationary wave. Advantage is taken of this to couple a specially tuned amplifier at the point where the stationary wave is a maximum and the free wave a minimum, thereby largely eliminating spark and atmospheric interference.

Another interesting development is that of utilizing two frequencies, one very much higher than the other, but both "supersonic," and either interrupting or modulating the higher frequency at the frequency of the lower. It is found that this system is to a certain extent less affected by either spark or atmospheric interference, and it has the advantage over single-frequency working that it allows of a much greater number of possible variations and so makes for better selectivity and, to a certain extent, secrecy. The tuning of the two frequencies is quite independent.

Unfortunately it generally happens that an improvement in method involves complication in handling. When we bear in mind the fact that instruments have to be carried into the very forefront of the modern battle it will be realized how important it is that manipulation should be as simple as possible. The ideal is to have an instrument which can be tuned and clamped before advancing, which shall exhibit no variation in wave-length due to reasonable variations in its aerial, and which in consequence can be started with a minimum of preparation and thought. The operator may be too exhausted to think. Battalion runners in the recent war, men who travelled as light as possible, actually died from sheer fatigue in travelling over the shell-torn ground, which resembled a superlatively ploughed field. One cannot ask for fine adjustments. The very utmost to expect is an optimum setting.

Another system, which seems at first sight attractive, has, I believe, been tried in the field by certain American units, and forms one of our current experiments. In this, a continuous-wave system, both transmitter and receiver work at the same time, heterodyning each other. It has the advantage that the knowledge that communication is established is mutual, for if one hears the other he knows the other hears him on the same note. It seems likely, however, to be expensive in power, which is rather a serious drawback, and the use of such a system amongst more than a very few stations is likely to be confusing, or at all events difficult to organize properly. It is claimed, however, that it is comparatively free from jamming, and is well worth following up.

In wireless telephony I think we may claim that, so far as simplicity of manipulation, clearness of articulation and stability of wave-length are concerned (the wave-length is only $27\frac{1}{2}$ metres), our latest set is quite on a par with, if not ahead of, any other system. It is not, however, capable of simultaneous two-way conversation which, though very desirable, possesses nowadays certain prohibitive features for forward working.

I need hardly say that further investigations into the properties and applications of valves of various classes continue to form part of our programme. During the war the Valve Production Committee did marvels in making the 3-electrode valve, whether used as a rectifier, an amplifier, or a generator of undamped oscillations, as much an article of consistent manufacture as a metal-filament lamp. There is presumptive evidence that German valves were costing about four or five times what ours were costing, and appeared to be mainly hand-made, whereas ours were very largely machine-made.

We now have 30-watt and 60-watt transmitting valves. For higher powers we use a Naval 250-watt valve. Thus, by using one or two valves as the case may be, we have the useful scale of 30, 60, 120, 250 and 500 watts for sets with effective ranges of from 5 to 800 miles, probably all that is necessary for purely military purposes. We also have a good valve for both rectification and amplification, differing from the transmitting valve mainly in the "geography," as it is called, of the three electrodes, and a pure rectifying 2-electrode valve. These valves have probably been a better working compromise than anyone expected, but there is no intention of allowing them to be the last word.

Lastly I may say that steady work has been and is going on in investigating the properties of dry cells for small high-tension batteries, and also in the selection and perfection of storage batteries. The improvement of these or of mechanical sources of power is a matter of first importance, although its effects may not be so immediately arresting as some of the better-advertised phenomena.

(6) Need for Central Bureau to Co-ordinate and Regulate Effort and to Disseminate Information.

It is a matter for serious thought to consider how many excellent ideas are lost in this world. The loss has always been going on, and is going on now. At the beginning of the war much valuable work was done over again; inventions which had been tried and rejected, either on their demerits or because funds were not available to develop them when first brought forward, were re-invented, re-tried and re-developed, the reason being that those who knew their previous history had been absorbed in our first mobilization. But this is going on all around us. Information, even if existent, is not readily available. Not everyone has the leisure to examine the records of the Patent Office. Again, if the information is available, it may be out of date. A man may patent a process, and make claims based on what subsequent discovery shows to have been a freak effect. Information should be authoritative.

Now, the ramifications of modern scientific research are so widespread and so diverse in their object that no one individual or group can hope to cope with more than a very small portion of the whole. It is therefore of the very greatest importance to them and to others that, so far as possible, their discoveries, their claims, and even their failures should be placed on record, pronounced on, and placed at the ready disposal of others. We are at the beginning of what portends to be a big national organization of research, but I feel sure that central research alone is insufficient. The centre will have to feed the branches, but the process must be reciprocal. Research cannot well be portioned out, for it must frequently occur that different investigators are engaged on the same class of research with entirely different objects. But there is no reason why they should work in watertight compartments and duplicate each other's mistakes and failures.

Hence such a Central Bureau should be somewhat similar to an Intelligence Department in war. It must in the nature of things live largely on information from the Front, in this case the individual laboratory, filtered perhaps through local centres which would separate out the grain from the chaff and eliminate the effects of inevitable duplication. Having received this information, it must be in a position to digest it and pronounce on it before issuing it to all concerned. It is probably in this process of digestion and pronouncement that its function of research can be most usefully exercised. Its final issue of information must be authoritative and must command confidence.

A further and most important duty of the Central Bureau would be the collection, translation and examination of all foreign matter. In fact, it could in all probability be the only source of general information in this direction.

A *sine qua non* of such an organization is the establishment of a universal standard index. Probably, this is all that could be published in any great profusion. But this standard index, kept up to date, should be readily available at all branches and sub-branches of the organization. Why not at every public library and every Post Office? But it must be standard, it must be universal, and it must be readily expansible.

Finally, the work of this Central Bureau will have to be aided as far as possible by co-operation between the lower units. Lines of communication must be circumferential as well as radial.

A good deal of this ground has been traversed in the paper * by the late Mr. H. R. Constantine, and by the discussion it evoked when read before the Institution. So far as my own work is concerned, the more intimate the co-ordination of research the better. We have profited by it in war and we hope to continue to do so in peace.

* "Co-ordination of Research in Works and Laboratories," *Journal I.E.E.*, 1919, Supplement to vol. 57, p. 134.

ELECTRIC OSCILLATIONS IN STRAIGHT WIRES AND SOLENOIDS.

By Professor J. S. Townsend, F.R.S.

(*Lecture delivered before the* Wireless Section of The Institution, 8 *June*, 1921.)

Summary.

A number of experiments made by the author and Mr. J. H. Morrell to find the relation between the frequencies of the normal oscillations of systems are described.

As in the case of mechanical oscillations, it is necessary to distinguish between a series of free normal modes of oscillation and a series of harmonic oscillations. In the free oscillations of a system having distributed capacity and inductance there is, in general, no exact relation between the frequencies of the different modes of oscillation. A harmonic series of oscillations is obtained in nearly all cases where a fundamental oscillation of constant amplitude is maintained in a system, as, for example, when a tuning fork is maintained in vibration by an interrupted current, or a current is maintained in an oscillatory circuit by a thermionic valve.

The oscillations of solenoids and parallel wires are discussed, and an estimate of the accuracy obtained in measuring wave-lengths by wires is given.

A simple method, depending on harmonic oscillation, is described whereby the lengths of short waves measured by means of parallel wires may be compared with those of long waves measured by means of standard inductances and capacities.

(1) The improved methods of generating continuous oscillations by means of valves provide simple means of investigating either a harmonic series of oscillations or a series of free oscillations in circuits with distributed capacity and inductance. The points of resonance and the frequencies of oscillations in solenoids and wires may be thus obtained to a higher degree of accuracy than was possible in the earlier determinations, where the resonance effects were obtained with waves emanating from a spark oscillator.

In considering the different oscillations of a series it is necessary from the outset to distinguish between the two different kinds of series, namely, the harmonic series and the series of free oscillations.

(2) A harmonic series of oscillations is, in general, obtained when an oscillatory circuit is maintained in continuous oscillation by means of an arc or valve. The complete oscillation in these cases comprises a series of simple periodic oscillations, each simple oscillation being of the form $A \sin(pt - \alpha)$. The principal or fundamental oscillation of the series with the large amplitude A_1 has the lowest frequency $p_1/2\pi$. The amplitudes A_2, A_3, A_4, etc., of the oscillations with the higher frequencies $p_2/2\pi$, $p_3/2\pi$, $p_4/2\pi$, etc., which accompany the fundamental oscillation, depend on the manner in which the system is maintained in oscillation, but in all cases of continuous oscillation the frequencies are exactly proportional to the numbers 1, 2, 3, 4, etc. The complete oscillation, therefore, consists of a harmonic series of oscillations, the principal oscillation (as in analogous phenomena in mechanical oscillations) is called the fundamental, and those with higher frequencies are called the harmonics.

The effect of the arc or valve is to supply to the oscillatory circuit energy which compensates for the loss of energy in conductors and dielectrics, so that currents of constant amplitude are obtained. The period $p_1/2\pi$ of the fundamental oscillation depends on the inductance and capacity of the oscillatory circuit, and is approximately the same as the period of a free oscillation of the circuit. This period is also the period of an alternating force with which the circuit resonates. The harmonics are not free oscillations; they only occur along with the fundamental oscillation, and no resonance effects are observed when the circuit is acted on by an alternating force having a frequency equal to that of one of the harmonics.

For practical purposes in wireless telegraphy the valve generators are designed with a view to reducing the amplitudes of the harmonics to small values.

In some experiments, which are here described, the coupling between the valve and the oscillatory circuit is adjusted so as to obtain a series of harmonic oscillations of large amplitude, and the exact relation between the frequencies of the harmonics is used in determinations of wave-lengths.

(3) A simple case of a mechanical system performing oscillations of constant amplitude is that of a tuning fork maintained in continuous vibration by means of an interrupted circuit. When a fork A is used to interrupt the current that maintains it in vibration, the interrupted current may be represented by a Fourier series of simple periodic currents. Each component of the series has a frequency which is an exact multiple of the frequency of the fork, so that the total current is a harmonic series. When this current interrupted by the fork A is used to excite oscillations in another fork B, sharp resonance is obtained when the natural frequency of B coincides with one of the harmonics. This principle was used many years ago by Lord Rayleigh, to determine the pitch of a fork of high frequency by means of a fork of low frequency.*

(4) The free electric oscillations of a system correspond to the mechanical oscillations known as the normal modes of oscillation. They occur when a disruptive discharge takes place across a spark-gap in a circuit having distributed capacity and inductance. There is, in general, no exact relation between the frequencies of the normal modes of oscillation. It is possible to obtain a system in which these frequencies are nearly proportional to the numbers 1, 2, 3, and 4,

* *Nature*, 1877, vol. 17, p. 12.

as in a single wire or a pair of parallel wires. But with an ordinary oscillatory circuit consisting of a condenser of large capacity and an inductance coil, the lowest frequency depends on the capacity of the condenser and is widely different from the frequencies of the other free oscillations which depend on the capacity of the inductance coil and are very little affected by the capacity of the condenser. The series of free oscillations thus obtained are sometimes referred to as harmonic series, but it is much better to call these oscillations "free oscillations" or "normal modes of oscillation" and to apply the term "harmonic" only to the series in which the frequencies of the components are exactly proportional to the numbers 1, 2, 3, 4, etc.

(5) Any system with distributed capacity and inductance has several normal modes of oscillation, and a free oscillation may take place in any of these modes independently of the others. Also, when the system is acted on by an alternating force, a large resonating current is obtained when the frequency of the force coincides with the frequency of one of the normal modes of oscillation.

In these respects the series of free oscillations differs completely from the series of harmonics which accompanies the fundamental oscillation maintained by means of a valve.

When oscillations are excited by means of a spark discharge, the amplitudes of the various free oscillations depend on the initial distribution of charge and current in the system.

In the case of an aerial of small inductance connected to earth through an inductance coil and used as a spark transmitter, the wave-length of the principal oscillation depends on the capacity of the aerial and the self-induction of the coil. A series of oscillations of smaller amplitude is also generated, having wave-lengths depending on the self-induction of the aerial and the capacity of the inductance coil. In wireless telegraphy the spark transmitters are arranged so that the amplitude of the oscillation of lowest frequency is large, and that of the others comparatively small.

When the same aerial is used as a receiver it resonates to an incoming wave, having a frequency equal to that of any one of the free oscillations which would be generated if it were used as a spark transmitter. It does not resonate when the incoming wave has a frequency equal to that of one of the harmonics which accompany the fundamental oscillation generated by connecting the aerial to a valve.

(6) The oscillatory system consisting of two parallel wires, as used by Lecher in his well-known experiments, is of particular interest. If the wires be very long compared with the distance between them, the frequencies of the free oscillations are almost exactly in simple proportions, as the effects of the ends and of neighbouring conductors are small in these modes of oscillation where the currents are in opposite directions at opposite points of the two wires.

A general theory of the propagation of waves along parallel wires has been given by Rayleigh,* and a simple solution of Maxwell's equations for the forces in the space around the wires is obtained. The investigation indicates that the waves are propagated along the wires with the velocity of light in air, a result which is independent of the diameter of the wires or of the distance between them, provided that the resistance of the wires is negligible.

When currents are induced in the wires by waves of a given length λ emitted by a short-wave generator, resonance is obtained when the length of the wires is adjusted so that the frequency of one of the normal modes of oscillation is equal to that of the generator. If wires of a fixed length form part of an oscillatory system, this adjustment is made by means of a bridge between two opposite points on the wires. If l_1, l_2, l_3 be the lengths of the wires when the first, second and third modes of oscillation are in resonance with the short-wave generator, the wave-length λ is $2(l_2 - l_1)$ or $2(l_1 - l_2)$ if the effect of the ends is the same in all cases.

A number of experiments have been made in order to find to what degree of accuracy the lengths of short waves may be obtained by parallel wires, as this method is much simpler than that depending on measurements of the capacity and self-induction of a closed oscillatory

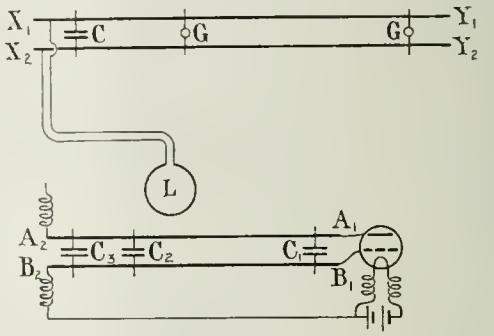


FIG. 1.

circuit. The short waves measured by means of the parallel wires may be tuned to coincide with the harmonics of a long-wave generator, and the wave-length of the fundamental oscillation may thus be determined in terms of the lengths of short waves.

The principle of comparing wave-lengths by harmonics is used in the French *multivibrateur*, which is adapted for determining the high frequencies obtained in electric oscillations in terms of the frequency of a standard fork. There is a considerable advantage, however, in using the short wave as a standard, as it is easily adjusted to any required length.

(7) The arrangement of a short-wave generator having a fundamental oscillation which may be measured by a pair of parallel wires is shown in Fig. 1. The oscillatory circuit consists essentially of two rods A_1A_2, B_1B_2, 75 cm long, and a mica condenser C_2 of about one milli-microfarad. Its exact capacity is unimportant, provided it is large compared with that of the rods. The condenser is of small dimensions, and two short wires are connected to its terminals and slide on the rods, so that the condenser may be placed at any distance from the valve. The end A_1 of the upper rod is connected to the anode of the valve, and the end

* *Philosophical Magazine*, ser. 5, 1897, vol. 44, pp. 129 and 204.

A_2 is connected through a choke coil to the positive terminal of a high-tension battery. The end B_1 of the lower rod is connected to the grid, and B_2 is connected through a choke coil to the filament and to the negative terminal of the high-tension battery.

The oscillatory current flows from A_1 to B_1 through the condenser C_2, and the wave-length is adjusted by moving the condenser along the rods. The fine adjustment of the wave-length is made by a small adjustable condenser C_1 connected to the ends A_1, B_1.

It was found that the system did not oscillate when the condenser C_2 was in one particular position, owing to the damping introduced by the circuit $A_2C_2B_2$ when resonating with $A_1C_2B_1$. This difficulty was overcome by bridging the rods with a second condenser C_3, which stops the resonance.

The parallel wires X_1Y_1, X_2Y_2 (Fig. 1) were about 8 m long, and connected at one end to a loop L for coupling to the short-wave generator. A small condenser C was connected between the ends X_1, X_2, for the purpose of adjusting the distribution of current in the wires so as to have resonance when the bridge was near the ends X_1, X_2. In order to avoid the effects of surrounding objects the wires were stretched at a distance of about a metre above the tables. With waves from 5 to 15 m long, resonating lengths of the wires may be determined within a few millimetres by using a small incandescent lamp G to indicate the current in the bridge between the wires. A higher degree of accuracy may be obtained by using a thermo-junction instead of the lamp, and when the filament of the valve in the generating circuit is maintained at a constant temperature the wave-lengths along the wires may be measured to one part in 5 000.

In order to obtain accurate measurements the current in the free ends GY_1, GY_2 should be very small when the bridge is in a position of resonance. It is therefore necessary that the system comprising the parallel wires should have no free mode of oscillation which is nearly in resonance with the short-wave generator when the bridge G is removed. The simplest method of obtaining this result is to adjust the condenser C so that the detector indicates a maximum current in the wires when the bridge is near the free ends Y_1, Y_2. Another position of resonance of the bridge is required in order to measure the wave-length, and this position may be within one or two metres of the ends X_1, X_2. A wave nearly twice the length of the parallel wires may be thus measured accurately.

When the effect of the free ends was thus eliminated, and a current was induced in the oscillatory system comprising the wires by a wave of constant length, the distance measured along the wires between the resonating positions of the bridge remained accurately the same (within one part in 5 000) when the distance between the wires was changed from 2 to 10 cm, or the diameter of the wires changed from 1·2 to 2·05 mm, or the resistance of the thermo-junction changed from 0·3 to 8 ohms.

The measurement of the wave-length was not affected by altering the shape of the loop L used for coupling to the generator, or by altering the capacity of the condenser C, provided one position of resonance of the bridge was well within a quarter of a wave-length of the ends Y_1, Y_2. It was also found that the wires need not be accurately parallel, as the length of a wave as found with wires 5 cm apart at one end and 5·2 cm at the other end was the same as when the wires were 5 cm apart at both ends.

(8) In order to measure waves which are long compared with the length of the wires, a circuit consisting of an ordinary inductance with tappings and a variable condenser of about one milli-microfarad was maintained in oscillation by a valve. The reactance was made much higher than that required for starting oscillations, in order that the harmonics should be well developed.

With an oscillatory current of about 2 amperes in the generator the first 10 harmonics were easily observed directly by a wave-meter, using a thermo-junction to detect the current, and frequencies were exactly proportional to the numbers 1, 2, 3, 4, etc. The wave-meter had been carefully adjusted to the National Physical Laboratory standards, and changes of 1 per cent in wave-length were easily observed.

The higher harmonics of the long-wave generator may be measured by the beats obtained by combining them with oscillations induced by the short-wave generator. These beats are easily observed by inserting a telephone transformer in the anode circuit of the long-wave generator, and making the grid connection in the latter circuit with a wire which passes near the short-wave generator. The beats thus produced in the telephones are sufficiently strong to be easily heard without tuning the anode connection of the long-wave generator to the frequency of the short-wave generator.

If w be the wave-length of the fundamental oscillation of the short-wave generator, and W that of the long-wave generator, then W is an exact multiple of w when one of the harmonics is in tune with the short-wave generator. When W is fixed, and the wave-length of the short-wave generator is varied continuously, a series of m harmonics comes into tune as w passes from the value w_1 to w_2. The number m may thus be counted, and the following relations between the wave-lengths are obtained:

$$W = nw_1 = (n + m)w_2$$

Thus $W = mw_1w_2/(w_1 - w_2)$.

The first and last oscillations of the series w_1 and w_2 are accurately adjusted to resonance with harmonics, and their lengths determined by means of the parallel wires. It will be noticed that the quantity $mw_2/(w_1 - w_2)$ should be a whole number, and it may be taken as the nearest integer to the value obtained experimentally.

For example, in one experiment in which 30 harmonics of the long-wave generator were counted in changing the wave-length of the short-wave generator from 12·12 to 7·996 m, the quantity $mw_2/(w_1 - w_2)$, as found experimentally, was 58·1, and, neglecting the figure in the decimal place as being due to experimental error in measuring the difference $(w_1 - w_2)$, the fundamental wave-length of the long-wave generator was

$$W = 12{\cdot}12 \times 58 = 703 \text{ m}$$

In this case the wave-length W, as indicated directly by the wave-meter, was 706 m.

The wave-length W may also be measured when the short-wave generator is adjusted to a fixed wave-length w_1, by counting the number of harmonics n that come into tune when W is increased continuously from a value W_1 to $2W_1$. The factor n is thus known, and $W_1 = nw_1$. In this method W is adjusted by the variable condenser in the long-wave oscillator, and it is necessary to have a means of determining when W is changed from any wave-length W_1 to $2W_1$. For this purpose an oscillatory circuit consisting of a fixed condenser and a variometer in which the two coils are of equal self-induction is used. This circuit with the coils in parallel is brought into tune with the oscillation W_1, and when the coils are changed from parallel to series the variometer circuit is again in tune when W attains the value $2W_1$.

This method is useful in determining a series of waves which are multiples of a simple standard length such as 10 m. Wave-lengths up to 1 000 m as measured by this method were found to be in agreement with those given by the wave-meter, which was calibrated by the National Physical Laboratory.

(**9**) In this method of measuring long waves it might be supposed that a difficulty would arise if short-wave free oscillations as well as the higher harmonics of the principal free oscillation were maintained in the inductance coil of the generator. It is well known, for example, that a coil, even when connected to a condenser, has several free modes of oscillation with wave-lengths much shorter than that of the principal free oscillation determined by the product of the self-induction of the coil and the capacity of the condenser. These free oscillations may occur in any coil; but, with a condenser of comparatively large capacity connected to the ends of the coil, the wave-lengths of the short, free oscillations are not affected to any appreciable extent by changing the capacity of the condenser, and are thus distinguished from the harmonics of the principal oscillation.

When the latter is maintained in a circuit by means of a valve no short-wave oscillations are observed, except the harmonics which change in wave-length with the capacity.

It thus appears that the principal oscillation and one of the shorter free oscillations are not maintained simultaneously by the valve, but it is possible to arrange the valve connections in such a way that the principal oscillation with its harmonics is absent, and a short wave is maintained in one of the free modes of oscillation of the coil.

The following example shows how the mode of oscillation of an ordinary circuit may be changed by altering the valve connections. The circuit consisted of a condenser adjustable from 0·2 to 2 milli-microfarads, connected to the ends of a solenoid 14 cm long having tappings dividing the coil into 8 sections of 35 turns each. When the anode of the valve was connected through the high-tension battery to one end of the coil, the grid to the other end, and the filament to the centre, an oscillation of 4 000 m wave-length was maintained with a condenser of 1·4 milli-microfarad capacity, the first 12 harmonics were observed directly by a wave-meter, and the wave-lengths of the system changed continuously with the capacity of the condenser in the ordinary manner. No other oscillations were observed. When the number of sections of the coil between the valve connections was reduced to one section between the anode and the filament, and two between the grid and filament, no long oscillation was observed, but a wave of 318 m and its first two harmonics were indicated by the wave-meter and were not affected by changing the condenser in the oscillatory circuit from 0·2 to 2 milli-microfarads.

(**10**) The theory of the oscillations of solenoids is comparatively simple when the investigation is limited to cases where the end effects are neglected, and the results give a general explanation of the normal modes of oscillation. Several corrections have to be taken into consideration in order to obtain formulæ which give approximate values of the frequencies of the free oscillations.* There is no exact relation between the frequencies of the free oscillations in a long solenoid, but these oscillations are interesting from an experimental point of view, and the distribution

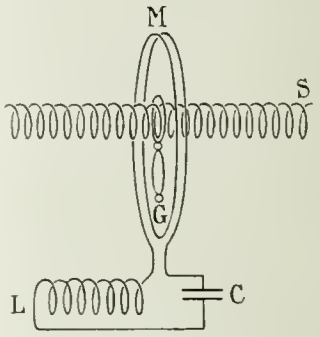


Fig. 2.

of the charge and current in the solenoid is easily demonstrated by means of simple apparatus.

For this purpose a continuous-wave generator was used to induce oscillations in a solenoid. The solenoid was wound with 29 turns per cm on a wooden cylinder 5 cm in diameter, the total length of the solenoid being 227 cm and the specific inductive capacity of the cylinder being 3·95.

The arrangement of the apparatus is shown in Fig. 2. An oscillatory circuit LC with adjustable capacity C was maintained in oscillation by means of a valve, and a coil M of a few turns was used to induce oscillations in the solenoid S. In order to reduce the effect of neighbouring bodies on the capacity, the solenoid was suspended by strings at a height of 120 cm above the table.

In the normal oscillations having a maximum current at the centre the potential is zero in the plane perpendicular to the axis throughout the centre, so that when the coupling coil M is in this plane it has no effect on the capacity of the solenoid.

The current in the solenoid was detected by a small incandescent lamp G connected to two loops of wire forming a figure of 8. One loop passed round

* J. S. Townsend and J. H. Morrell: "Electric Oscillations in Straight Wires and Solenoids," *Philosophical Magazine*, ser. 6, 1921, vol. 42, p. 248.

the solenoid, and a maximum current was obtained in the lamp when the solenoid was in resonance with the oscillator and the detector in a position where the current in the solenoid was a maximum. The electromotive forces in the two loops due to the current in the coupling coil M are in opposite directions, so that the current in the lamp was maintained solely by the current in the solenoid. The lamp is not sufficiently sensitive to detect any oscillations set up by the harmonics of the generator.

The centre of the solenoid is one of the points of maximum current when the inducing coil is in the central plane, and resonance is obtained when the wave-length of the fundamental oscillation of the generator is equal to that of one of the normal modes of oscillation of the solenoid corresponding to the odd numbers.

The following table gives the wave-lengths of the

Wave-lengths of Free Oscillations of Solenoid 227 *m long, diameter* 5 *cm ;* 29 *turns per cm.*

n	Calculated Wave-length, in Metres	Observed Wave-length, in Metres
1	773	870
2	434	—
3	315	338
4	253	—
5	217	231
6	192	—
7	175	184
8	161	—
9	150	158

free oscillations as calculated from theoretical considerations, and also the wave-lengths of the 1st, 3rd, 5th, 7th, and 9th obtained experimentally. The mode of oscillation is indicated by the number n in the first column, which gives the number of points on the solenoid at which a maximum current is obtained.

The discrepancy between the observed and calculated value, which is very marked in the longer waves, is due to the effect of the ends and the increase of capacity produced by neighbouring bodies.

(**11**) The increase in wave-length due to the ends may be shown by measuring the distances between the points of maximum current in the solenoid. These points are found by moving the detector along the solenoid to the positions where the current in the lamp is a maximum. Although these positions cannot be determined very accurately by the lamp indicator, the experiments show that the distances between the points of maximum current are larger in the middle than at the ends, by an amount which cannot be attributed to experimental error. An example is given in Fig. 3, where the curve indicates the distribution of current along the solenoid AB when resonance is obtained with a wave of 231 m length, $n = 5$.

(**12**) A complete series of free oscillations of a solenoid may be obtained by using two similar inducing coils which are connected in parallel and which form part of the oscillatory circuit connected to the valve.

The coils are placed near the ends, and by means of a reversing key the connections may be altered so

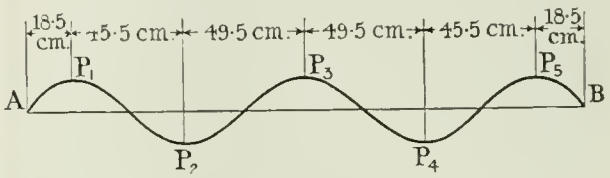


Fig. 3.

that the currents in the two coils may be either in the same or in opposite directions. In the modes of oscillation corresponding to the odd numbers the currents in the solenoid are in the same direction in sections near the ends, and these oscillations are obtained when the currents in the inducing coils are in the same direction. In the modes corresponding to the even numbers the currents in the sections near the ends are in opposite directions, and these are obtained by changing the connections of one of the inducing coils by means of a reversing key.

The inducing coils affect the capacity of the solenoid, but the method has the advantage of giving a symmetrical distribution of current on either side of the centre of the solenoid for both the odd and even series of free oscillations.

Discussion before the Wireless Section, 8 June, 1921.

(Note.—The majority of the points referred to in the discussion have been dealt with by the author in preparing the account of the Lecture for publication in the *Journal*.)

Dr. W. H. Eccles : The importance of the method of accurate measurement which the author has described will appeal to everyone. I hope we shall have something like it set up at the National Physical Laboratory and compared with the French method, in order to see which is really the more convenient and, perhaps, the better.

Professor G. W. O. Howe : It has truly been said that what is one man's meat is another man's poison. This apparently applies also to harmonics. While a number of wireless engineers are trying to get rid of harmonics, others are devising methods of developing them. The harmonics in some of the large transmitting stations are well known to be a cause of great trouble, and recently in the *Jahrbuch der drahtlosen Telegraphie* there has been an account of some very lengthy experiments made in order to get rid of the harmonics in the aerial current at Nauen. A very important point which is often confused is that made by the author with regard to the difference between harmonics and

normal modes of vibration. I have heard many people express a doubt as to whether the frequencies of the harmonics of the aerial of an arc generating station were really exact multiples of the fundamental frequency, or whether they had some odd relationship. This was because they had seen formulæ for the different possible natural frequencies of an aerial with a given loading coil. These formulæ showed that there was no simple relationship between these frequencies, and the suspicion arose that all these various frequencies were present when the aerial was excited by a Poulsen arc. This cannot be so if we assume that every complete cycle is a replica of the preceding one. Obviously, in this case, all the component frequencies would be real harmonics and not these normal modes of vibration of which the antenna is capable. There is one question I should like to ask the author. When the oscillating system connected to the valve ceased to oscillate at its fundamental wave-length of 4 000 m and started to oscillate at another much shorter wave-length, i.e. 318 m, was it possible for oscillations to occur with other fundamental wave-lengths ? I suppose that the latter figure corresponded to one normal mode of vibration of the solenoid and that, by putting the connections still closer together, another set of vibrations could be obtained. I suppose also that the third, fourth, fifth, and the various other modes of vibration would be set up in the solenoid. Presumably the anode tap and the grid tap must be approximately at the anti-nodes of potential along the solenoid, corresponding to any system of oscillations it is desired to maintain. In a recent number of the *Archiv für Elektrotechnik* (1920, vol. 9, p. 1) Gothe described some very accurate measurements made on a very carefully constructed single-layer solenoid wound on a framework. He there explored the various modes of oscillation with direct-current excitation, going up to about the 30th harmonic and mapping out the distribution of the field along the coil. The method he adopted for detecting the wave distribution was slightly different from that used by the present author. He had a glass rod running parallel with the solenoid, an inch or two away from it, and on that was mounted a little exploring coil connected up with the thermo-junction and galvanometer, and by means of silk threads the exploring coil, which was parallel to the turns on the coil, was moved along the glass rod, and the readings on the galvanometer were plotted. The results obtained were very interesting, and tables were given of the various wave-lengths of these normal modes of oscillation. Experiments were made both with a long solenoid and with a short coil of large diameter, and the results showed that the ratios of the frequencies were quite different in the two cases. In my opinion the most complete mathematical treatment of this subject is due to Lenz.* The experiments referred to above were carried out with a view to checking experimentally the results predicted by Lenz's calculations. I am very interested in the latter part of the present paper, in which the author describes a modification of the Lecher wire system. Whereas Abraham and Bloch in the multivibrator use harmonics to link up radio frequencies with accurately determinable acoustic frequencies, the author uses them in the opposite direction to link up radio frequencies with extremely high frequencies, the wave-lengths of which can be readily measured.

* *Annalen der Physik*, 1914, vol. 43, p. 749.

Mr. L. B. Turner: One of the features of the two parallel wires for measuring wave-lengths which the author mentioned, was that the wave-length was independent of the distance between the wires. I presume that that statement requires a certain amount of qualification. The distance between the Lecher wires obviously cannot be made indefinitely great, and I should be glad if the author would give some indication of the distance to which it is safe to go. I should like to refer to a point mentioned quite early in the paper, namely, that as the coupling between the two coils is altered, oscillation being just maintained by moving the anode tap and the grid tap together, there comes a point when the original mode of oscillation entirely ceases and a new oscillation, another normal mode of much higher frequency, occurs, the ammeter behaving in such a way as to lead one to suspect that the system is no longer in oscillation at all. That is a phenomenon which I dare say many here have often observed in the laboratory. I once experienced some trouble from it in quite an acute form in one instrument which was not intended to contain high-frequency currents at all. It was a very sensitive low-frequency relay arrangement which was misbehaving, and I discovered that the peculiar things which were happening were due to a self-sustained oscillation of extremely high frequency, which declared itself by an alteration in the ammeter reading when I put my hand near the instrument without actually touching it.

Major H. P. T. Lefroy: The author has described his " current " method of analysing solenoids, and, in this connection, a description of a " potential " method, which I used last year for the same sort of work, may be of interest. It is very simple and gives the frequency in the circuit under test, as well as an approximate idea of the relative potentials at different points of that circuit, and it is particularly useful for finding points of zero potential accurately, on working circuits, without the introduction of ohmic resistance into such circuits. The method is as follows :—A small variable air condenser, e.g. 50 to 150 cm, is connected across a variable inductance of suitable value, to form the closed oscillation receiving circuit. The grid of a receiving triode is then connected to the insulated terminal of the small condenser, and the filament of the triode to the " earthy " terminal of the condenser ; a telephone and anode battery are connected up as usual. A single insulated wire, of any convenient length and size, is then connected at one end to the " earthy " terminal of the condenser and fitted at its other end with a well-insulated finger grip (similar to that used on portable voltmeters), so that that end can be touched on to any point on the circuit under test ; a very fine wire will do for this connecting link, e.g. I have used 48 S.W.G., and I have obtained good results with links up to 100 yards long. The receiving circuit is now tuned in, and the frequency in the circuit under test thus obtained ; the tuning will be found to be sharp. In moving the contact end of the connecting link from

point to point on the circuit (open or closed), or the solenoid, in which oscillations are flowing, signals of variable strength are received in the telephone, thus the points of maximum, zero, and intermediate potential are easily located, even though the oscillations are very weak, e.g. those being received in the circuit under test from a distant wireless station, and it is obvious that the above apparatus acts as an effective receiving set on leaving the link connected to a point of maximum potential in the aerial circuit; it will be observed that, when using this method of receiving, the operator may be located at a considerable distance from the receiving aerial. I have found the method particularly useful for tracing the potential at all points on variable aerial tuning inductances, for the study of "dead end" effect, etc. It may be noted that, if the condenser of a rejector is short-circuited, we have circuits exactly similar to the above, namely, "contact coupling." Various uses may be made of the above principle, e.g. high-frequency telephony to and from captive balloons, using their anchor wire as the link, with a closed oscillation circuit at each end of it. It can also be used for connecting to the rotary loop aerial of a direction-finding station. I noticed that when the author gave his formula (13) he did not make any particular remark as regards the proportion of capacity to inductance, in connection with the harmonics generated. I find in practice, when generating continuous-wave oscillations, that I get the weakest harmonics in an oscillation circuit when the capacity is greatest for any given *CL* value, i.e. when the stiffness is least; a stiffness of 5 000 000 (henrys/farads) giving considerable freedom from harmonics, and a stiffness of 500 000 still more so. In practice, however, the use of a large capacity in wireless transmission unfortunately gives rise to a large oscillating current, and thus to considerable ohmic loss, thereby reducing the efficiency. My experience has always led me to the conclusion that high-frequency circuits with "lumped" capacity are far freer from harmonics than circuits of equal *CL* value with distributed capacity; incidentally I found this to be the case in aerials in 1910, and it was for that reason, among others, that the "umbrella" aerial was retained for the pre-war army wagon set, in spite of certain obvious physical objections to it.

Dr. A. Russell: I hope the author will explain a little more fully when the paper is published in the *Journal* how the capacity and inductance vary with the frequency.

Mr. J. St. V. Pletts: I am not quite clear whether a stationary wave of a higher frequency than the fundamental is a harmonic or a different mode of oscillation. It is only when we have a certain amount of distribution of capacity and inductance that these stationary waves become possible. To take the analogy of a sound wave which can be decomposed into Fourier harmonics, each of these may be due to a separate stationary wave on a vibrating string, and it seems to me, therefore, that the wireless engineer is justified in calling them harmonics, although I think the author would call them different modes of oscillation.

Dr. J. H. Vincent: Many people will be pleased to get definite information on series of harmonics in valve-maintained circuits. I understand from the paper that, provided the coupling is fairly close in a maintained circuit, if we desire to get two waves, one exactly half as long as the other, all that is necessary is to set the arrangement to produce the longer one, when the other also will be produced, and the accuracy of the relationship of 2 to 1 can be relied upon. This matter is really quite an elementary one if looked at from the standpoint of sound, but I think these things are found rather more difficult to understand when electrical oscillations are considered. With regard to normal modes not existing concurrently, this used to be given as a kind of tenet of faith in sound. It was thought that a system could not vibrate in two distinct ways. But that was proved to be untrue; in fact, Koenig put on the market some steel bars which not only vibrated in different normal modes simultaneously but exhibited the beats resulting from their combination.

Mr. R. C. Clinker: As I understand it, this exact method of measuring wave-lengths depends on the knowledge that the velocity of the wave along two wires is the same as that in free space. Is it not necessary, therefore, to be careful, in stretching those wires, to keep them away from any conducting or semi-conducting material? When I had the pleasure of seeing the author's apparatus at Oxford some months ago I noticed that the wires were stretched against the wall, probably about 6 inches away, the distance between the wires being perhaps 3 inches. Is it not possible that the presence of the semi-conducting wall might alter the capacity of those wires and so change the velocity of the wave? There is one other point in connection with the oscillation of the valve. Supposing that we have a valve steadily oscillating, we can say at once that there must be harmonics in the wave, because the valve simply increases the amplitude of its oscillation until it encroaches on the curved part of the characteristic, and so produces the harmonics. Surely it is not necessary to change the coupling at all. I think that, once the valve is oscillating, the harmonics must be there.

Dr. E. H. Rayner: The consideration of some of the phenomena dealt with in the paper would, I think, be made simpler if it were dealt with in terms of frequency rather than wave-length. It avoids the necessity of thinking in reciprocals. Multiples are generally more easily comprehended, and are commonly used in analogous phenomena in sound. The type of the series of tones obtained from a solenoid would be more easily appreciated if diagrams were given of the electrical distribution on the solenoid. It seems that the mode of vibration would be something like the following: If AB be a longitudinal cross-section of a solenoid vibrating electrically in the gravest mode, the electrical field due to distributed capacity will be represented by the dotted lines in Fig. A. If the capacity C be small, the first overtone will be represented by Fig. B, in which the resonance is analogous to that of an open organ-pipe. It is to be noted that the condenser plates are always at the same potential when the mode of vibration is of this type. The effect of the condenser,

except for the distributed capacity of leads, etc., will disappear. The next overtone in this mode is shown in Fig. C, in which the condenser plates are always

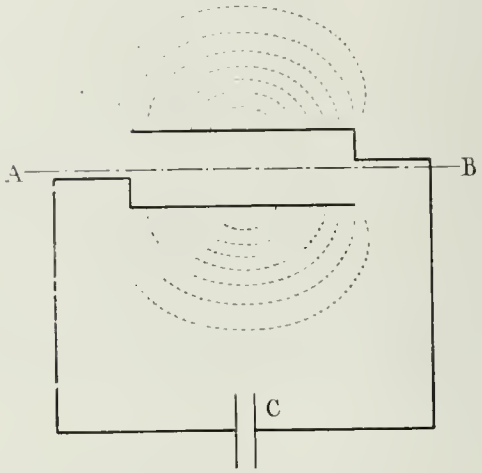


Fig. A.

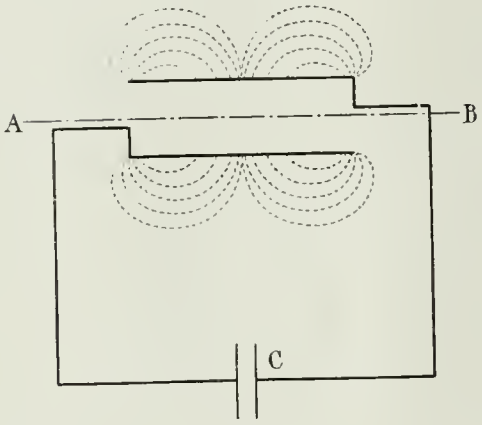


Fig. B.

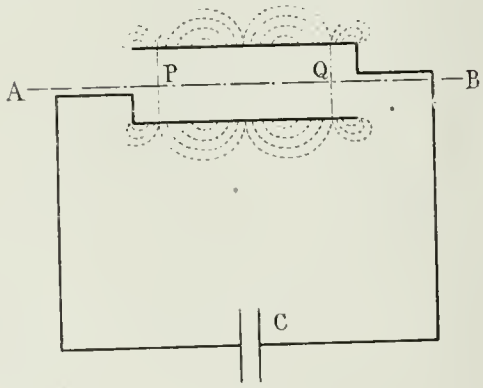


Fig. C.

at the same potential, and there is a node at the centre, with two others, P and Q, nearer the ends. The intermediate form of vibration of the type shown in Fig. A has two nodes corresponding to P, Q, without the centre node. The condenser plates are at different potentials, and the position of the end nodes must be very dependent on the relative magnitude of the condenser C, and of the distributed capacity of the solenoid. As the higher modes are produced the types shown in Figs. A and B will alternate, the one series of frequencies being independent of the capacity C, and the other dependent on it, and in general the frequencies of the two

Fig. D.

series will not run alternately in magnitude. The impurities which may occur in the wave emitted from an aerial are well shown in oscillographs taken by M. Dufour at the Eiffel Tower and reproduced in *Le Journal de Physique et Le Radium*. In one of these (Fig. D) the

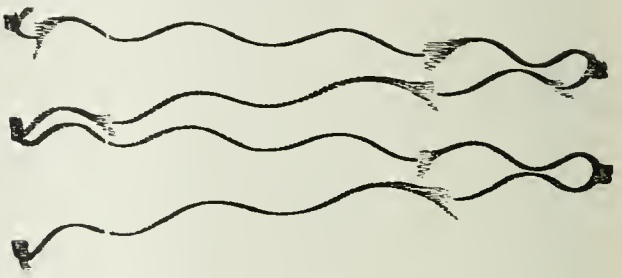


Fig. E.

main frequency of 220 000 is shown as being very peaked, while overtones of the order of 10 times the frequency of the fundamental are clearly indicated, as is also their damping. In Fig. E the fundamental is 33 000, and an overtone of relatively very high frequency appears to break out about every third wave. The results are most illuminating as showing what may happen with an arc-driven oscillating system.

Mr. A. G. Warren: The author has carefully distinguished between the treatment of the solenoid and of the straight wire. In the former case it is assumed that the potential at any point is determined by the charges on the various parts at the instant under consideration. This treatment cannot be applied to a straight wire; the velocity of propagation is finite and must be taken into account. In the case of a wire of low resistance the velocity of propagation is the same as that of light. If we consider a disturbance which is sinusoidal in both space and time, it would appear that the potential at any point is the same as it would be if the whole wire had a steady uniform charge of the same density as that existing at the point under consideration. In other words, the "capacity per unit length" is a perfectly definite function determined by the constants of the wire itself, and is independent of frequency. This is not the case with a solenoid, though it is still possible to define the capacity. In the general case, however, e.g. in an open helix, it does not appear to me that the terms "capacity" and "inductance"

have any meaning. I should like the author's views upon this point. Further, if the suppositions already made are correct, we should expect, as the author has found by experiment, that the wave-lengths occurring on a pair of parallel wires are nearly independent of the distance between them. The capacity and inductance of such wires are inversely proportional to one another when the size of the wires is small compared with the distance between them. I should also like to ask the author whether he has obtained on wires frequencies which approximate closely to harmonics of the fundamental. The case is analogous to that of an organ pipe in which normal oscillations may be set up having frequencies rather greater than 3, 5, 7, . . ., etc., times the fundamental. In the case of a vertical wire, where the fundamental has a wave-length about 4·2 times the height of the antenna, we should expect the frequencies of other modes of oscillation to be of the form $(1 + 2{\cdot}1n)f$, where f is the fundamental frequency, and n an integer. This is only approximate and assumes that the "end effect" is confined to that portion of the wire between the end and the last node. I was interested in the author's method of determining the current distribution in a solenoid. Could this method be applied to exact experimental work? In finding the resistance of solenoids I have determined the distribution of power consumption by measuring the initial rate of rise of temperature of the various parts of the coil. So far, the measurements have been made only upon coils of natural wave-length fairly short as compared with the wave-length of the supply. The resistance loss in that case is greatest where the current is least. In carrying the experiments further it would be necessary to measure the current distribution with the same accuracy as that to which the power distribution can be determined.

OBITUARY NOTICES.

ARTHUR ARMITAGE was born in 1876 and received his early education at the People's College, Nottingham. From 1890 to 1892 he was a student at the University College, Nottingham. He was then apprenticed to the Central Marine Engine Works, West Hartlepool, and afterwards to the G.P.O. Instrument Repair Shop, Nottingham. In April, 1893, he was appointed as a junior on the staff of the Nottingham Corporation Electricity Department, and after passing through the various sections became Chief Assistant in 1902. He held this position until his death, which occurred on 23rd January, 1921. He was elected an Associate Member of the Institution in 1902, and a Member in 1917. H. T.

JULES CARPENTIER was born in Paris on 30th August, 1851. In 1871 he entered the Ecole Polytechnique, on leaving which he became a member of the corps of Ingénieurs des Manufactures de l'Etat. In 1876, after spending a year in the workshops of the Paris–Lyons–Mediterranean Railway, he was appointed chief assistant to the Mechanical Engineer of that Railway. He had intended to devote his attention to mechanical engineering, which particularly appealed to him, but on Ruhmkorff's death he purchased the latter's workshops, etc. Electrical science was at that time making rapid progress and being applied to industry, and new methods of measurement were required. In this field, England had the advantage of Lord Kelvin's guidance. In France M. Carpentier undertook the complete reorganization of the Ruhmkorff works. As a result, the production of all classes of instruments rapidly increased and large quantities of electrical apparatus, including the well-known d'Arsonval galvanometer, were placed on the market. The firm also constructed the Baudot multiple telegraph printer. Into several of these appliances M. Carpentier introduced his own ideas. In addition, he did a great deal of work in connection with photographic reproduction and the recording of musical pieces, and the following inventions are due to him: a portable dark-room; a stereoscopic camera; and the "melograph," used in conjunction with the "melotrope," the latter of which enables pieces of music registered by the former to be replayed. He also, in conjunction with M. Charles Cros, gave a great deal of attention to three-colour photography. Most important of all, however, were the submarine periscopes which have proved of such great service in naval warfare. All these inventions testify to his great ingenuity and accuracy, combined with an excellent scientific education. In 1897 he joined the Bureau des Longitudes and in 1907 was elected a Member of the Academie des Sciences. A skilled manufacturer, he represented on this body the mechanical arts and the manufacture of precision measuring instruments. In 1907 he received the rank of Commander of the Legion of Honour. Equally well known in both the scientific and industrial worlds, all those who came in contact with him appreciated his high qualities. He died on 30th June, 1921, as a result of a motor-car accident. He was elected a Foreign Member of the Institution in 1887, and a Member in 1911. L. J.

TOM ERNEST GATEHOUSE was born at Norwich in 1854; the son of a mechanical engineer, he had a natural bent towards engineering, and was fortunate in becoming a pupil of Robert Sabine, one of the most able workers in the field of electrical science and industry, then practically virgin ground. At a later date he was closely associated with Sir Charles Wheatstone and Sir Samuel Canning who, like Sabine, were in the front rank of the telegraphic branches of the industry. Thus it came about that his early years were spent in close connection with telegraphic engineering, and throughout his life he numbered amongst his most valued friends many of the leaders of submarine cable manufacture. In those days, however, the electrical field was not subdivided into specialized plots; it was cultivated in common by all electricians, and the young engineer was as keenly interested in the beginnings of electric lighting as in telegraphy. He took part in the lighting of the Victoria Embankment by means of the Jablochkoff candle, and patented improvements in connection with this device and the Lontin arc lamp; he assisted in the illumination of Aldgate station for the first time with the latter; and he was also concerned in the development of the Werdermann arc lamp and the Gramme dynamo. His connection with telegraphy was maintained for many years by his inspection and testing of submarine cables at the makers' works, on behalf of Sir Samuel Canning.

The incandescent lamp in turn attracted his attention, and he invented and patented a device depending on the high temperature-resistance coefficient of iron wire, to protect the carbon filament from excess of pressure, for which he received a handsome price. Shortly afterwards he was invited by Mr. H. R. Kempe, another of Sabine's pupils, to edit the *Telegraphic Journal and Electrical Review*, of which Mr. H. Alabaster was the proprietor, and in 1883 he became a partner in the firm of H. Alabaster, Gatehouse and Co. From that time to his death his interests were bound up with those of the *Electrical Review*—a period of nearly 40 years, which was marked by never-failing prosperity. His literary gifts were of a high order, and were allied with personal qualities which not only gained for him a remarkable degree of popularity, but also contributed in no small measure to the success and renown of the journal which he edited.

Apart from his professional occupations, he was passionately devoted to music, and as a violinist attained a degree of skill to which few amateurs aspire. His

services as such were in constant request and freely given; for many years he took part in the work of the South London Institute of Music, and he was a founder of the Electro-Harmonic Society, of which he was musical director up to the time of his death on 31st March, 1921. He was also for 27 years the honorary leader of the Diss Choral Society. Amongst other activities may be mentioned his service as juror at various exhibitions (including the Milan Exhibition of 1906), his work on the Executive Committee of the Electrical Trades Benevolent Institution, and his connection with Freemasonry. He always held rigidly aloof, however, from any connection which might conceivably hamper him in the impartial and disinterested discharge of his editorial functions. Unshakably loyal to the friends of his youth, generous to a fault, and solicitous for the well-being of all with whom he came in contact, he had a remarkable personality, and stood high in the esteem of the electrical world as well as of the circles in which his private life was spent. He left a widow, a son and two daughters. He was elected an Associate of the Institution in 1877, and a Member in 1886. A. H. A.

CHARLES P. HAMMOND was born in 1859. He served a 6-years' apprenticeship with Messrs. Humphrys Tennant and Co., and was subsequently for $2\frac{1}{2}$ years engineer on a passenger ship. From 1873 to 1887 he was engaged in general electric lighting work, for part of the time with the Brush Company. In 1887 he joined the staff of Messrs. Hayward-Tyler and Co. to take charge of the electric light work in which the firm was then beginning to take an interest. For some time he was occupied in fitting up electric light installations on oil-tank steamers. This branch of the business proving successful, Mr. Hammond was engaged for a number of years in carrying out installations in many important public buildings and large private residences in London and the provinces. In 1904, when the firm moved their offices to their present address, it was decided, owing to the large number of new firms which had taken up the business, to discontinue this branch, and Mr. Hammond's energies were directed to the pump work which was more strictly in the line of the firm's regular manufacture. In this connection his experience as a marine engineer proved very valuable, and his genial temperament made him a host of friends, not only among marine engineers but also among the many civil engineers, merchants, etc., with whom he came into contact. He remained with Messrs. Hayward-Tyler and Co., Ltd., until within a few days of his death, which occurred on 13th March, 1921, after a short illness. He was elected a Member of the Institution in 1897.

WILSON HARTNELL, one of the oldest and most respected engineers in Leeds, died on the 10th November, 1920, in his 82nd year, and notwithstanding his great age he maintained to the last an active interest in engineering matters. In his earlier years he was in the drawing office of Messrs. Kitson & Co., Leeds, where he obtained considerable knowledge of locomotive design. Later, he was for a short time engaged in the manufacture of nuts and bolts. He was for some time agent in the Leeds district for Messrs. Crompton and Co. About 1876 he became connected with Messrs. Marshall, Sons & Co., Ltd., Gainsborough, in the design of horizontal engines for industrial purposes working at a boiler pressure of 80 lb. per square inch which, in those days, was about the maximum for which steam boilers were constructed. These engines were made in sizes of from 12 to 35 nominal horse-power and proved very successful—many of them are still working. They were fitted with Mr. Hartnell's patent governor and automatic expansion gear. Mr. Hartnell's name is very widely known in connection with his isochronous governor, which is still manufactured. This governor was notable for its sensitiveness, ease of regulation, and "powerfulness," and gave much better results than the throttle valve and slow-running heavy-weight governors previously used. Mr. Hartnell started in business for himself nearly 40 years ago and gave special attention to the design and manufacture of two-pole direct-current dynamos. His arc lamp also was at one time well known. For some years he made a speciality of the electric lighting of flour mills and was very well known in this connection. He became a Member of the Institution in 1903 and was Chairman of the North Midland Centre during the session 1912–13, in addition to which he served for some years on the Committee and was a frequent contributor to the discussions. Before the Leeds Corporation adopted electric traction for the tramways he strenuously advocated in well-reasoned letters to the newspapers the adoption of that system as against the then much advocated cable system. Long before the formation of associations for protecting the interests of electrical manufacturers and contractors he was successful in bringing about associated action. Whilst he did not take a prominent part in public affairs, he was ever ready both in trading and in civic life to stand up for what was for the benefit of his fellow citizens. He was a devoted churchman and at Roundhay, where he resided for a great many years, was a zealous member of the congregation of St. John's Church. As an indication of the esteem in which he was held locally, he was presented on the occasion of his 80th birthday with a silver rose bowl by a large number of his friends, including some of the most prominent citizens of Leeds. He suffered a great bereavement during the early stages of the War in the loss of his only son, Lieutenant Cuthbert Hartnell, who was killed in action in France. He was married twice and is survived by his widow and two daughters. J. D. B.

JOHN FLETCHER MOULTON, BARON MOULTON, P.C., K.C.B., C.B.E., M.A., F.R.S., was born on 18th November, 1844. He received his early training at the Wesleyan School at New Kingswood, near Bath, and afterwards entered St. John's College, Cambridge, where he had a brilliant career. In 1868 he became Senior Wrangler and took a gold medal at London University. In 1874 he was called to the Bar, where he rapidly became famous, particularly

in connection with patent cases. He was elected an Associate of the Institution in 1877, and a Member in 1881. His death, which occurred on 9th March, 1921, removed from English society a public man whose career made a special appeal in many different quarters and, among others, a very special one to electrical engineers. Being an eminent lawyer who for many years conducted a leading practice at the Bar in connection with patent and industrial cases it would, in any event, have been inevitable that he should be largely occupied with a branch of industry in which technical advance and practical extensions have been so greatly developed as they were in the electrical industries during the years covered by his active career. The early telephone litigation; the distribution of the London area among competing electric companies; the litigation that arose about the original incandescent electric lamp; the 3-wire system of laying distribution mains; the use of transformers in the distributing system; the distributing arc lamp; the protection of telephone circuits from interference by traction currents; all these matters, of capital importance in connection with the electrical engineering industry, were matters in the conduct of which he was largely occupied, and many are the stories told of his acumen and resource in dealing with them. One of these stories which can now be given to the larger world, tells how he astonished both his clients and the adversary in the Edison *v.* Holland action *re* the incandescent electric lamp. It was a very hotly contested action; physicists of great eminence were called on both sides to give evidence, and the question became vital whether certain forms of filament which Edison had described in his specification could be made to work. Eminent chemists had tried to make them and had failed. The plaintiff's experts were confident that they could succeed, but the only evidence which they could give was evidence of opinion and insufficient to counterweigh the definite evidence of failure adduced for the defence. Ultimately, in complete perplexity, the judge ordered a test experiment to be made in the presence of Sir George Gabriel Stokes. This was an experiment of great moment, having regard to the issues which hung upon it, and the plaintiff company proposed to go to their leading expert and engage him and his laboratory in their exclusive service until the test was over. They attended chambers to consult Mr. Moulton who, with Sir Richard Webster (Lord Alverstone), was acting as their counsel, as to the arrangement of details. He took their breath away by an emphatic negative and counter proposal. "The right way," said he, "to conduct this experiment is not in a laboratory at all but in a factory. Take a large room in your establishment at Ponders End, fit it up with complete appliances for the manufacture of these filaments and set a small staff of operatives to produce lamps from them. When the defendants come for inspection let them see, not an expert chemist exhibiting his unrivalled manipulative skill, but boys and girls making Edison filaments as a matter of business routine." The thing was done. A dozen operatives were engaged for a fortnight in producing the lamps in question and when the inspecting experts, Sir George Stokes, the reporter, and the experts of the other side came to inspect, they found an industry running on a small commercial scale. There were thousands of Edison lamps in stock, and scores in every stage of manufacture. Criticism was seen to be futile and for once a litigant convinced not only his judge, but his adversary also, that he was right.

It was not in connection with litigation only that Lord Moulton was effective; he had also a fine faculty of exposition which enabled him to address students with very marked success. His mind was comprehensive and his knowledge very extended, so that the material for helpful analogies came very easily to him. To the mathematical type of mind such working in analogy is very familiar. Famous examples occur in the application by Fresnel of the formulæ of wave motion to explaining the phenomena of light, and the elaboration by Clerk Maxwell of the pregnant electromagnetic theory by which the phenomena of electricity, magnetism and light are co-ordinated. In the perception of such underlying analogies Lord Moulton was singularly ready, and his utterances—especially his addresses to scientific bodies—owed much of their charm and effect to this great quality. But it was not as an expositor only that he came before the electrical world. In this department he made a notable contribution of pioneer work in the paper on the sensitive state of electrical discharges in rarefied gases which, in collaboration with the late Dr. Spottiswoode, at that time President of the Royal Society, he contributed to the "Proceedings" of that Society in 1879 and 1880.

It is not altogether possible to stifle a regret that a career so rich in promise should in the end have deviated from the path of scientific research into that of professional eminence. And yet, when all is considered, it would be unreasonable to quarrel with a fate which enabled Lord Moulton to discharge so important a part as that which he undertook, when over 70 years of age, in the organization of the supply of munitions during the great war. A personal influence and consideration which scientific eminence, however distinguished, would not alone have sufficed to secure for him, was necessary to the effective discharge of the onerous duties of Director General of Explosives Supplies. Any career so crowned is crowned with a very complete success, and we may well forbear to discuss alternatives in a case where it is so hard to suggest one which would, on the whole, be an improvement either from his point of view or from ours. J. W. G.

ALEXANDER MUIRHEAD, D.Sc., F.R.S., was elected an Associate of the Institution in 1871 and a Member in 1879. The following biographical sketch—slightly abbreviated—appeared in a Scottish paper with the advantage of corrections and the general approval of the Muirhead family.

"Alexander Muirhead was the second son of John Muirhead, at one time farmer and tenant of the historical Barley Mill at East Saltoun, and of his wife, Margaret Lauder. Born in 1848, he was rendered partially but permanently deaf by a fall from his nurse's arms. His early education was locally acquired, and, although of keen reasoning faculties, he does not appear to

have taken kindly at first to the arithmetical and other studies in which he afterwards excelled.

"His father, who had mechanical aptitudes, gave up farming at the age of forty and removed with his family to London, where he became known as a telegraph engineer, being a member of the well-known firm of Messrs. Latimer Clark, Muirhead, and Co. Young Alexander was sent to University School, London, and the boy, till now considered a dunce, steadily carried off more and more prizes, these culminating in the 'Cook Prize' for mathematics. Chemistry and Greek were also favourite branches of study. At his father's works, he came into contact with such visitors as Clerk Maxwell and Sir William Thomson, afterwards Lord Kelvin.

"University College, London, came next in his educational experience, and to the strong influence of Professor De Morgan, and his horror of inexactitude, it may be attributed that throughout his life he refrained from publishing his work—to the occasional annoyance, it may be said, of his friends. After being a private pupil in Dr. Matthiessen's laboratory at St. Bartholemew's Hospital, he took his degree of D.Sc. in the London University, became, in 1870, Fellow of the Chemical Society, in 1874 an original member of the Physical Society of London, and soon after Membre de la Société Française de Physique. In 1875, he succeeded in the task of duplicating cables. For a few years he became a consulting expert, with rooms in 'Elm Court,' but returned to research work; and many friends, including Kelvin, Rayleigh, Hopkinson, Lodge, and Silvanus Thompson, often met and discussed problems in his laboratory.

"At the urgent request of the cable companies, he took up the manufacture of instruments for cables, and many inventions of his came in succession to improve their working. In 1896, so important was the evidence he gave before the committee appointed to consider the practicability of laying a cable across the Pacific, that he was informed that he had saved the scheme from being dropped. In 1894, on hearing Oliver Lodge lecture on Hertzian waves, at the Royal Institution, he went to him with suggestions, and the two collaborated, until Dr. Muirhead's practical mind was satisfied they had a reliable system of signalling across space without directing wires. The lecture, it should be noted, was given before Signor Marconi began his remarkable work.

"In 1904, he was made F.R.S. But the lameness which had hampered him since an attack of influenza in 1891 now began to affect his health, and he was prevented, to his deep regret, from serving on the Board of the National Physical Laboratory. Then came the strain of war, and, though with the coming of peace his still alert mind returned to problems and new ideas, his bodily strength gradually failed, and he passed gently to rest at his residence, The Lodge, Shortlands, Kent, on 13th December, 1920, and was laid in the family grave at West Norwood.

"Dr. Muirhead was a man of warm affections, and of kindly and lovable disposition. He travelled widely, but retained to the end his strong attachment to the country and the district in which he was born. He married Miss Blomfield, of London, by whom he is survived, but has left no children."

To these general facts the present writer must be allowed to testify to the friendship in which Alexander Muirhead was held by the few who really knew him. He was a man who suffered under exceptional bodily disabilities, but his genius for accurate measurement and scrupulous care in recording observations were of great service in the early days of cable telegraphy and to practical electrical science generally. It may be said that his chief work lay in the direction of electrical capacity. His were the original standards of capacity, and his experiments became the basis for later official work on those standards at the National Physical Laboratory. Questions of permanence were dependent for some time on the past history of Muirhead's standards and modes of measurement. Moreover, it was by his invention of distributed capacity that he achieved his notable task of duplexing cables. Automatic and rapid sending and receiving devices are a severe test of the exactness of imitation in artificial lines, and it was primarily by realizing the requirements that must be satisfied by a good artificial line that he succeeded, after many trials, in devising good and practical arrangements for deep-sea cable duplex telegraphy; thus largely increasing the practical efficiency of every long-distance cable.

The work of men like Charles Hockin, Fleeming Jenkin, and Alexander Muirhead, and of many other telegraph engineers, guided of course largely by the theory of Lord Kelvin, was of the utmost service in rescuing electrical science from its vague and qualitative condition and putting it on a metrical basis. It may be difficult for the present generation of electrical engineers to realize that the very names, Ohm, Volt, Ampere, and Farad, were born during the memory of people still living, and that methods of measurement extremely familiar to them had all to be devised and brought into existence so recently. When cable telegraphy began, in the most infantile manner, in or about the year 1851, few could have imagined its present developments. While as for the heavier branch of the profession, the name "Dynamo" was introduced at a meeting of the British Association in the late seventies or early eighties of last century. Through all that interesting but strenuous period, to the general ignorance prevailing in which the early numbers of *The Electrician* bear witness, Alexander Muirhead and many another pioneer lived and worked. We owe a debt to such men, which it is well occasionally to recognize.

O. L.

CHARLES FREDERICK PARKINSON was born in 1864 and received his technical education at the Storey Institute, Lancaster. For 5 years he was an apprentice with the London and North-Western and Lancashire and Yorkshire Joint Railway Companies. On the completion of his apprenticeship he was appointed assistant engineer to Messrs. Storey Brothers and Co., Ltd., of Lancaster, which post he held for a year. He then joined the Metropolitan Railway Carriage Co., Ltd., of Lancaster, as chief engineer, in which capacity he had charge of the electrical department, and devised

and carried out tramway and power schemes both at home and abroad. In 1896 he was appointed consultant and resident engineer to the Morecambe Town Council and supervised the construction of the town's electricity supply system. On the completion of this work he became general manager and later drew up a complete scheme for the electrification of the borough tramways. In 1901 he was appointed Borough Electrical Engineer to the Paisley Corporation, which post he held until 1919. He took out many patents, among them being one for locking railway carriage doors when the train is in motion. Another, which was received only a few days after his death, which occurred on 8th December, 1920, was an improved inclinomoter for use on aircraft. He was elected an Associate of the Institution in 1897, and a Member in 1898. F. W. P.

ROBERT RANKIN was born in 1885 at Cairnbrock, Ayrshire, the son of James Rankin, farmer, and obtained his early education locally. After two years at the High School, Stranraer, he entered the Royal Technical College, Glasgow, and in 1906 went to Glasgow University. He served his apprenticeship with Messrs. Mavor and Coulson. In 1908 he obtained his D.Sc. in engineering science with special distinction, and in addition he received from the Glasgow University its certificate of proficiency in engineering science. He also received in 1906 the diploma of the Royal Technical College, Glasgow. For some time he was lecturer in electrical engineering at the Technical College, Coatbridge, and later was assistant lecturer at the Royal Technical College, Glasgow. In June, 1909, he joined the staff of the Chloride Electrical Storage Company, and in 1912 became chief engineer to the Electrical Power Storage Co., Ltd. In 1916 he purchased the business of Messrs. Van Raden and Co., Ltd., at Coventry, and it was to this business that he was devoting his energies at the time of his death. In 1919 he was appointed Chairman of the new Committee constituted by the Council to organize the Informal Meetings Section, and it is largely to his initiative and ingenuity that its success is due. A man of strong conviction and indomitable pluck, he was always ready to face the consequences of the forcible expression of his views. He was possessed of a keen sense of humour, and his ready wit, sometimes tinged with acid sarcasm, was welcomed no less by his opponents than by his friends. Time and opportunity had not served to enable him to reach a great position in the profession he had chosen, yet his death leaves in the minds of his friends and acquaintances the impression of an unfulfilled destiny. Some years ago he contracted valvular disease of the heart as a result of acute rheumatism, and eventually succumbed to that ailment on 17th February, 1921. He was elected an Associate Member of the Institution in 1910, and a Member in 1915. F. P.

ARTHUR WILLIAM SCLATER was a son of the late Robert Slater, J.P., F.G.S., and was born in 1865. Educated at St. Paul's School, he was a pupil of Professors Ayrton and Perry. In his early days he was connected with the Metropolitan Electric Supply Company as assistant engineer, and built up a reputation as a reliable engineer, one of his first considerations being to guard against risk of fire. He invented and patented many electrical devices for improving the standard of electric lighting work, and was early in the field with electrical arrangements for starting internal combustion engines. He was consulting engineer to the late London School Board, to the Guardians of Paddington, Croydon, etc., and to many London hospitals. He was engineer to the Galway Electric Co., and consulting engineer to the Honourable Society of Lincoln's Inn and the Warden and Fellows of Winchester College. He specialized in the electrification of large country estates for lighting, power, etc., and also carried out electrical work at many historical mansions, including the State rooms at Chatsworth, Chequers, Shirburn Castle, Devonshire House, etc. An indefatigable worker, he carried on during the war and the past few years under great physical difficulties. A master of detail, and endowed with consummate taste, as an expert in an advisory capacity he had few equals. He will live long in the memory of his clients, professional friends and employees, for his ability, fascinating personality, and never-failing kindness. He died on 21st July, 1921, while he was on a business tour in the North. He was elected an Associate of the Institution in 1886 and a Member in 1898. F. J. L.

CAPTAIN FREDERICK WILLIAM SMITH-CLEBURNE was born in 1867. He received his technical education at Finsbury Technical College, from 1886 to 1889, and at the same time was a pupil with Messrs. Woodhouse and Rawson. For the next two years he was employed by the Electrical Engineering Corporation in the erection of power plants. He was then appointed an assistant in the firm of Messrs. J. G. Statter and Company and was principally engaged in the test-room. From 1893 to 1896 he was successively branch manager to Messrs. Paterson and Cooper and English manager to Messrs. Calvert and Co. In 1896–97 he was engaged upon work for the city of Cork and also at Las Palmas. He then became Managing Director to Messrs. F. W. Smith and Co., Ltd., of Southport, tramway equipment and power-transmission contractors, a post which he held until his death on the 27th April, 1920. Prior to the War he was an officer in the Royal Engineers (T.F.) and on the outbreak of hostilities served as Camp Adjutant and Garrison Engineer at the camp for German prisoners in the Isle of Man. Under his supervision considerable improvements were carried out, including the installation of electric light and the erection of a large power station. He was elected an Associate of the Institution in 1891 and a Member in 1905.

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 22. DECEMBER 1920. INSTITUTION NOTES. [ISSUED WITH No. 296 OF THE JOURNAL.]

Arrangements for January and February 1921.

ORDINARY MEETINGS OF THE INSTITUTION.

(*Thursdays.*)

[To be held at the Institution of Civil Engineers, Great George-street, Westminster, S.W., at 6 p.m. (light refreshments 5.30 p.m.).]

Date. 1921.	*Author and Title of Paper.*
13 Jan.	Sir W. BRAGG, K.B.E., F.R.S.: "Electrons" (Twelfth Kelvin Lecture).
27 Jan.	G. A. JUHLIN: "Temperature Limits of Large Alternators."
10 Feb.	Discussion on "Electric Appliances for Domestic Purposes" (with introductory paper by Messrs. E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.
17 Feb.	Prof. E. WILSON: "Feebly Magnetic Materials" (Lecture I: Instrumentation).
24 Feb.	Prof. E. WILSON: "Feebly Magnetic Materials" (Lecture II: Results of Scientific Research).

INFORMAL MEETINGS.

(*Mondays.*)

[To be held, except where otherwise stated, in the Lecture-hall of the Chartered Institute of Patent Agents, Staple Inn Buildings (South Block), W.C., at 7 p.m.]

Date. 1921.	*Author and Title of Paper.*
17 Jan.	P. PITT, M.A.: "The Mental Equipment of an Engineer."
24 Jan. (6 p.m.)	Discussion on improving the usefulness of the Institution (*At the Institution of Civil Engineers, Great George-street, Westminster, S.W.*).
31 Jan.	L. MILNE: "Possible Economies in House Wiring."
14 Feb.	J. R. COWIE: "Some Notes on the Design of a Large Power Station."
28 Feb.	Informal Social Evening.

MEETINGS OF TERRITORIAL CENTRES AND SUB-CENTRES.

Date. 1921.	*Place of Meeting.*	*Author and Title of Paper.*
4 Jan.	Loughborough	Colonel NEWTON, D.S.O.: "The Experiences of a State-controlled Engineer."
10 Jan.	Newcastle	Visit of the President, Mr. Ll. B. ATKINSON.
10 Jan.	Dundee	W. SUTCLIFFE: "Electric Driving of Jute Factories" (Lecture).
11 Jan.	Leeds	Visit of the President, Mr. Ll. B. ATKINSON.
11 Jan.	Edinburgh	Captain R. C. TRENCH, R.E.: "Range of Wireless Stations."
11 Jan.	Manchester	R. O. KAPP: "Some Economic Aspects of E.H.T. Distribution by Underground Cables."
12 Jan.	Birmingham	R. O. KAPP: Some Economic Aspects of E.H.T. Distribution by Underground Cables."
17 Jan.	Liverpool	F. J. HEYES, B.Eng.: "Electric Arc Welding applied to Aluminium." J. H. PATERSON, D.Sc.: "The Chemical and Metallurgical Aspects of Electric Arc Welding." L. B. WILSON: "Cost of Electric Resistance Welding."
18 Jan.	Loughborough	E. G. PHILLIPS and H. B. CLARKE: "The Utilization of Exhaust Steam."
21 Jan.	Birmingham	Annual Dinner of the South Midland Centre.
24 Jan.	Newcastle	R. O. KAPP: "Some Economic Aspects of E.H.T. Distribution by Underground Cables."
25 Jan.	Leeds	W. B. WOODHOUSE: "The Distribution of Electricity."
25 Jan.	Manchester	G. A. JUHLIN: "Temperature Limits of Large Alternators."
26 Jan.	Sheffield	Discussion on Chairman's Remarks at Inaugural Meeting.
1 Feb.	Derby	W. O. RIDLEY: "Recent Developments in Wireless Telegraphy and Telephony."
2 Feb.	Birmingham	G. A. JUHLIN: "Temperature Limits of Large Alternators."
3 Feb.	Middlesbrough	Discussion of "Report on the Heating of Buried Cables."
7 Feb.	Bristol	Lieut.-Colonel H. E. O'BRIEN: "Application of the Electric Locomotive to Main-Line Traction on Railways."
7 Feb.	Liverpool	Discussion of "Report on the Heating of Buried Cables."
8 Feb.	Leeds	G. A. JUHLIN: "Temperature Limits of Large Alternators."
8 Feb.	Glasgow	A. E. MCCOLL: "Automatic Protective Devices for Alternating Current Systems."
8 Feb.	Manchester	Visit of the President, Mr. Ll. B. ATKINSON.
11 Feb.	Newcastle	Conversazione at Armstrong College.
14 Feb.	Newcastle	G. A. JUHLIN: "Temperature Limits of Large Alternators."
14 Feb.	Dundee	R. D. ARCHIBALD, B.Sc.: "Electrical Analogies."
15 Feb.	Nottingham	F. S. GROGAN: "Electrical Heating and Cooking."
18 Feb.	Aberdeen	J. FRASER: "Some Problems in Telegraphic Transmission."
22 Feb.	Leeds	(To be announced later.)

Date.	*Place of Meeting.*	*Author and Title of Paper.*
1921.		
22 Feb.	Manchester	E. A. WATSON: "Magnetos for Ignition Purposes in Internal Combustion Engines."
23 Feb.	Birmingham	Discussion on "Electric Appliances for Domestic Purposes" (with introductory paper by Messrs. E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.
23 Feb.	Sheffield	L. ROTHERA: "Factors which influence the Size of Rolling-mill Drives."
28 Feb.	Liverpool	G. A. JUHLIN: "Temperature Limits of Large Alternators."
28 Feb.	Newcastle	W. D. OWEN: "Thermionic Valves" (Lecture).

STUDENTS' SECTION.

MEETINGS.

Date.	*Author and Title of Paper.*
1921.	
21 Jan.	H. J. HOWARD: "Electric Welding." (*At Faraday House, Southampton Row, W.C.*, 6.30 *p.m.*)
4 Feb.	R. TAIT: "The Selection of Prime Movers for Small and Medium Power." (*At Finsbury Technical College*, 6.30 *p.m.*)
18 Feb.	Address by the President. (At King's College, Strand, W.C., at 6.30 p.m.)

ANNUAL DINNER.

26 Feb. At Gatti's Restaurant, Strand, London, W.C.

VISITS TO WORKS.

Date.	*Time.*	*Works to be visited.*
1921.		
14 Jan.	2 p.m.	The Osram-Robertson Lamp Works, Hammersmith.
29 Jan.	2 p.m.	The Associated Equipment Company Works, Walthamstow.
11 Feb.		Messrs. Everett, Edgcumbe & Co.'s Works, Hendon.
24 Feb.		Messrs. Bryant and May's Works, Bow.

The above visits, for which tickets of admission are necessary, are open only to Students of the Institution, and those wishing to attend should notify Mr. G. K. Perkins, 49, St. Gabriel's-road, N.W. 2, at least 10 days before each visit. In some cases the number forming the party is limited by the authorities of the works to be visited, and on these occasions the Committee will decide by ballot or otherwise which Students may attend.

Annual Dinner.

The Annual Dinner of the Institution will be held at the Hotel Cecil on 3rd March, 1921. Further particulars will shortly be announced.

Honorary Member.

Professor André Blondel has been elected by the Council an Honorary Member of the Institution.

Benevolent Fund.

The Council have received from Mr. E. M. Hughman, of Bombay, an offer of a donation of £250 to the Benevolent Fund on condition that 19 similar donations are received before 31st March, 1921. The Committee of Management of the Fund earnestly hope that the 19 donations specified will be forthcoming, as the fund will thereby be benefited to the extent of £5 000. All communications should be addressed to the Secretary of the Institution at 1, Albemarle-street, London, W. 1.

Alexander Graham Bell.

Professor Alexander Graham Bell, the inventor of the telephone, attended a meeting of the Wireless Section on the 24th November, 1920. An account of his remarks in reply to a resolution welcoming him to the meeting will be found on page 65 of No. 296 of the *Journal*. The following is a facsimile of his signature in the Attendance Book :—

Summer Meeting.

The next Summer Meeting of the Institution will be held in Scotland and will begin at Glasgow on Tuesday, 7th June, 1921, when a paper will be read by Mr. R. B. Mitchell on "The Dalmarnock Generating Station." Visits will be paid to this Station, to Works, and to a Clyde shipyard. There will also be a paper on the "Water Power Resources of Scotland." On Thursday, 9th June, the party will proceed to Fort William and will return the next day to Oban by steamer via Kinlochleven. This trip will give an opportunity for the party to see some of the most magnificent scenery of the Western Highlands. The meeting will end on Friday, 10th June. Further particulars will be circulated later.

Students' Section.

Mr. J. S. Highfield has been appointed by the Council a Consultative Member of the Students' Committee in place of Mr. G. W. Partridge.

"Beama" Journal.

The price of the above "Journal" for 1921 has been fixed at 2 shillings per copy, the yearly subscription being 30 shillings post free. The Association is willing to supply copies to Students of the Institution at the reduced price of 1s. 8d.

Associate Membership Examination.

It is proposed, if a sufficient number of candidates make application, to hold in London in April 1921

an examination for Associate Membership. Arrangements will also be made for the examination to be held at Local Centres in the United Kingdom and abroad.

Those desiring to sit for the examination should send their Entry Forms and Examination Fees in time to reach the Secretary of the Institution, 1 Albemarle-street, London, W. 1, not later than the 1st March, 1921.

Entry Forms and copies of the Examination Regulations may be obtained on application to the Secretary.

Membership Advisory Committee for India.

The following Committee has been appointed for the purpose of advising the Council in regard to applications for election of new members and for transfer from one class to another from candidates in India :—

Mr. J. W. Meares (Simla), *Chairman.*

Mr. A. H. Burbidge (Mandalay).
Mr. H. Burkinshaw (Calcutta).
Mr. A. C. Coubrough, C.B.E. (Calcutta).
Mr. H. P. Gibbs (Bombay).
Mr. J. F. Jones (Madras).
Mr. P. G. Moore (Cawnpore).
Colonel J. S. Pitkeathly, C.V.O., C.B.E., D.S.O. (Delhi).
Colonel W. Sutherland, V.D. (Calcutta).

And

Mr. C. C. T. Eastgate (Lahore), *Secretary to the Committee.*

Scholarships.

The following Scholarships have been awarded by the Council for the year 1920-21 :—

Salomons Scholarships (value £50 each) to :—Mr. E. T. Donovan (Birmingham University), Mr. J. Fowler (Leeds University), Mr. H. S. Petch (East London College), Mr. L. P. Turton (Liverpool University).

David Hughes Scholarships (value £50 each) to :—Mr. F. H. Cooper (Finsbury Technical College), Mr. C. Dannatt (Armstrong College), Mr. E. H. Lakey [City and Guilds (Engineering) College], Mr. R. C. Titman (Sheffield University).

Paul Scholarship (value £25) to :—Mr. A. G. Pearce (Finsbury Technical College).

The George Montefiore Endowment.

The Secretary has been notified that the next awards in connection with the above will be made in 1921. A prize or prizes not exceeding 20 000 francs in value will be awarded for the best paper or papers presented dealing with scientific developments or improvements in the technical applications of electricity. Papers, which must have been prepared within the last three years, may be in French or English, and must be either typewritten or printed. Further particulars can be obtained from M. le Secrétaire-Archiviste de la Fondation George Montefiore, Rue Saint-Gilles, 31, Liége, Belgium. The closing date for entries is the 30th April, 1921.

"Therm."

In view of the definition of the term "Therm" in the Gas Regulation Act 1920, as meaning 100 000 British thermal units, it has been decided to discontinue its use in the *Journal* for the "British thermal unit" and to use the abbreviation "B.Th.U." instead.

Wherever "therm" appears in Volumes 56, 57 and 58, and in the Supplement to Volume 57 of the *Journal*, it means one British thermal unit.

Informal Meetings.

The thirteenth Informal Meeting was held on Monday, 15th November, 1920, at 7 p.m., at the Chartered Institute of Patent Agents, Staple Inn Buildings, W.C. Mr. Llewellyn B. Atkinson, President, who took the chair, opened a discussion entitled "Is Specialization a Danger," in which the following members took part :—Messrs. F. Pooley, W. E. Warrilow, A. G. Healy, P. Pitt, R. W. Hughman, M. D. Hart, J. W Beauchamp, J. R. Bedford, J. R. Cowie, G. J. D. Scott, and A. E. Gott.

The fourteenth Informal Meeting was held on Monday, 29th November, 1920, at 7 p.m., at the Chartered Institute of Patent Agents, Staple Inn Buildings, W.C., Mr. W. L. Wreford in the chair. Mr. A. F. Harmer opened a discussion on "Electrical Transmission on Petrol Vehicles," in which the following took part :—Messrs. W. A. Stevens, W. Burton, M. D. Hart, J. H. Reyner, M. Whitgift, Lieut.-Colonel W. H. Prichard, Messrs. D. A. R. Partridge, J. R. Harding, Lieut.-Colonel A. E. Davidson, D.S.O., and Captain W. P. Durtnall.

The fifteenth Informal Meeting was held on Monday, 13th December, 1920, at 7 p.m., at the Chartered Institute of Patent Agents, Staple Inn Buildings, W.C., Mr. F. Pooley in the chair. Mr. W. J. Minton opened a discussion on : "(1) Percentage and Accuracies, (2) Meter Constants and Standards, (3) Nameplates," in which the following took part :—Messrs. B. B. Heaviside, K. Edgcumbe, J. R. Bedford, C. H. W. Gerhardi, J. F. Avila, M. D. Hart, A. E. Gott, P. M. Baker, A. B. Eason, G. D. Malcolm, R. F. Windett, F. A. Nield, E. W. Moss, E. W. Hill, A. F. Harmer, L. C. Gleaves, J. C. Elvy, and C. L. Lipman.

International Electrotechnical Commission.

The Papers Committee have decided that the symbols in papers published in the *Journal* shall be those adopted by the International Electrotechnical Commission, viz.

INTERNATIONAL SYMBOLS (FIRST PART).

LETTER SYMBOLS.

(The I.E.C. first issued these letter symbols in January 1913, and at the Plenary Meeting held in London in October 1919, certain small modifications were approved and are here incorporated.)

Introductory Remarks on the Standardization of Letter Symbols.

In so far as Electrotechnics alone are concerned, it would seem possible to standardize letter symbols, and the following principles have served as the basis of the work of the Special Committee (now known as the Advisory Committee) in the attainment of this object :

The letter symbols must be clearly distinguishable one from another when writing with a pen on paper, with chalk on a blackboard, or with a typewriter. In the printed text, it is advisable to use for the letter symbols a different type from that of the text. It is desirable also that, in ordinary handwriting, one should not be obliged to add distinctive signs to letter symbols to specify the type to be employed. It should be possible to spell out the letter symbols when writing them on the blackboard. Finally, preference should be given to those letter symbols already in common use. From this it will be seen that it is impossible to make a distinction, in ordinary handwriting, between Roman letters and italics, and that small roundhand letters, being too difficult to differentiate from the above, cannot be used. It is generally agreed to abandon Gothic type, as requiring too long a time in writing. Finally, many of the Greek capitals are identical with Roman capitals. Taking the above points into account, there remain about 100 symbols available in Roman, Script and Greek type, of which several are already used for mathematical symbols and which are necessary for the purposes of the electrician. A list of letter symbols most frequently needed in electrotechnics is appended herewith. Taking into account certain letter symbols which are occasionally made use of, it is obvious that there will be none left for purely physical or mechanical quantities. Thus, in the same formula, electrotechnical letter symbols may occur in conjunction with other letter symbols used in mechanics and physics generally; this is especially the case in equations containing mass, moment of inertia, speed, density, temperature, quantity of heat, etc. The I.E.C. recommends, therefore, that in such cases, for physical and mechanical quantities, the letter symbol habitually used by physicists and mechanical engineers should be employed, if this letter symbol does not already exist in the formula as an electrotechnical letter symbol. If, on the contrary, it already exists in the formula, it is desirable that it be accompanied by a distinctive sign or that the notation be changed.

Rules for Quantities.

(*a*) Instantaneous values of electrical quantities which vary with the time to be represented by small letters. In case of ambiguity, they may be followed by the subscript " t."

(*b*) Virtual or constant values of electrical quantities to be represented by capital letters.

TABLES OF LETTER SYMBOLS ADOPTED.

I. Quantities.

Name of quantity	Letter symbol	Letter symbols recommended for the case in which the principal letter symbol is not suitable
1. Length	l	In dimensional equations the capital letters L, M, T, are to be employed
2. Mass	m	
3. Time	t	
4. Angles	α, β, γ..	
5. Acceleration of gravity	g	
6. Work	A	W
7. Energy	W	U*
8. Power	P	
9. Efficiency	η	
10. Number of turns in unit of time	n	
11. Temperature Centigrade	t	$\theta\ \vartheta$
12. Temperature absolute	T	Θ
13. Period	T	
14. $2\pi/T$	ω	
15. Frequency	f	ν
16. Phase displacement	ϕ	
17. Electromotive force	E	
18. Current	I	
19. Resistance	R	
20. Resistivity	ρ	
21. Conductance	G	
22. Quantity of electricity	Q	
23. Flux-density, electrostatic	D	
24. Capacity	C	
25. Dielectric constant	ϵ	
26. Self-inductance	L	$\mathfrak{L}$
27. Mutual inductance	M	$\mathfrak{M}$
28. Reactance	X	$\mathfrak{X}$
29. Impedance	Z	$\mathfrak{Z}$
30. Reluctance	S	$\mathfrak{S}$
31. Magnetic flux	Φ	$\mathfrak{F}$
32. Flux-density, magnetic	B	$\mathfrak{B}$
33. Magnetic field	H	$\mathfrak{H}$
34. Intensity of Magnetization	J	$\mathfrak{J}$
35. Permeability	μ	
36. Susceptibility	κ	
37. Difference of potential, electric	V	U
38. Magnetomotive force	†	$\mathfrak{E}$

Script, heavy-faced or special type.

* This letter symbol is used in thermo-dynamics.
† Letter symbol to be proposed by the National Committees.

(*c*) Maximum values of periodic electrical and magnetic quantities to be represented by capital letters followed by the subscript " m."

(*d*) In cases where it is desirable to distinguish

between magnetic and electric quantities, constant or variable, magnetic quantities to be represented by capital letters of either script, heavy-faced or any special type. Script letters to be only employed for magnetic quantities.

(*e*) Angles to be represented by small Greek letters.

(*f*) Dimensionless and specific quantities to be represented, wherever possible, by small Greek letters.

II. Units. Abbreviations for Names of Units.

Abbreviations for names of Electrical Units to be employed only after numerical values :

Name of unit	Abbreviation
1. Ampere	A
2. Volt	V
3. Ohm	*
4. Coulomb	C
5. Joule	J
6. Watt	W
7. Farad	F
8. Henry	H
9. Volt-coulomb	VC
10. Watt-hour	Wh
11. Volt-ampere	VA
12. Ampere-hour	Ah
13 Milliampere	mA
14. Kilowatt	kW
15. Kilovolt-ampere	kVA
16. Kilowatt-hour	kWh

m for milli- μ for micro- or micr-
k for kilo- M for mega- or meg-

* As a sign for the ohm, one of the two letters O or Ω is provisionally recommended. The letter Ω should no longer be used for megohm.

III. Mathematical Letter Symbols and Rules.

Name	Letter symbol	Letter symbols recommended for the case in which the principal letter symbol is not suitable
Total differential	d	*d*
Partial differential	∂	
Base of Napierian logarithms	e	ϵ
Imaginary $\sqrt{-1}$	i	j
Ratio of circumference to diameter	π	
Summation	Σ	
Summation, integral	$\int$	

1. Ordinary numerals as exponentials shall exclusively be used to represent powers. (In consequence, it is desirable that the expression $\sin^{-1} x$, $\tan^{-1} x$, employed in certain countries be expressed by arc sin x, arc tan x.)

2. The comma and the full-stop shall be employed for separating the decimals according to the custom of the country, but the separation between any three digits constituting a whole number shall be indicated by a space and not by a full-stop or a comma (*Example*, 1 000 000).

3. For the multiplication of numbers and geometric quantities, indicated by two letters, it is recommended to use the sign ×, and the full-stop only when there is no possible ambiguity.

4. To indicate division in a formula, it is recommended that the horizontal bar or the colon be employed. Nevertheless the oblique line may be used when there is no possibility of ambiguity ; when necessary, ordinary brackets ( ), square brackets [ ], and braces { } may be employed to obtain clearness.

IV. Abbreviations for Weights and Measures.

Length :—m ; km ; dm ; cm ; mm ; μ = 0·001 mm.
Surface :—a ; ha ; m^2 ; km^2 ; dm^2 ; cm^2 ; mm^2.
Volume :—l ; hl ; dl ; cl ; ml ; m^3 ; km^3 ; dm^3. cm^3 ; mm^3.
Mass :—g ; t ; kg ; dg ; cg ; mg.

Committees, 1920-1921.

Among the Committees appointed by the Council for 1920-21 are the following :—

INFORMAL MEETINGS.

The President.

Mr. J. F. Avila.
Mr. W. E. Bradshaw.
Mr. A. B. Eason.
Mr. A. F. Harmer.
Mr. G. C. Law.
Mr. E. W. Moss.
Mr. F. Pooley.
Mr. W. E. Warrilow.
Mr. W. L. Wreford.

LIBRARY AND MUSEUM.

The President.

Col. R. E. Crompton, C.B.
Mr. D. N. Dunlop.
Mr. K. Hedges.
Mr. W. M. Mordey.
Lt.-Col. W. A. J. O'Meara, R.E., C.M.G.
Mr. C. C. Paterson, O.B.E.

"SCIENCE ABSTRACTS."

The President.

Prof. E. W. Marchant, D.Sc.
Mr. W. M. Mordey.
Mr. C. C. Paterson, O.B.E.
Dr. A. Russell.

And	*Representing*
Mr. W. R. Cooper..	The Physical Society of London.
Dr. D Owen ..	

SHIP ELECTRICAL EQUIPMENT.

The President.

Mr. J. H. Collie.
Mr. B. M. Drake.
Mr. A. Henderson.
Mr. J. W. Kempster.
Mr. J. F. Nielson.
Mr. N. W. Prangnell.
Mr. A. P. Pyne.
Mr. S. G. C. Russell.
Mr. T. A. Sedgwick.
Mr. C. H. Wordingham, C.B.E.

And	*Representing*
Sir W. S. Abel, K.B.E.	Lloyd's Register of Shipping.
Mr. J. T. Milton ..	
Mr. H. H. Berry .	British Electrical and Allied Manufacturers' Association.
Mr. C. Rodgers ..	
Mr. T. Carlton ..	Board of Trade.
Mr. W. Cross ..	Electrical Contractors' Association.
Mr. J. Foster King.	British Corporation for the Survey and Registry of Shipping.
Mr. J. Lowson ..	Institution of Engineers and Shipbuilders in Scotland.
	Electrical Contractors' Association of Scotland.
Mr. A. W. Stewart .	Institution of Naval Architects.
Mr. H. Walker, O.B.E.	N.E.Coast Institution of Engineers and Shipbuilders.

TERRITORIAL CENTRES.

The President.

Mr. H. J. Cash.	Mr. P. V. Hunter, C.B.E.
Sir J. Devonshire, K.B.E.	Sir W. Noble.
Mr. F. Gill.	Mr. C. H. Wordingham, C.B.E.
Mr. J. S. Highfield.	

WIRELESS SECTIONAL MEETINGS.

Dr. W. H. Eccles (*Chairman*)

The President.

Major E. A. Barker, M.C.	Sir Oliver Lodge, D.Sc.
Sir Charles Bright, F.R.S.E.	Prof. E. W. Marchant, D.Sc.
Mr. R. C. Clinker.	Capt. H. J. Round.
Prof. C. L. Fortescue.	Mr. A. A. C. Swinton, F.R.S.
Mr. A. Gray.	Mr. J. E. Taylor.
Prof. G. W. O. Howe, D.Sc.	Mr. L. B. Turner.
Admiral Sir H. B. Jackson, G.C.B.	Mr. C. H. Wordingham, C.B.E.

And	*Representing*
Major J. Erskine-Murray, D.Sc.	.. Air Ministry.
Mr. E. H. Shaughnessy, O.B.E.	.. Post Office.
Lt.-Col. H. Clementi Smith, D.S.O.	.. War Office.
Capt. J. K. Im Thurn, R.N., C.B.E.	.. Admiralty.

WIRING RULES.

The President.

Mr. H. J. Cash.	Mr. J. F. Nielson.
Mr. J. R. Cowie.	Mr. A. P. Pyne.
Mr. W. Cross.	Mr. E. Ridley.
Mr. J. Frith.	Mr. C. P. Sparks, C.B.E.
Dr. C. C. Garrard.	Mr. C. H. Wordingham, C.B.E.
Mr. P. V. Hunter, C.B.E.	
Mr. S. W. Melsom.	

And	*Representing*
Mr. B. M. Drake ..	Contractors (unofficially).
Sir T. O. Callender .	Cable Makers (unofficially).
Mr. J. F. W. Hooper	
Mr. A. C. Cockburn	Fire Offices (unofficially).
Mr. S. G. C. Russell	
Mr. A. L. Taylor ..	

And	*Representing*
Mr. J. R. Dick ..	The British Electrical and Allied Manufacturers' Association.
Mr. H. H. Berry ..	
Mr. W. F. Bishop..	The Cable Makers' Association.
Mr. E. J. B. Lowdon	The Electrical Contractors' Association of Scotland.
Mr. J. Christie ..	The Incorporated Municipal Electrical Association.
Mr. F. W. Purse ..	
Mr. E. T. Ruthven Murray	The Incorporated Association of Electrical Power Companies.
Mr. W. R. Rawlings	The Electrical Contractors' Association.
Mr. S. H. Webb ..	
Mr. J. M. Crowdy	Association of Supervising Electricians.
Mr. O. M. Andrews	Conference of Chief Officials of the London Electric Supply Companies.

SECTIONAL COMMITTEES.

Lighting and Power.

The President.

Mr. J. R. Beard.	Mr. G. W. Partridge.
Mr. S. E. Fedden.	Mr. W. M. Selvey.
Mr. J. S. Highfield.	Mr. C. P. Sparks, C.B.E.
Mr. E. T. Ruthven Murray.	Mr. B. Welbourn.
Mr. W. B. Woodhouse.	

Electricity in Mines.

The President.

Mr. W. A. Chamen.	Mr. W. C. Mountain.
Dr. C. C. Garrard.	Mr. W. H. Patchell.
Mr. J. A. B. Horsley.	Mr. W. M. Selvey
Mr. P. V. Hunter, C.B.E.	Mr. C. P. Sparks, C.B.E.
Mr. W. B. Woodhouse.	

Traction.

The President.

Sir J. Devonshire, K.B.E.	Mr. A. H. W. Marshall.
Mr. H. W. Firth.	Mr. G. W. Partridge.
Lt.-Col. F. A. Cortez-Leigh.	Mr. J. Sayers.
	Mr. R. T. Smith.
Mr. F. Lydall.	Mr. B. Welbourn.

Electro-Chemistry and Electro-Metallurgy.

The President.

Mr. W. A. Chamen.	Mr. C. P. Sparks, C.B.E.
Mr. S. E. Fedden.	Mr. J. Swinburne, F.R.S.
Mr. J. S. Highfield.	and 3 non-members of Council to be co-opted by the Committee.
Mr. W. M. Mordey.	

Telegraphs and Telephones.

The President.

Dr. W. H. Eccles.	Mr. H. H. Harrison.
Sir Charles Bright, F.R.S.E.	Sir W. Noble.
Mr. H. G. Brown.	Mr. F. Ryan.
Mr. F. Gill.	Mr. J. Sayers.
Sir W. Slingo.	

Representatives of The Institution on Other Bodies.

The following is a list of representatives of the Institution on other bodies and the dates on which they were appointed.

Birmingham Chamber of Commerce:
Mr. S. T. Allen (27 March, 1919).

Bradford Public Libraries Committee:
Mr. T. Roles (27 Feb., 1919).

Bristol University:
Mr. H. F. Proctor (6 Dec., 1917).

British Electrical and Allied Industries Research Association:
Mr. Ll. B. Atkinson (2 April, 1919).
Dr. C. C. Garrard (30 Oct., 1919).
Mr. J. S. Highfield (2 April, 1919).
Mr. C. C. Paterson, O.B.E. (4 Oct., 1917).
Mr. R. T. Smith (30 Oct., 1919).
Mr. C. P. Sparks, C.B.E. (4 Oct., 1917).
Mr. C. H. Wordingham, C.B.E. (4 Oct., 1917).

Sectional Committee on Electric Control Apparatus Research:
Mr. C. H. Wordingham, C.B.E. (22 Nov., 1920).
Major H. C. Gunton (2 Feb., 1921).

British Electrical Development Association:
Mr. R. Hardie (27 Jan., 1921).
Mr. B. Welbourn (27 Jan., 1921).
Mr. C. H. Wordingham, C.B.E. (18 Sept., 1919).

British Engineering Standards Association:
Main Committee:
Col. R. E. Crompton, C.B. (2 April, 1914).
Sir John Snell (2 April, 1914).
Mr. C. H. Wordingham, C.B.E. (26 Feb., 1920).

Sectional Electrical Committee:
Mr. F. Gill (21 May, 1914).
Mr. J. S. Highfield (21 May, 1914).
Mr. R. T. Smith (21 May, 1914).
Mr. C. H. Wordingham, C.B.E. (18 Nov., 1915).
Mr. W. B. Woodhouse (19 Dec., 1918).

Overhead Transmission Lines Material Sub-Committee:
Mr. C. H. Wordingham, C.B.E. (30 Oct., 1919).

Sectional Committee on British Standards in Colonial and Foreign Trade:
Mr. C. P. Sparks, C.B.E. (26 Oct., 1916).

Sectional Committee on Machine Parts and their Gauging and Nomenclature:
Mr. J. H. Rider (8 Feb., 1917).

Electrical Nomenclature and Symbols Sub-Committee:
Mr. C. C. Paterson, O.B.E. (8 Jan., 1920).

Corrosion Committee, Institute of Metals:
Mr. J. S. Highfield (4 Oct., 1917).

Darlington Board of Invention and Research:
Mr. R. M. Longman (15 May, 1919).
Mr. J. R. P. Lunn (15 May, 1919).
Mr. H. G. A. Stedman (15 May, 1919).

Fuel Economy Committee, British Association:
Mr. C. H. Wordingham, C.B.E. (9 Jan., 1919).

Imperial College of Science and Technology, Governing Body:
Mr. W. M. Mordey (30 Oct., 1919).

Imperial Mineral Resources Bureau Conference:
Mr. J. H. Rider (23 Jan., 1919).
Mr. W. B. Woodhouse (23 Jan., 1919).

Copper Committee:
Mr. B. Welbourn (18 Sept., 1919).

Miscellaneous Minerals Committee:
Prof. E. Wilson (18 March, 1920).

International Illumination Commission, British National Illumination Committee:
Prof. W. C. Clinton (13 Dec., 1917).
Mr. K. Edgcumbe (27 Nov., 1913).
Mr. Percy Good (18 Sept., 1919).
Mr. H. T. Harrison (27 Nov., 1913).
Prof. J. T. MacGregor-Morris (27 Nov., 1913)

International Scientific Unions:
Committee on International Union in Physics:
Dr. A. Russell (18 March, 1920).

Committee on International Union in Radio-Telegraphy:
Dr. W. H. Eccles (18 March, 1920).
Prof. G. W. O. Howe, D.Sc. (18 March, 1920).
Prof. E. W. Marchant, D.Sc. (18 March, 1920).

Labour, Ministry of, Trade Advisory Committee for the Electrical Industry (Disabled Sailors and Soldiers):
Mr. C. P. Sparks, C.B.E. (4 Oct., 1917).

Leeds Civic Society:
Mr. E. C. Wallis (27 March, 1919).

Leeds Municipal Technical Library Committee:
Mr. W. B. Woodhouse (19 Dec., 1918).

Metalliferous Mining (Cornwall) School, Governing Body:
Mr. J. S. Highfield (18 Sept., 1919).

Munitions Inventions Department, Nitrogen Products Committee:
Sir John Snell (12 Oct., 1916).

Munitions, Ministry of, Disposal of Surplus Government Property:
Mr. J. S. Highfield (13 Feb., 1919).

National Committee for Physics (Royal Society):
Dr. A. Russell (16 Dec., 1920).

National Committee in Radio Telegraphy (Royal Society):
Dr. W. H. Eccles (4 Aug., 1920).
Prof. E. W. Marchant, D.Sc. (4 Aug., 1920).

National Physical Laboratory, General Board:
Mr. Ll. B. Atkinson (21 Oct., 1920).
Mr. C. H. Wordingham, C.B.E. (22 Nov., 1917).

Röntgen Society Advisory Committee for British X-ray Industry:
Dr. W. H. Eccles (21 Feb., 1918).
Mr. J. E. Taylor (21 Feb., 1918).

Scientific and Industrial Research Advisory Council, Engineering Committee:
Mr. J. S. Highfield (9 March, 1916).

Scientific Societies, Conjoint Board of:
Mr. C. C. Paterson, O.B.E. (9 Dec., 1920).
Dr. A. Russell (9 Dec., 1920).

Education Committee:
Dr. A. Russell (12 Oct., 1916).

Technical Societies of Canada, Joint Committee:
Mr. S. L. B. Lines (8 March, 1917).

Transport Ministry, Advisory Panel and Committees:
Mr. Ll. B. Atkinson (30 Oct., 1919).
Sir J. Devonshire, K.B.E. (30 Oct., 1919).
Mr. J. S. Highfield (30 Oct., 1919).
Mr. R. T. Smith (30 Oct., 1919).
Sir John Snell (30 Oct., 1919).
Mr. C. P. Sparks, C.B.E. (30 Oct., 1919).
Mr. C. H. Wordingham, C.B.E. (30 Oct., 1919).

War Office Committee on Engineer Organization:
Mr. C. H. Wordingham, C.B.E. (10 April, 1919).

Accessions to the Reference Library.

ASKENASY, P. Einführung in die technische Elektrochemie. 2 Bänd[e]. 8vo. *Braunschweig*, 1910–16
1, Elektrothermie.
2, Ausgewählte Kapitel der Elektrolyse, wässeriger Lösungen und die Gewinnung des Aluminiums.

ASSOCIATION SUISSE DES ELECTRICIENS. Prescriptions sur l'établissement et l'entretien des installations électriques intérieures.
3e éd. sm. 8vo. 172 pp. *Zurich*, 1919

BATES, L. W. Colloidal fuel: composites of oil and carbon. [A collection of pamphlets on colloidal fuel by Lindon W. Bates, Dr. S. E. Sheppard, H. O'Neill, H. G. Barnhurst and Lindell T. Bates. Reprinted]. la. 8vo. 179 pp. *London*, 1918–20

BRITISH ENGINEERING STANDARDS ASSOCIATION. [Publications]. no. 73, 79, 95, 98, 105, 107, 111–114, 122, 132, 136. 8vo. *London*, 1919
73, British standard specification for five-ampere, fifteen-ampere and thirty-ampere two-pin wall plugs and sockets for domestic purposes (without earthing connection). (Revised December, 1919.)
79, Report on British standard special trackwork for tramways.
95, British standard tables for use in engineering workshops giving corrections to effective diameter required to compensate pitch and angle errors in screw threads of Whitworth form.
98, British standard specification for goliath lamp caps and lamp holders, etc.
105, British standard sections of light and heavy bridge type railway rails.
107, British standard rolled sections for magnet steel.
111, British standard schedule of wrought steels for aircraft.
112, British standard schedule of cold worked steels for aircraft.
113, British standard schedule of sheet steels for aircraft.
114, British standard schedule of valve and valve spring steels for aircraft.
122, British standards for milling cutters and reamers.
132, British standard specification for steam turbines for electrical plant.
136, Interim British standard terms and definitions for use in connection with automatic telephone systems.

CARPMAEL, H. Electric welding and welding appliances.
la. 8vo. 140 pp. *London*, 1920

CODD, M. A. Induction coil design.
8vo. 238 pp. *London*, 1920

DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH. Advisory Council. Bulletin no. 4, Memorandum on solid lubricants. 8vo. 28 pp. *London*, 1920

DICK, J. R., and FERNIE, F. Electric mains and distributing systems.
2nd ed. 8vo. 476 pp. *London*, 1919

GARRARD, C. C. Electric switch and controlling gear. A handbook on the design, manufacture and use of switchgear and switchboards in central stations, factories and mines.
2nd ed. 8vo. 676 pp. *London*, 1920

GASTER, L., and DOW, J. S. Electric lighting in the home. sm. 8vo. 31 pp. *London, n.d.*

—— Modern illuminants and illuminating engineering.
2nd ed. 505 pp. *London*, [1919]

GENERAL ELECTRIC COMPANY, *Schenectady, N.Y.* X-ray studies. [A collection of papers describing original researches which have been made by various members of the staff of the Research Laboratory of the G.E.C., relating to the subject of X-rays, 1912–1919].
4to. 292 pp. *Schenectady, N.Y.*, 1919

GOLDSMITH, H. E. Modern road construction and maintenance. 8vo. 150 pp. *Hongkong*, 1920

HALER, P. J., and STUART, A. H. A second course in mathematics for technical students.
sm. 8vo. 370 pp. *London*, 1920

HARVARD UNIVERSITY. Contributions from the Jefferson Physical Laboratory and from the Cruft High-Tension Electrical Laboratory of Harvard University. vol. 13: 1916–1918.
la. 8vo. *var. pagin. Cambridge, Mass., n.d.*

HOME OFFICE. FACTORIES AND WORKSHOPS. Annual report of the Chief Inspector of Factories and Workshops for the year 1919.
fol. 124 pp. *London*, 1920

HYDRO-ELECTRIC POWER COMMISSION, ONTARIO. Hydro-electric power in the Niagara District, Province of Ontario, Canada. 4to. 27 pp. *n.p.*, 1920

—— Report on hydro-electric railways: Toronto–Hamilton–Niagara Falls, Toronto and Eastern and Hamilton–Galt–Guelph–Elmira. By W. S. Murray. 8vo. 15 pp. *n.p.*, [1920]

IBBETSON, W. S. Tramway motorman's handbook.
sm. 8vo. 214 pp. *London*, 1920

ILLINOIS, UNIVERSITY OF. Engineering Experiment Station. Circular no. 7, Fuel economy in the operation of hand fired power plants. Prepared under the direction of a committee.
8vo. 94 pp. *Urbana, Ill.*, 1918

INTERNATIONAL ELECTROTECHNICAL COMMISSION. Publication no. 33, 34. 4to. *London*, 1920
33, Report of Fourth plenary meeting, London, Oct., 1919.
34, I.E.C. rules for electrical machinery. vol. I.

JOHNSON, V. E. Modern high-speed influence machines. Their principles, construction and applications to radiography, radiotelegraphy, spark photography, electro-culture, electro-therapeutics, high-tension gas ignition, the testing of materials, etc.
8vo. 285 pp. *London*, 1921

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 23. JANUARY 1921. INSTITUTION NOTES. [ISSUED WITH No. 297 OF THE JOURNAL.]

Arrangements for February and March 1921.

ORDINARY MEETINGS OF THE INSTITUTION.

(Thursdays.)

[To be held at the Institution of Civil Engineers, Great George-street, Westminster, S.W., at 6 p.m. (light refreshments 5.30 p.m.).]

Date. 1921.	*Author and Title of Paper.*
10 Feb.	Discussion on "Electric Appliances for Domestic Purposes" (with introductory paper by Messrs. E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.).
17 Feb.	Prof. E. WILSON: "Magnetic Susceptibility of Low Order" (Lecture I: Instrumentation).
24 Feb.	Prof. E. WILSON: "Magnetic Susceptibility of Low Order" (Lecture II: Results of Scientific Research).
10 Mar.	Prof. E. WILSON: "Magnetic Susceptibility of Low Order" (Lecture III: Practical Applications).
17 Mar.	Sir W. NOBLE: "The Long-distance Telephone System of the United Kingdom."

MEETING OF THE WIRELESS SECTION.

(Wednesday.)

[To be held at the **Institution of Mechanical Engineers,** Storey's Gate, Westminster, S.W., at 6 p.m. (light refreshments 5.30 p.m.)]

16 Mar.	Dr. W. H. ECCLES and Miss W. A. LEYSHON: "Some Thermionic Tube Circuits for Relaying and Measuring." G. STEAD, M.A.: "The Effect of Electron Emission on the Temperature of the Filament and Anode of a Thermionic Valve."

INFORMAL MEETINGS.

(Mondays.)

[To be held, except where otherwise stated, in the Lecture-hall of the Chartered Institute of Patent Agents, Staple Inn Buildings (South Block), W.C., at 7 p.m.]

Date. 1921.	*Author and Title of Paper.*
14 Feb.	J. R. COWIE: "Some Notes on the Design of a Large Power Station."
28 Feb.	Informal Social Evening (at Anderton's Hotel, Fleet-street, E.C.).
14 Mar.	Discussion on "Rectifiers" (opened by Mr. R. L. MORRISON)

MEETINGS OF TERRITORIAL CENTRES AND SUB-CENTRES.

Date. 1921.	*Place of Meeting.*	*Author and Title of Paper.*
1 Feb.	Derby	W. O. RIDLEY: "Recent Developments in Wireless Telegraphy and Telephony."
2 Feb.	Birmingham	G. A. JUHLIN: "Temperature Limits of Large Alternators."
3 Feb.	Middlesbrough	Discussion of "Report on the Heating of Buried Cables."
7 Feb.	Bristol	Lieut.-Colonel H. E. O'BRIEN: "Application of the Electric Locomotive to Main-Line Traction on Railways."
7 Feb.	Liverpool	Discussion of "Report on the Heating of Buried Cables."
8 Feb.	Leeds	G. A. JUHLIN: "Temperature Limits of Large Alternators."
8 Feb.	Glasgow	A. E. MCCOLL: "Automatic Protective Devices for Alternating Current Systems."
8 Feb.	Manchester	Visit of the President, Mr. Ll. B. ATKINSON.
11 Feb.	Newcastle	Conversazione at Armstrong College.
14 Feb.	Newcastle	G. A. JUHLIN: "Temperature Limits of Large Alternators."
14 Feb.	Dundee	R. D. ARCHIBALD, B.Sc.: "Electrical Analogies."
15 Feb.	Nottingham	F. S. GROGAN: "Electrical Heating and Cooking."
18 Feb.	Aberdeen	J. FRASER: "Some Problems in Telegraphic Transmission."
22 Feb.	Leeds	E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.: "Some Thermal Characteristics of Electric Ovens and Hot-plates."
22 Feb.	Manchester	E. A. WATSON: "Magnetos for Ignition Purposes in Internal Combustion Engines."
23 Feb.	Birmingham	Discussion on "Electric Appliances for Domestic Purposes" (with introductory paper by Messrs. E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.).
23 Feb.	Sheffield	L. ROTHERA: "Factors which influence the Size of Rolling-mill Drives."
28 Feb.	Liverpool	G. A. JUHLIN: "Temperature Limits of Large Alternators."
28 Feb.	Newcastle	W. D. OWEN: "Thermionic Amplifiers" (Lecture).
1 Mar.	Loughborough	G. A. PEARSON: "Phase Advancers, Rotary and Static."
7 Mar.	Cardiff	W. T. KERR: "The Prospects of Electricity in Agricultural Centres" (Lecture).

MEETINGS OF TERRITORIAL CENTRES AND SUB-CENTRES—*continued.*

Date.	*Place of Meeting.*	*Author and Title of Paper.*
1921.		
8 Mar.	Leeds	W. M. Miles: "Modern Feed-water Treatment." (Lecture.)
8 Mar.	Manchester	A. B. Mallinson: "Electric Driving in the Paper Mill, on Heat Economy Lines."
8 Mar.	Edinburgh	Sir W. Noble: "The Long-distance Telephone System of the United Kingdom."
14 Mar.	Newcastle	Discussion on "The Board of Trade Regulations."
16 Mar.	Birmingham	Prof. F. W. Burstall, M.Sc., M.A.: "Possibilities of Power Production." (Lecture.)
21 Mar.	Liverpool	A. B. Mallinson: "Electric Driving in the Paper Mill, on Heat Economy Lines."
22 Mar.	Manchester	Sir W. Noble: "The Long-distance Telephone System of the United Kingdom."
30 Mar.	Sheffield	H. J. Round: "Direction and Position Finding."

STUDENTS' SECTION.

Meetings.

Date.	*Author and Title of Paper.*
1921.	
4 Feb.	R. Tait: "The Selection of Prime Movers for Small and Medium Power." (*At Finsbury Technical College, 6.30 p.m.*)
18 Feb.	Address by the President. (*At King's College, Strand, W.C., 6.30 p.m.*)
4 Mar.	A. Rosen, B.Sc.: "Telephonic Transmission through Submarine Cables." (*At Faraday House, Southampton Row, W.C., 6.30 p.m.*)
11 Mar.	J. A. Broughall, B.Sc.: "Some Recent Developments in Converting Machinery for Small Substations." (*At King's College, Strand, W.C., 6.30 p.m.*)

Visits to Works.

Date.	*Works to be visited.*
1921.	
11 Feb.	Messrs. Everett, Edgcumbe & Co.'s Works, Hendon.
24 Feb.	Messrs. Bryant and May's Works, Bow.
11 Mar.	The Electrophone Exchange, Gerrard-street.

The above visits, for which tickets of admission are necessary, are open only to Students of the Institution, and those wishing to attend should notify Mr. R. C. Hawkins, 133 Park-lane, N. 16, at least 10 days before each visit. In some cases the number forming the party is limited by the authorities of the works to be visited, and on these occasions the Committee will decide by ballot or otherwise which Students may attend.

Kelvin Medal.

The first triennial award of the Kelvin Medal has been made by the Award Committee of the Kelvin Memorial Fund to Dr. W. C. Unwin, F.R.S.

New Vice-President.

At the Ordinary Meeting of the Institution on the 10th February, 1921, the President announced that the Council had accepted with regret the resignation of Mr. S. L. Pearce, C.B.E., from the office of Vice-President on grounds of health, and had elected Mr. A. A. Campbell Swinton, F.R.S., to fill the vacancy.

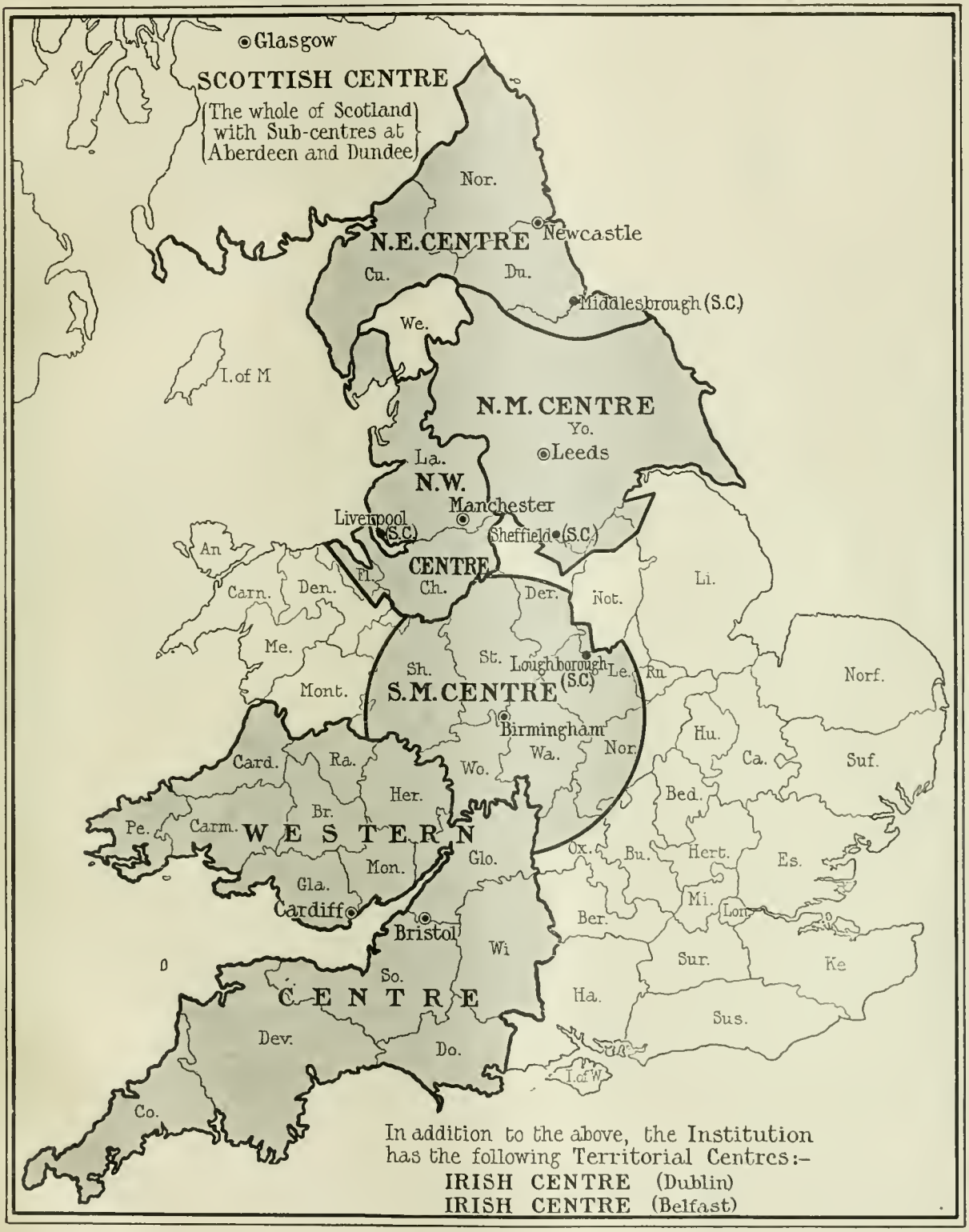
Benevolent Fund.

Donations from the following were reported by the President at the Ordinary Meeting held on the 10th February, 1921:

B. N. Adams, W. J. Addison, E. O. Alabaster, C. D. Alder, D. W. Aldridge, G. A. Allan, P. F. Allan, A. H. Allen, W. R. Alston, H. M. Anderson, O. M. Andrews, R. Ardis, J. M. Armstrong, K. N. Arnold, J. W. Astley, R. E. Atkins, Ll. B. Atkinson, A. Bailey, A. R. E. Bailey, F. J. Baldwin, C. M. Ballard, E. Balmford, B. Barber, A. G. S. Barnard, P. C. Barrand, S. J. Bartlett, S. Batten, G. R. Battle, L. Batty, E. Bawtree, H. E. I. Bax, C. W. Bayliss, M. Beales, J. R. Beard, C. A. Beaton, A. W. Beaumont, J. T. Bedford, D. E. Bell, L. M. T. Bell, C. H. Bennett, H. W. J. Bennett, J. J. C. Berkeley, F. Birtwistle, C. E. Bisdee, D. W. Bishop, R. S. C. Blance, E. H. Bond, A. F. Bound, I. Braby, H. P. Bramwell, F. G. Briant, W. R. O. Bridgeman, J. C. Briggs, R. E. Bristow, S. K. Broadfoot, R. V. Broberg, H. Brooke, A. E. Brooks, L. G. Brough, H. Brown, W. Brown, W. L. Brown, G. H. Browne, W. Brumwell, R. H. Bryans, J. D. Buckley, H. L. Buckman, H. S. Bulley, N. K. Bunn, A. F. Burgess, W. Burgess, H. Burgoine, G. V. B. Burgoyne, J. Burke, P. H. C. Butcher, W. H. M. Butcher, A. Butler, J. H. Buttress, J. F. Caine, J. M. Calder, H. G. Cameron, A. C. Campbell, H. S. Cannon, A. G. Cardrey, H. S. Carnegie, J. L. Carr, W. N. Carson, E. L. R. Chamberlain, W. A. Chamen, W. F. Chamen, C. C. Charlton, H. C. Charlton, A. R. Chaytor, G. A. Cheetham, G. G. Chisholm, J. M. Christie, B. G. Churcher, L. J. Clark, R. C. L. Clark, J. C. Clarke, P. Clegg, T. Clutterbuck, G. D. Coe, R. T. Coe, J. Waley Cohen, C. H. Colborn, A. Collins, H. J. Collyer, J. B. Colquhoun, N. J. Cook, R. Cook, W. K. Cook, W. W. Cook, A. C. Corner, A. W. Costin, A. B. Cousins, S. Craig, R. L. Cribbes, E. J. Cripps, C. Crompton, O.B.E., L. H. Crowther, W. Cuffley, E. Cunningham, I. S. Dalgleish, J. D. Dallas, J. F. F. Dalston, A. J. Dalton, H. E. Dance, C. Dannatt, C. H. Davidson, D. P. Davies, G. H. Davies, P. G. Davies, W. D. Davies, B. M. J. Davis, H. E. Davis, H. F. E. Deane, J. D. Dennison, E. D. Dent, R. H. Dickinson, The Diesel Engine Users' Association, A. N. Dixey, H. S. Double, M. Doxat-Pratt, J. P. A. Drewry, L. Drucquer, G. L. Drury, H. Dutton, A. H. Dykes, H. C. Eager, P. L. Edmonds, L. Edwards, H. S. Edwin, C. J. A. Ellis, G. H. Elsden, C. F. Elwell, C. W. Emanuel, L. H. Euler, B. Evans, C. L. Evans, S. Evershed, J. F. E. Farrell, M. Farrer, P. V. Fearon, H. H. Fell, A. B. Field, C. E. Field,

[*Continued on page* (12)]

I.E.E. Territorial Centres and Sub-Centres.



In addition to the above, the Institution has the following Territorial Centres:–

IRISH CENTRE (Dublin)

IRISH CENTRE (Belfast)

A. J. I. Fincham, A. H. Finlay, G. H. Fletcher, F. M. Foster, W. Foulds, D. S. Foulkes-Roberts, M. W. Foulkes-Roberts, C. F. Fowler, P. S. Fox, H. G. Frampton, F. H. Francis, M. R. Francis, H. W. Franks, W. A. Frazer, P. Freedman, B. French, C. A. Friendship, F. G. Frost, R. A. Frost, A. Fuchs, R. E. Gamlen, J. C. Garnes, E. A. Gatehouse, W. J. Gear, D. C. Gerrard, H. C. Gibson, H. J. Gibson, H. W. Gilbert, F. Gill, O.B.E., M. McA. Gillespie, G. A. Gillman, C. G. Glass, E. L. Glew, E. H. Glover, H. Gobie, M. L. Gorham, W. C. Gorrie, H. J. Grapes, E. C. Green, F. W. Green, F. Y. Grepe, W. H. F. Griffiths, S. J. Grose, J. L. Gurney, W. Gwynn, D. C. J. Halford, M. H. Hall, E. Halton, C. E. Hambleton, F. J. Hancock, L. S. Harley, P. Harris, R. F. Harris, A. W. Harrold, A. F. S. Harvey, F. B. O. Hawes, J. R. Hawes, J. C. Hawkhead, H. Hazelwood, K. W. Hedges, W. E. Hedley, V. J. Hegney, Messrs. W. T. Henley's Telegraph Works Co., Ltd., A. S. Herbert, C. J. Hickleton, W. A. Higginbotham, J. Higham, W. E. Highfield, E. P. Hill, H. Hirst, H. S. Hirstendahl, W. E. Hobbs, W. Hodson, P. M. Hogg, R. H. Holleyman, A. S. Hollin, J. H. Holmes, S. Holmes, W. Holmes, T. Hood, A. S. Hooppell, W. D. Horsley, A. Howard, C. Howard, P. Huggins, P. R. Hughes, R. G. Hullah, R. W. Humm, F. W. Hutton, T. Iddon, The Incorporated Municipal Electrical Association (per Mr. A. C. Cramb), The Informal Meetings Committee (per Mr. F. Pooley), O. Inman, Admiral Sir H. B. Jackson, G.C.B., K.C.V.O., F.R.S., H. B. Jackson, A. Jarratt, L. B. G. Jeffery, F. K. Jewson, T. M. Johnson, P. S. Jolin, V. N. Jolliffe, C. E. Jones, W. G. Jones, H. H. Jowers, W. E. Kidner, W. N. Kilner, E. Kitchen, H. A. Knowles, T. A. Lacey, J. W. Laing, E. H. Lakey, L. J. G. Lambert, W. Lang, A. W. Langridge, K. E. Latimer, N. Lawrence, S. Lawton, T. A. Ledward, A. H. Leeves, E. Lerpiniere, G. Lewis, E. C. Linay, J. W. Lincoln, G. S. Lisle, P. H. Lissenden, J. Lockhart, A. L. Long, H. H. Long, J. N. Long, W. A. R. Long, L. P. Lowry, J. R. P. Lunn, A. McClelland, D. J. McCourt, H. R. McDermott, G. Macdonald, J. McDonald, W. McGeoch, G. W. Macilwraith, W. M. Mackay, J. McLaren, I. K. McLean, R. F. J. Maidment, Sir H. Mance, LL.D., C.I.E., G. W. Mansfield, W. B. Marr, F. R. Marsh, J. R. Marsh, C. F. B. Marshall, J. M. Marshall, S. W. Marsland, W. J. Marston, J. Martin, A. W. Matson, H. A. Matt, J. M. S. Maxwell, A. E. May, C. G. Mayo, F. C. Meaby, A. Mears, W. B. Medlam, A. C. Merry, C. H. Merz, W. G. Messer, P. Miley, W. C. Millard, H. W. Miller, S. C. Miller, S. Millington, J. E. Minchen, J. Mirrey, A. P. Mitchell, J. F. Mitchell, F. E. Mitton, A. T. K. Moir, T. J. Monaghan, W. Montague, W. Morecombe, I. Morgan, R. Morley, A. Morris, A. J. Morris, W. M. Morrison, J. A. Morton, J. S. Morton, H. Moss, G. E. W. Murray, W. Murray, H. Murray-Russell, E. A. Nash, S. H. Nash, R. O. B. Neil, C. W. G. Nelson, R. Nelson, G. O. Nevile, E. W. J. Newland, A. J. Newman, F. A. Nicholls, G. F. Nicholson, J. K. A. Nicholson, Sir W. Noble, A. C. Norman, L. G. Norton, T. J. F. Oldham, R. Orr, A. C. Padley, J. B. Palmer, W. G. Palmer, H. Parker, H. Parry, C. C. Paterson, O.B.E., H. Paterson, E. R. Patrick, R. N. Patterson, N. B. Payne, J. G. Pearce, S. L. Pearce, C.B.E., J. D. Peattie, T. Pedrick, A. C. Pegg, W. S. Penstone, J. N. R. Perks, F. H. Pernet, C. S. Perry, A. G. Phillips, L. W. Phillips, A. Pickersgill, D. F. Pilkington, K. Pittaway, V. L. J. Plascott, D. E. Pledge, H. Pollock, W. F. Polson, A. Prado, T. W. Price, H. F. Proctor, C. Prosser, J. R. Protheroe, F. W. Purse, L. Quilliam, C. J. Radcliffe, W. E. Radforth, G. S. Ram, A. J. Ramsay, H. A. Ratcliff, G. Rattenbury, W. R. Rawlings, J. Rendell, G. A. Rendle, J. H. Reyner, C. G. Richards, F. Richardson, W. O. Ridley, F. Riley, F. M. Robb, D. E. Roberts, E. J. Roberts, H. Robertson, G. Robinson, W. M. Robinson, H. M. Rochester, P. Rocks, G. D. Rogers, F. A. Ross, M. E. Rowe, A. Rushton, Dr. A. Russell, S. G. C. Russell, E. W. Saggerson, W. Samples, H. P. Samuel, H. E. Sancto, R. Savory, J. E. Sayers, F. H. Schroeder, G. J. D. Scott, A. C. Seager, A. J. L. Seccombe, E. Seddon, A. Serner, F. Shakeshaft, E. E. Sharp, H. Shaw, W. Shead, A. Shearman, J. E. Shepherd, A. H. Short, W. J. C. Short, J. O. Sibree, W. Sinclair, A. W. Sirett, S. R. Siviour, A. E. Slack, L. Slattery, J. A. Slee, F. Smethurst, B. R. Smith, J. A. Smith, N. V. Snipe, E. H. Snook, H. C. Sparks, C.M.G., D.S.O., P. G. Spary, H. C. Spence, R. J. Spencer-Phillips, C. E. Springsguth, G. J. Stanley, E. Steadman, H. E. Steel, W. H. Steele, T. F. Stent, J. W. Stobart, H. Stokes, R. W. Street, A. Strickland, J. U. Stuart, W. S. Stuart, S. W. A. Sturman, J. A. Sutcliffe, W. E. Swale, A. L. Tackley, G. A. Tatchell, F. D. Taylor, J. Taylor, J. E. Taylor, F. J. Teago, N. S. Tennant, J. R. Thavenot, D. Thomas, J. Thomas, S. Thomas, T. R. Thomas, W. G. Thomas, F. A. Thompson, H. J. D. Thompson, J. S. Thomson, J. Torrance, R. G. Torry, J. Toulmin, T. D. Trees, H. W. Tremayne, S. H. Trippe, F. J. Tucker, H. G. Tumilty, J. Turner, L. Turton, A. E. Underdown, D. J. Unwin, S. Upton, J. H. Valentine, C. Vernier, B. J. Vine, A. Vines, J. C. Wade, C. F. A. Wagstaffe, H. Waizborn, A. Walker, E. Walker, H. B. Walker, Dr. R. M. Walmsley, T. C. T. Walrond, A. Walsh, J. M. Walshe, L. E. Ward, R. G. Ward, T. M. E. Ward, A. E. Warren, H. W. Warwick, C. T. Waterhouse, F. Waters, E. A. Watson, O.B.E., E. P. Watson, C. E. Webb, W. Weir, D. Welsford, R. H. Whalley, H. Whewell, A. C. Whish, A. White, A. E. White, J. R. Wilkinson, A. Willcock, J. S. P. Willcocks, H. Williams, J. W. Williams, L. F. Willson, N. J. Wilson, B. R. Wingfield, J. P. Winn, H. H. Withrington, J. T. Wolfenden, G. W. L. Wolland, A. R. Wood, L. E. Wood, R. C. Woods, C. H. Woodward, E. H. E. Woodward, T. H. R. Wray, C. H. Wright, E. L. Wright, F. Wright, N. Yeadon, and W. Young.

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 24. FEBRUARY 1921. INSTITUTION NOTES. [ISSUED WITH No. 298 OF THE JOURNAL.]

Arrangements for March, April and May, 1921.

ORDINARY MEETINGS OF THE INSTITUTION.

(*Thursdays.*)

[To be held at the Institution of Civil Engineers, Great George-street, Westminster, S.W., at 6 p.m. (light refreshments 5.30 p.m.).]

Date. 1921.	*Author and Title of Paper.*
10 Mar.	Prof. E. WILSON: "Magnetic Susceptibility of Low Order" (Lecture III: Practical Applications).
17 Mar.	Sir W. NOBLE: "The Long-distance Telephone System of the United Kingdom."
7 April	K. BAUMANN: "Some Recent Developments in Large Steam Turbine Practice."
14 April	E. A. WATSON: "Magnetos for Ignition Purposes in Internal Combustion Engines."
28 April	Discussion on "Tariffs" (Papers by J. R. BLAIKIE: "Electric Supply: Present Conditions and the Hopkinson Principles"; and J. W. BEAUCHAMP: "Multi-Part Tariffs for Domestic Electricity Supply").
5 May	Continuation of discussion on "Tariffs."

MEETING OF THE WIRELESS SECTION

(*Wednesday.*)

[To be held at the Institution of Mechanical Engineers, Storey's Gate, Westminster, S.W., at 6 p.m. (light refreshments 5 30 p.m.).]

16 Mar.	Dr. W. H. ECCLES and Miss W. A. LEYSHON: "Some Thermionic Tube Circuits for Relaying and Measuring." G. STEAD, M.A.: "The Effect of Electron Emission on the Temperature of the Filament and Anode of a Thermionic Valve."

INFORMAL MEETINGS.

(*Mondays.*)

[To be held in the Lecture-hall of the Chartered Institute of Patent Agents, Staple Inn Buildings (South Block), W.C., at 7 p.m.]

Date. 1921.	*Author and Title of Paper.*
14 Mar.	Discussion on "Rectifiers" (opened by Mr. R. L. MORRISON).
11 April	Discussion on "Some Characteristics and Applications of Multi-speed A.C. Motors" (opened by Mr. F. CREEDY).
25 April	Discussion on "Engineering in Russia" (opened by Mr. C. L. LIPMAN).

MEETINGS OF TERRITORIAL CENTRES AND SUB-CENTRES.

Date. 1921.	*Place of Meeting.*	*Author and Title of Paper.*
1 Mar.	Loughborough	G. A. PEARSON: "Phase Advancers, Rotary and Static."
3 Mar.	Middlesbrough	W. H. DUNCAN: "Some Notes on E.H.T. Cable Practice."
7 Mar.	Cardiff	W. KERR: "The Prospects of Electricity in Agricultural Centres" (Lecture).
8 Mar.	Edinburgh	Sir W. NOBLE: "The Long-distance Telephone System of the United Kingdom."
8 Mar.	Leeds	W. M. MILES: "Modern Feed-water Treatment."
8 Mar.	Manchester	A. B. MALLINSON: "Electric Driving in the Paper Mill, on Heat Economy Lines."
14 Mar.	Newcastle	Discussion on "The Board of Trade Regulations for Electricity Supply."
16 Mar.	Birmingham	Lecture by Prof. F. W. BURSTALL, M.A., M.Sc.
21 Mar.	Liverpool	A. B. MALLINSON: "Electric Driving in the Paper Mill, on Heat Economy Lines."
22 Mar.	Manchester	Sir W. NOBLE: "The Long-distance Telephone System of the United Kingdom."
30 Mar.	Sheffield	H. J. ROUND: "Direction and Position Finding."
31 Mar.	Middlesbrough	W. G. BASS: "The Conservation of Heat in the Power Station."
4 April	Swansea	S. P. SMITH: "The Commercial Application of Variable-speed Motors" (Lecture).
6 April	Birmingham	J. D. MORGAN: "Some Accessory Uses of Electricity on Motor Vehicles" (Informal Address).
6 April	Manchester	Prof. E. W. MARCHANT: "Modern Developments in Wireless Telegraphy and Telephony" (Lecture).
7 April	Aberdeen	Annual General Meeting of Aberdeen Sub-Centre.
11 April	Liverpool	Sir W. NOBLE: "The Long-distance Telephone System of the United Kingdom."
11 April	Newcastle	T. CARTER: "Some Notes on Power Factor Correction" (Informal Address).
12 April	Glasgow	C. W. MARSHALL: "Continuity of Supply" (Informal Address). Annual General Meeting of Scottish Centre.
12 April	Leeds	F. CREEDY: "Some Developments in Multi-speed Cascade Induction Motors."

Date. 1921.	*Place of Meeting.*	*Author and Title of Paper.*
12 April	Lough-borough	H. COTTON, M.B.E., B.Sc.: "Electric Drives for Rolling Mills and Colliery Winders."
12 April	Manchester	K. BAUMANN: "Some Recent Developments in Large Steam Turbine Practice." Annual General Meeting of North-Western Centre.
14 April	Middles-brough	C. T. S. ARNETT: "Breakdowns and Possible Remedies of Steel-works Electrical Gear."
19 April	Manchester	Annual Smoking Concert of North-Western Centre.
25 April	Liverpool	Annual General Meeting of Liverpool Sub-Centre.
25 April	Newcastle	E. GRIFFITHS, D.Sc., and F. H. SCHOFIELD, B.A., B.Sc.: "Some Thermal Characteristics of Electric Ovens and Hot-plates."
26 April	Leeds	Annual General Meeting of North Midland Centre.
27 April	Birmingham	E. A. WATSON: "Magnetos for Ignition Purposes in Internal Combustion Engines." Annual General Meeting of South Midland Centre.
27 April	Sheffield	F. SMITH: "Powdered Fuel."
29 April	Dundee	E. GRIFFITHS, D.Sc. and F. H. SCHOFIELD, B.A., B.Sc.: "Some Thermal Characteristics of Electric Ovens and Hot-plates."
2 May	Bristol	Address by the President, Mr. Ll. B. ATKINSON.
9 May	Newcastle	K. BAUMANN: "Some Recent Developments in Large Steam Turbine Practice." Annual General Meeting of North-Eastern Centre.
13 May	Middles-brough	Annual General Meeting of Tees-side Sub-Centre.
28 May	Rotherham	E. CROSS: "Present-day Commercial Problems in Electricity Supply."

LONDON STUDENTS' SECTION.

MEETINGS.

Date. 1921.	*Author and Title of Paper.*
4 Mar.	A. ROSEN, B.Sc.: "Telephonic Transmission through Submarine Cables." (*At Faraday House, Southampton Row, W.C.*, 6.30 *p.m.*)
11 Mar.	J. A. BROUGHALL, B.Sc.: "Some Recent Developments in Converting Machinery for Small Substations." (*At King's College, Strand, W.C.*, 6.30 *p.m.*)
15 April	F. H. JOHNSON: "Modern Electric Battery Vehicles." (*At Finsbury Technical College*, 6.30 *p.m.*)
22 April	R. C. HAWKINS: "Searchlights as used for Coast Defence Work." (*At Finsbury Technical College*, 6.30 *p.m.*)
29 April	A. C. WARREN: "Radio-Telegraphic Transmitting Apparatus." (*At the City and Guilds Engineering College, South Kensington*, 6.30 *p.m.*) Annual General Meeting.

VISITS TO WORKS.

Date. 1921.	*Works to be visited.*
11 Mar.	The Electrophone Exchange, Gerrard-street.
8 April.	"The Daily Sketch," Shoe-lane, E.C.
23 April.	Messrs. Marconi & Co., Ltd., Messrs. Crompton & Co., Ltd., and The Hoffman Manufacturing Co., Ltd. (All-day Visit to Chelmsford.)
13 May.	Messrs. Siemens Bros. & Co., Ltd., Woolwich.
27 or 28 May.	Messrs. Waygood-Otis, Ltd.

The above visits, for which tickets of admission are necessary, are open only to Students of the Institution, and those wishing to attend should notify Mr. R. C. Hawkins, 133 Park-lane, N. 16, at least 10 days before each visit. In some cases the number forming the party is limited by the authorities of the works to be visited, and on these occasions the Committee will decide by ballot or otherwise which Students may attend.

Council's Nominations for Election to the Council.

The following have been nominated by the Council for the vacancies which will occur in the offices of President, Vice-Presidents, Honorary Treasurer, and Ordinary Members of Council on the 30th September, 1921:—

President. (*One Vacancy.*)
J. S. HIGHFIELD.

Vice-Presidents. (*Two Vacancies.*)
Prof. E. W. Marchant, D.Sc. C. C. Paterson, O.B.E.

Honorary Treasurer. (*One Vacancy.*)
Sir James Devonshire, K.B.E.

Ordinary Members of Council.

MEMBERS. (*Four Vacancies.*)

S. Evershed. A. Page.
A. L. C. Fell, C.B.E. T. Roles.
H. E. O'Brien, D.S.O. Capt. H. J. Round, M.C.

ASSOCIATES. (*Two Vacancies.*)

Sir Andrew M. Ogilvie, K.B.E., C.B. A. W. Tait, C.B.E.

Electrical Research.

The Secretary of the Institution has received from the British Electrical and Allied Industries Research Association the following:

QUARTERLY PROGRESS REPORT BY THE DIRECTOR OF RESEARCH (MARCH 1921).

JOINT COMMITTEE ON DIELECTRIC LOSSES.

Mr. H. Tinsley has agreed to make up sample condensers for the Committee for use in proving the proposed methods of testing, and as standards of reference in checking results obtained by different investigators,

The Council has approved the development of a comprehensive programme of research at the National Physical Laboratory, and the programme is being worked out by the Committee in co-operation with the Laboratory.

Section A: Fibrous Insulating Materials.

Fabrics, untreated and treated.—Arrangements are being made for testing a number of samples to hand of cotton fabrics dressed and undressed, of various weights and treated with varying thickness of varnish, to ascertain the effect of the dressing and of the presence of varying amounts of cotton fibre.

The varnishing of fabrics woven for the Committee is deferred until the results of these preliminary tests are to hand.

Meanwhile, a collection is being made of samples of treated fabric now in use in the industry, with a view to classification and to ensuring that the tests about to be made will cover the ground.

Papers, untreated and treated.—Arrangements have now been made to carry out the complete series of proposed purchasing tests on samples of untreated papers now known to be in use in the industry for various purposes. Some reports are already to hand. The Committee has discussed the difficulties met with in testing for impurities, such as metallic inclusions and carbon obtained from soot in the atmosphere, and has arranged further experimental work with a view to improving the specification. The improvement of porosity tests is also receiving attention.

A large number of samples of treated papers are now to hand, and the assistance of the manufacturers is being sought in the classification of these.

Fibres and boards.—The numerous materials in use have been classified according to make and according to use, with the exception of vulcanized fibres, the various grades of which have not yet been distinguished. A preliminary schedule of tests has been prepared, providing for acceptance tests for purchasing purposes, and research tests for demonstrating the characteristics of individual materials.

The Committee is now giving attention to drafting clauses for methods of testing.

Section B: Composite Insulating Materials.

General research.—The Committee is awaiting completion of experimental work now in hand, on the results of which it is expected that the general clauses for mechanical and electrical tests for acceptance purposes can be completed. Tests for fire-resisting materials have yet to be dealt with.

Tooling tests.—The reports on the latest series of tooling tests on ebonite have now been received and discussed, and arrangements are being made to test the proposed clauses on a variety of materials with a view to ensuring that the methods are satisfactory and to obtaining limits for specification.

Section C: Porcelain.

A valuable report has been received from Messrs. Langton and Harris on preliminary researches on dielectric strength, mechanical strength, ability to endure sudden change of temperature, hardness, specific gravity, thermal conductivity and moisture absorption, forming the basis on which further work of the Committee has been done.

The Committee is now detailing programmes of research dealing with insulation resistance, improvement of surface, dielectric strength, and uniformity of vitrification.

Section D: Mica.

Mica and micanite for commutators.—The data obtained in response to a questionnaire extensively circulated has now been classified and considered by the Committee. It is found that the views of users are matters of opinion, not supported by sufficient evidence to enable the Committee to make decisions. Questions on the relative merits of mica and micanite have been referred back for further evidence, after the receipt of which it is expected that a preliminary report will be drafted. Information to hand bearing on commutation troubles not directly connected with the use of mica and micanite has been tabulated and is being forwarded to the B.E.A.M.A. for consideration by manufacturers of commutating machinery.

The Committee is considering research on abrasive hardness, and on the manufacture of micanite.

Mica for condensers.—Information furnished by manufacturers bearing on troubles met with in the manufacture of condensers is being tabulated and will be considered by the Committee shortly.

Section E: Insulating Oils.

Preliminary specifications.—The final draft of the preliminary purchasing specification for insulating oils for transformers, oil switches and breakers, is being reproduced for submission to the British Engineering Standards Association after approval by the Council.

Researches in hand.—The N.P.L. has undertaken researches on vapour pressures, thermal transference coefficients and thermal conductivity.

Researches are being arranged to deal with specific heats at high temperatures, latent heats, boiling points, effect of numerous catalysers on sludging, such as metals commonly in contact with the oil and dryers used in paints in contact with oil, the effect of the electric arc on the oil, the carbon deposit and sludge produced, and qualitative examination of sludge produced in transformers and oil switches.

Section F: Conductors.

Thermal characteristics of the soil.—The reports of the Meteorological Office have been studied and compared with the observations made by the investigators. This will enable the Committee to draw general conclusions of wide application.

Moisture content of the soil.—Arrangements have been made for obtaining data on the normal moisture content from a number of additional districts at home and in the Colonies. The Committee is now engaged in planning research to ascertain the effect of the heat of the cable on the moisture content under the various conditions which obtain in practice.

Three-core cables.—The mathematical formulæ developed are being checked experimentally over a range

wide enough to avoid the necessity of making assumptions.

Dielectric losses.—The Committee has considered in detail the methods to be employed in measuring the dielectric losses, and the experimental work is progressing.

Effects of grouping and spacing of cables.—The Committee is preparing a programme for this part of the research.

Overhead line wires.—The material for test has now been delivered to the N.P.L. and work commenced. A report should be available shortly.

Section G: Electric Control Apparatus.

The phenomena of switching and arcing.—The Report on this subject is being issued in sections, some of which will be available for distribution within a few weeks. Information is being collected from power station engineers and others. A number of engineers are actively co-operating, and a quantity of data has already been furnished bearing on the problems under investigation. Steps are being taken towards testing existing designs of switchgear under controlled conditions to obtain more precise information on rupturing capacity and on various phenomena accompanying the breaking of the circuit.

Mining switchgear.—A Committee has been called to approve a report on the Home Office tests made at Eskmeals on samples of mining switchgear furnished by manufacturers, and to prepare a programme for further research. The Home Office has consented to continue co-operation in this important work.

Heavy-duty fusible cut-outs.—A report is in preparation on published information bearing upon the design and operation of heavy-duty fusible cut-outs and matters incidental thereto. A Committee has been called to arrange a programme for further inquiry to be followed by experimental research.

Direct-current circuit breakers.—A strong Committee has been formed to collect information on the rupturing capacity of direct-current circuit breakers and the operation of attachments used with them, with a view more especially to the completion and improvement of purchasing specifications.

Conductivity of joints and contacts.—A Committee has been called to consider a programme of research on the conductivity of joints, more especially bolted, clamped and screwed joints as used in heavy conductors and busbars.

Section H: Corrosion of Condenser Tubes.

A Committee is being formed to watch the interests of members in this research, which is being conducted by the Institute of Metals with the assistance of other bodies. A substantial contribution to the funds has been made by this Association.

Informal Meetings.

The following Informal Meetings have been held:

16th Informal Meeting (17th January, 1921).

Chairman.—Mr. W. E. Warrilow.

Subject of Discussion.—"The Mental Equipment of an Engineer" (introduced by Mr. P. Pitt).

Speakers.—Messrs. H. W. Couzens, H. Brown, A. F. N. Chandler, R. W. Hughman, W. E. Bradshaw, A. W. Blake, C. L. Lipman, M. D. Hart, J. R. Bedford, W. F. Andrews, W. N. C. Clinch, R. Grierson, W. E. Rogers, W. E. Warrilow, and F. Pooley.

17th Informal Meeting (24th January, 1921).

Chairman.—Mr. F. Pooley.

Subject of Discussion.—"On Improving the Usefulness of the Institution."

Speakers.—Messrs. F. H. Masters, G. L. Addenbrooke, M. L. Jockel, H. L. Leach, E. W. Moss, K. Edgcumbe, W. Bevis, A. Wright, C. A. Newell, G. Layton, W. E. Highfield, J. R. Bedford, R. W. Hughman, A. G. Whyte, J. F. Avila, M. D. Hart, W. L. Wreford, P. Dunsheath, H. Brazil, H. Brown, M. Whitgift, W. E. Warrilow, and A. F. Harmer.

18th Informal Meeting (31st January, 1921).

Chairman.—Mr. E. W. Moss.

Subject of Discussion.—"Possible Economies in House Wiring" (introduced by Mr. L. Milne).

Speakers.—Messrs. A. F. Harmer, G. C. Allingham, W. Bishop, J. D. Spark, J. R. Bedford, W. E. Bradshaw, A. G. Hilling, J. R. Bowden, E. H. Freeman, W. L. Wreford, A. E. Gott, W. E. Warrilow, A. F. N. Chandler, P. Dunsheath, M. D. Dale, R. Grierson, and E. W. Moss.

19th Informal Meeting (14th February, 1921).

Chairman.—Mr. A. F. Harmer.

Subject of Discussion.—"The Design of a Large Power Station" (introduced by Mr. J. R. Cowie).

Speakers.—Dr. R. D. Gifford, Messrs. S. Utting, W. N. C. Clinch, R. O. Kapp, E. F. Hetherington, W. J. Oswald, H. B. Jenkins, L. M. Jockel, W. Dargavel, A. E. Gott, K. Edgcumbe, R. Grierson, A. B. Eason, and A. F. Harmer.

Accessions to the Reference Library.

Kemp, P. Rudiments of electrical engineering. sm. 8vo. 263 pp. *London*, 1920

Lancashire & Cheshire Coal Research Association, The. Bulletin 4, 5, 7. sm. 8vo. [*Manchester*], 1920

4, Notes on coal analysis, by F. S. Sinnatt.
5, Coal dust and fusain, by F. S. Sinnatt, H. Stern, and F. Bayley.
7, The determination of the carbon dioxide in coal, by F. S. Sinnatt and W. Harrison.

Marquand, H. S. Electric welding: its theory, practice, application and economics. 8vo. 204 pp. *London*, 1920

Meares, J. W. Report on the Laxapana–Aberdeen Hydro-Electric Project, Central Province, Ceylon. fol. 19 pp. *Calcutta*, 1920

Mitsuda, R. Economical design of a transmission line. [Japan, Dept. of Communications, Researches of the Electrotechnical Laboratory, no. 79]. 8vo. 143 pp. *Tokyo*, 1919

Parr, S. W. Effects of storage upon the properties of coal. [University of Illinois, Engineering Experiment Station, Bulletin no. 97]. 8vo. 44 pp. *Urbana, Ill.*, 1917

Poole, J. Telegraphy, telephony and wireless. sm. 8vo. 127 pp. *London* [1921]

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 25. MARCH 1921. INSTITUTION NOTES. [ISSUED WITH No. 299 OF THE JOURNAL.]

Arrangements for May 1921.

ORDINARY MEETINGS OF THE INSTITUTION.

[To be held at the **Institution of Civil Engineers,** Great George-street, Westminster, S.W., except where otherwise stated, at 6 p.m. (light refreshments 5.30 p.m.).]

Date. 1921.	*Author and Title of Paper.*
5 May	Continuation of discussion on "Tariffs."
26 May	A. JOHNSEN and K. RAHBEK: "A Physical Phenomenon and Its Application to Telegraphy, Telephony, etc." (Lecture).
31 May at 6.30 p.m.	Dr. F. B. JEWETT: "Research Work in the United States" (Lecture).

ANNUAL GENERAL MEETING.

(*Corporate Members and Associates only.*)

To be held on Tuesday, 31st May, 1921, at 5 p.m. (light refreshments 4.30 p.m.), at the **Institution of Civil Engineers,** Great George-street, Westminster, S.W., to receive and consider the Annual Report of the Council and the Accounts for the year ended 31st December, 1920, and to elect Auditors. The Report and Accounts will be found on pages 343–358 of the present Number of the *Journal* (No. 299).

SPECIAL GENERAL MEETING.

(*Corporate Members and Associates only.*)

To be held on Tuesday, 31st May, 1921, at 5.45 p.m., at the **Institution of Civil Engineers,** Great George-street, Westminster, S.W., for the purpose of considering and, if thought fit, of passing the following Resolution:—"That the Council of the Institution of Electrical Engineers be requested to apply to the Privy Council for a Royal Charter of Incorporation and that the draft Petition and draft Charter therefor which have been initialled for purposes of identification by the Chairman of this meeting be approved subject to such alterations as the Council of the Institution may deem it necessary or expedient to make therein prior to the application to the Privy Council, and to such alterations as may be made therein by the Privy Council and approved by the Council of the Institution."

MEETING OF THE WIRELESS SECTION.

(*Wednesday.*)

[To be held at the **Institution of Mechanical Engineers,** Storey's Gate, Westminster, S.W., at 6 p.m. (light refreshments 5.30 p.m.).]

25 May Discussion on "Long-distance Wireless Transmission" (introduced by Mr. C. F. ELWELL).

MEETINGS OF TERRITORIAL CENTRES AND SUB-CENTRES.

Date. 1921.	*Place of Meeting.*	*Author and Title of Paper.*
2 May	Bristol	Address by the President, Mr. Ll. B. ATKINSON.
9 May	Newcastle	K. BAUMANN: "Some Recent Developments in Large Steam Turbine Practice." Annual General Meeting of North-Eastern Centre.
10 May	Loughborough	Annual General Meeting of East Midland Sub-Centre.
12 May	Middlesbrough	H. G. WRIGHT: "Diesel Engines." Annual General Meeting of Tees-side Sub-Centre.
28 May	Rotherham	E. Cross: "Present-day Commercial Problems in Electricity Supply."

LONDON STUDENTS' SECTION.

VISITS TO WORKS.

Date. 1921.	*Works to be Visited.*
13 May	Messrs. Siemens Bros. & Co., Ltd., Woolwich.
27 May	Messrs. Waygood-Otis, Ltd.

The above visits, for which tickets of admission are necessary, are open only to Students of the Institution, and those wishing to attend should notify Mr. R. C. Hawkins, 133, Park-lane, N. 16, at least 10 days before each visit. In some cases the number forming the party is limited by the authorities of the works to be visited, and on such occasions the Committee will decide by ballot or otherwise which Students may attend.

Nominations for Election to the Council.

In addition to those members nominated by the Council (see *Institution Notes*, No. 24, page 14) the following have been nominated for the vacancies as Ordinary Members of Council which will occur on the 30th September next:

LIEUT.-COL. F. A. CORTEZ LEIGH, T.D., R.E.: (*Nominated by* Messrs. O. H. Baldwin, S. W. Baynes, J. H. Bowden, G. Broughall, R. H. Houghton, C. Jones, H. W. Jones, P. V. McMahon, E. T. Ruthven Murray, L. L. Murray, C. W. Neele, P. J. Pybus, C.B.E., P. Rosling, A. M. Sillar, W. J. Thorrowgood, and A. H. Walton.)

W. C. P. TAPPER: (*Nominated by* Messrs. A. W. Blake, F. A. Bond, E. Calvert, A. C. Cramb, F. W. Purse, L. L. Robinson, C. N. Russell, A. H. Shaw, C. N. Staniland, and W. C. Ullman.)

Informal Meetings.

The following Informal Meetings of the Institution have been held at the Chartered Institute of Patent Agents, Holborn, W.C.:

20TH INFORMAL MEETING (14TH MARCH, 1921).

Chairman: Mr. A. B. Eason.

Subject of Discussion: "Large Mercury Arc Rectifiers" (introduced by Mr. R. L. Morrison).

Speakers: Messrs. G. A. Juhlin, C. A. Newell, E. W. Moss, W. E. Rogers, A. G. Hilling, E. L. Webb, W. H. J. Holloway, J. Aylmer, P Kemp, W. L. Wreford, M. R. Gardner, A. B. Eason, R. Grierson, W. E. Bradshaw, F. Pooley, G. A. Cheetham, H. M. Sayers, and H. Cooch.

21ST INFORMAL MEETING (11TH APRIL, 1921).

Chairman: Mr. W. E. Bradshaw.

Subject of Discussion: "Some Characteristics and Applications of Multi-speed A.C. Motors" (introduced by Mr. F. Creedy).

Speakers: Messrs. W. E. M. Ayres, J. R. Smith, W. L. Wreford, G. J. D. Scott, B. P. Walker, and H. Cooch.

22ND INFORMAL MEETING (25TH APRIL, 1921).

Chairman: Mr. J. F. Avila.

Subject of Discussion: "Engineering in Russia" (introduced by Mr. C. L. Lipman).

Speakers: Messrs. H. E. Crowcroft, A. H. Allen, W. G. Laird, W. L. Wreford, W. E. Rogers, E. L. Webb, F. Pooley, B. B. Walker, A. B. Eason, M. Whitgift, G. J. D. Scott, and R. Grierson.

Benevolent Fund.

In No. 22 (December 1920) of *Institution Notes* it was announced that Mr. E. M. Hughman, of Bombay, had offered a donation of £250 to the Benevolent Fund on condition that 19 similar donations were received by the 31st March, 1921. The date of expiry of the offer has been extended to the 30th June, 1921, and the Committee charged with raising the fund have obtained Mr. Hughman's consent to the sums of £250 not being contributed solely by single donors, but by group subscriptions. The Territorial Centres of the Institution are taking steps to organize such group collections, which it is hoped will be supported by members of the Institution of all classes. All communications should be addressed to the Secretary of the Institution.

Donations to the Fund from the following were reported by the President at the Ordinary Meeting held on the 7th April, 1921:

J.A., E. Abbey, D. Adams, H. Alabaster, C. W. Allen, H. J. Allinson, E. J. Allman, G. Allom, R. Anderson, W. H. Armstrong, H. S. Austin, J. F. Avila, H. J. Aylott, C. J. Baker, Col. W. Baker-Brown, C.B., R.E., P. E. Bamford, E. Barlow, A. B. Barnacle, A. S. Barnes, H. A. Bastable, C. O. Bastian, E. J. Baxter, J. S. Bean, A. L. Bedford, S. Beeton, E. G. Bell, J. F. M. Bennett, P. L. Bernstein, W. Bird, R. W. Blades, N. Blazey, R. F. P. Blennerhassett, C. A. Boraston, F. J. Brown, E. Braathen, J. T. Bramwell, E. E. Briggs, S. E. Britton, E. W. Broadbent, J. E. Bromley, R. P. Brousson, J. S. Brown, C. F. Browne, W. H. Brownjohn, G. L. Bruce, J. S. Buckley, H. C. Bullman, W. C. Burbridge, W. E. Burnand, R. B. Burrowes, W. Burton, J. M. Butcher, J. E. Calverley, R. H. Campion, F. W. Capper, R. S. Carlisle, S. Carlisle, N. E. Carnaby, G. W. Carpenter, J. H. Carrick, A. F. Carter, F. S. Carter, H. J. Cash, H. C. Channon, C. E. Charman, C. B. Chartres, R. A. Chattock, L. F. Christy, B. C. Clayton, Prof. W. C. Clinton, H. W. Clothier, F. G. Cole, A. D. Constable, A. Cooper, C. W. Cooper, G. L. Covernton, J. R. Cowie, A. W. Cox, C. H. F. Cox, H. E. Cox, The Hon. E. Cozens-Hardy, J. F. Dale, J. C. J. Dalton, E. C. Dandy, Lieut.-Col. A. E. Davidson, D.S.O., B. Davies, F. E. Davies, J. Davies, W. J. F. Debley, H. Y. Denham, Sir A. Denny, Bart., Sir J. Devonshire, K.B.E., T. J. Digby, E. T. G. Donovan, H. M. Dowsett, B. M. Drake, C. V. Drysdale, O.B.E., D.Sc., H. M. Dudgeon, W. H. Duncan, P. C. Ebner, Dr. W. H. Eccles, Major K. Edgcumbe, R.E., F. S. Edwards, H. J. Eley, T. K. Evans, E. Fawsett, S. E. Fedden, L. G. Fenner, J. R. B. Ferguson, J. H. Field, A. J. Fippard, E. G. Fleming, H. S. Fox, A. E. Frankling, E. Garcke, F. H. Gardiner, W. H. Gatley, H. P. Gaze, K. S. Geipel, O.B.E., F. W. Geoghegan, V. W. Gill, Sir R. T. Glazebrook, K.C.B., D.Sc., F.R.S., G. D. Glegg, P. Good, G. F. C. Gordon, H. B. Gough, J. Hunter Gray, K.C., C. F. Green, Horace Green, W. G. Griffith, L. Griffiths, R. Grigor, F. E. Gripper, C. Grover, C. W. Gwyther, W. Hall, A. S. Hampton, R. Hardy, N. E. P. Harris, Haydn T. Harrison, T. Martin Harvey, J. H. Havelock, F. N. Haward, C. C. Hawkins, W. C. C. Hawtayne, W. J. Head, R. Heath, H. J. Henwood, J. F. Herd, G. E. Hider, J. S. Highfield, R. Hills, L. B. Hobgen, J. P. Hodges, W. A. Hole, C. W. Holman, H. C. Holroyd, W. F. M. Horne, A. J. Howard, H. K. Hunter, C. G. Huntley, E. W. James, W. L. James, B. C. Jennings, H. H. Johnson, J. H. Johnson, C. Jones, Dr. G. Kapp, A. C. Kelly, J. C. Kelso, W. T. Kerr, C. D. King, H. Kingsbury, J. E. Kingsbury, J. R. Kingston, G. C. Knox, G. Langdon, W. H. Lea, L. J. Leng, L. J. Lepine, A. E. Levin, E. W. Lewis, W. T. Maccall, C. N. McDermott, Prof. J. T. MacGregor-Morris, H. T. McKinnon, D. A. McLean, H. McNaul, A. C. McWhirter, F. W. Main, V. S. Manyam, W. Marden, F. Marks, J. Martin, T. D. Martin, J. W. Meares, S. W. Melsom, J. Meredith, E. H. Miller, L. B. Miller, T. H. Minshall, D.S.O., W. J. Minton, W. M. Mordey, W. C. & K. A. Mountain, S. Neville, C. G. M. New, A. V. Newell, A. S. Newman, J. F. Nielson, A. M. Niven, C. G. Nobbs, R. J. Nunn, Sir A. M. Ogilvie, K.B.E., C.B., C. Oliver, V. F. M. Oliver, Col. W. A. J. O'Meara, C.M.G., W. Orrell, F. L. Otter, A. Page, W. G. Palmer, E. A. Paris, L. E. Parker, D. Parkinson, E. Parry, The Hon. Sir C. A. Parsons, K.C.B., F.R.S., W. H. Patchell, W. Pate, Lieut.-Com. Q. H. Paterson, D.S.C., E. L. Payne,

F. G. Payne, D.S.O., L. Paysant, J. A. L. Pealing, E. M. Pearson, H. L. Percy, C. G. Phillips, J. W. Piggott, B. J. Piper, F. C. Platt, H. V. Pointon, F. C. Porte, E. B. Porter, A. W. Pougher, A. H. Preece, N. Prentice, A. J. Price, F. E. Price, S. C. Price, O. S. Puckle, A. P. Pyne, R. H. Rawll, T. B. Reader, A. Reame, J. P. Rees, J. Regis, J. Rendell, T. Rich, O.B.E., H. R. Rivers-Moore, J. A. Robertson, R. Robertson, B. A. Robinson, N. H. Robinson, E. E. Rogers, S. R. Roget, T. Roles, P. Rosling, G. Ross, G. L. Rosser, A. H. Rowles, J. Russell, S. A. Russell, W. V. Ryder, H. Sampson, F. Samuelson, H. R. Sanders, H. S. Saunders, H. M. Sayers, J. Sayers, O.B.E., A. W. Sclater, J. D. Scott, W. H. Scott, W. M. Selvey, S. Sharp, H C. Shepherd, G. E. Shires, A. Siemens, F. A. Simpson, M. G. Simpson, S. Simpson, H. A. Skelton, Sir W. Slingo, J. W. Slorach, A. J. Small, G. Smith, W. G. Smith, C. H. Smyth, Sir J. Snell, A. Sommerville, J. H. Southern, W. Spencer, A. L. Stanton, A. C. Stewart, C. Stewart, W. G. Stock, Rev. R. H. Streatfield, H. M. Stronach, A. J. Stubbs, H. S. Styan, H. W. Sullivan, W. E. Swale, W. H. T. Swire, J. H. Targett, E. E. Tasker, T. S. Thomas, N. A. Thompson, C. H. R. Thorn, R. Torikai, W. H. Tranmer, A. Travers, J. W. Turner, L. B. Tyler, C. J. Vaughan, O. V. Waddington, Prof. M. Walker, D.Sc., A. E. Wallis, A. S. Wallis, A. S. Wann, F. Wardrobe, C. E. Warner, A. G. Warren, E. C. Watson, O. C. Waygood, P. D. Webb, S. H. Webster, B. L. Wells, E. H. Wells, W. F. Whittaker, F. H. Williams, W. Wilson, F. H. Wise, K. L. Wood, W. B. Woodhouse, J. H. Woodward, J. H. Woolliscroft, C. H. Wordingham, C.B.E., H. G. Wright, H. E. Yerbury, and H. W. Young.

Accessions to the Lending Library.

BEAUMONT, W. W. Industrial electric vehicles and trucks. 8vo. 195 pp. *London*, 1920

BROUGHTON, H. H. The electric handling of materials. A manual on the design, construction and application of cranes, conveyors, hoists and elevators. Vol. 1, Electrical equipment. 4to. 215 pp. *London*, 1920

DUDLEY, A. M. Connecting induction motors. The practical application of a designing engineer's experience to the problems of operating engineers, armature winders and repair men. Also the presentation to students of practical questions arising in winding and connecting alternating current motors. 8vo. 263 pp. *New York*, 1921

GANDY, T. S., and SCHACHT, E. C. Direct-current motor and generator troubles: operation and repair. 8vo. 283 pp. *New York*, 1920

GARRARD, C. C. Electric switch and controlling gear: a handbook on the design, manufacture and use of switchgear and switchboards in central stations, factories and mines. 2nd ed. 8vo. 676 pp. *London*, 1920

GASTER, L., and DOW, J. S. Modern illuminants and illuminating engineering. 2nd ed. 8vo. 505 pp. *London*, [1919]

HAVELOCK, J. H. Electrical installation work (including "Electric wiremen's work"). 8vo. 374 pp. *London*, 1921

HERBERT, T. E. Telegraphy. A detailed exposition of the telegraph system of the British Post Office. 4th ed., with addendum. 8vo. 1039 pp. *London*, 1920

IBBETSON, W. W. S. Motor and dynamo control: theory and practice. A book for the motor and switchboard attendant, and all engineers who have the care and control of electrical machines under their supervision. 8vo. 495 pp. *London*, 1921

JAMES, H. D. Controllers for electric motors. 8vo. 368 pp. *London*, 1920

JOHNSON, V. E. Modern high-speed influence machines. Their principles, construction and applications to radiography, radiotelegraphy, spark photography, electro-culture, electro-therapeutics, high-tension gas ignition, the testing of materials, etc. 8vo. 285 pp. *London*, 1921

LEGGETT, B. Wireless telegraphy with special reference to the quenched-spark system. 8vo. 500 pp. *London*, 1921

PAGÉ, V. W. Modern starting, lighting and ignition. Principles, application, wiring diagrams, hints on repairs. 6th ed. 8vo. 815 pp. *London*, [1920]

PERCIVAL, G. A. The electric lamp industry. sm. 8vo. 133 pp. *London*, [1920]

RICKARD, T. A. Technical writing. 8vo. 183 pp. *New York*, 1920

RODENHAUSER, W., SCHOENAWA, J., and VOM BAUR, C. H. Electric furnaces in the iron and steel industry. Translated from the original by [C. H. Vom Baur] and now completely rewritten. 3rd. ed. 8vo. 481 pp. *New York*, 1920

ROWARTH, E. The engineering draughtsman. 8vo. 269 pp. *London*, [1919]

STILL, A. Principles of electrical design: d.c. and a.c. generators. 8vo. 379 pp. *New York*, 1916

SYLVESTER, C. Coil ignition for motor cars. A manual for the motor mechanic, owner-driver and all interested in coil ignition systems. sm. 8vo. 239 pp. *London*, 1921

TURNER, L. B. Wireless telegraphy and telephony. An outline for electrical engineers and others. 8vo. 205 pp. *Cambridge*, 1921

WADE, C. F. Efficient boiler management with notes on the operation of reheating furnaces. 8vo. 280 pp. *London*, 1919

WALL, T. F., D.Sc. Electrical engineering. 8vo. 502 pp. *London*, [1921]

YOUNG, A P., *O.B.E.* Magnetos: the theory and practice of magneto ignition for all forms of internal combustion engines. 2nd ed. sm. 8vo. 240 pp. *London*, [1920]

Accessions to the Reference Library.

ROBINSON, W. Everyday chemistry. sm. 8vo. 143 pp. *London*, [1920]

ROWARTH, E. The engineering draughtsman. 8vo. 269 pp. *London* [1919]

SANDICK, R. A. VAN. Bij het aftreden van D. Croll als president van het Kon. Instituut van Ingenieurs. 1916–1920. [Overgedrukt uit het weekblad "De Ingenieur," van 3 Jan., 1920, no. 1]. 8vo. 4 pp. *'s-Gravenhage*, 1919

Smithsonian Physical Tables. 7th revised ed. Prepared by F. E. Fowle. [Smithsonian Miscellaneous Collections, vol. 71, no. 1].
8vo. 496 pp. *Washington*, 1920

Spon, E., and F. N., Ltd. Workshop receipts for manufacturers and scientific amateurs.
new ed. 4 vol. 8vo. *London*, 1917

Stoek, H. H. The storage of bituminous coal. [University of Illinois, Engineering Experiment Station, Circular no. 6].
8vo. 192 pp. *Urbana, Ill.*, 1918

Swift, H. B. Practical electric welding.
8vo. 104 pp. *London*, 1919

Swinton, A. A. C. Wireless telegraphy and telephony. An address, Royal Society of Arts, 17th Nov., 1920. Reprinted from the Journal of the R.S.A. [vol. 69]. la. 8vo. 14 pp. *Hastings*, 1920

Switzerland. Département fédéral des postes et des chemins de fer. Législation suisse en matière d'installations électriques.
8vo. 189 pp. *Berne*, 1908

Sylvester, C. Armature winding. A practical handbook for students, armature winders, and engineers in charge. sm. 8vo. 184 pp. *London*, 1920

Taylor, W. T. Calculation of electric conductors.
la. 8vo. 33 pp. [*with* chart] *London*, 1919

United States : Dept. of Commerce. Bureau of Navigation. Radio communication Laws of the United States and the International Radiotelegraphic Convention. Regulations governing radio operators and the use of radio apparatus on ships and on land. Edition Aug. 15, 1919.
8vo. 102 pp. *Washington*, 1919

Wade, C. F. The fireman's handbook and guide to fuel economy. sm. 8vo. 84 pp. *London*, 1920

War Office. Signals Experimental Establishment. Pamphlet[s] no. 15–18. 8vo. *n.p.* 1919
15, Unit H.T., W.T., 100-watt.
16, Unit H.T., W.T., 50-watt.
17, W.T. sets, Field, 60-watt C.W.
18, Index of valves.

Warner, E. H., and Kunz, J. Corona discharge. [University of Illinois, Engineering Experiment Station, Bulletin no. 114].
8vo. 138 pp. *Urbana, Ill.*, 1919

Wells, H. M., and Southcombe, J. E. The theory and practice of lubrication : the " Germ " process. [Reprinted from the Journal of the Society of Chemical Industry, vol. 39, no. 5, March 15, 1920].
sm. 8vo. 37 pp. *London*, 1920

Yensen, T. D. The effect of boron upon the magnetic and other properties of electrolytic iron melted in vacuo. [University of Illinois, Engineering Experiment Station, Bulletin no. 77].
8vo. 19 pp. *Urbana, Ill.*, 1915

—— Magnetic and other properties of electrolytic iron melted in vacuo. [University of Illinois, Engineering Experiment Station, Bulletin no. 72].
8vo. 71 pp. *Urbana, Ill.*, 1914

—— Magnetic and other properties of iron-silicon alloys, melted in vacuo. [University of Illinois, Engineering Experiment Station, Bulletin no. 83].
8vo. 67 pp. *Urbana, Ill.*, 1915

—— and Gatward, W. A. Magnetic and other properties of iron-aluminium alloys melted in vacuo. [University of Illinois, Engineering Experiment Station, Bulletin no. 95].
8vo. 54 pp. *Urbana, Ill.*, 1917

Young, A. P., *O.B.E.* The elements of electro-technics.
sm. 8vo. 356 pp. *London*, 1920

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 26. MAY 1921. INSTITUTION NOTES. [ISSUED WITH No. 301 OF THE JOURNAL.]

Council for the Year 1921–22.

At the Ordinary Meeting on the 26th May, 1921, the result of the ballot to fill the vacancies which will occur in the Council on the 30th September next was reported as follows:—

President: Mr. J. S. Highfield.

Vice-Presidents: Professor E. W. Marchant, D.Sc., and Mr. C. C. Paterson, O.B.E.

Honorary Treasurer: Sir James Devonshire, K.B.E.

Ordinary Members of Council: Mr. S. Evershed, Lieut.-Colonel F. A. Cortez Leigh, T.D., R.E., Sir A. M. Ogilvie, K.B.E., C.B., Mr. A. Page, Mr. T. Roles, and Mr. A. W. Tait, C.B.E.

The Council for the year 1921–22 will therefore be constituted as follows:—

President.

J. S. Highfield.

The Past-Presidents.

Vice-Presidents.

W. H. Eccles, D.Sc., F.R.S.
Prof. E. W. Marchant, D.Sc.
C. C. Paterson, O.B.E.
A. A. C. Swinton, F.R.S.

Honorary Treasurer.

Sir James Devonshire, K.B.E.

Ordinary Members of Council.

H. J. Cash.
J. R. Cowie.
S. Evershed.
S. E. Fedden.
E. A. Gatehouse.
F. Gill, O.B.E.
P. V. Hunter, C.B.E.
Lt.-Col. F. A. Cortez Leigh, T.D., R.E.
A. H. W. Marshall.
Sir W. Noble.
Sir A. M. Ogilvie, K.B.E., C.B.
A. Page.
T. Roles.
A. Russell, D.Sc.
A. W. Tait, C.B.E.
P. D. Tuckett.
C. Vernier.
B. Welbourn,

and

The Chairman and Immediate Past-Chairman of each Territorial Centre.

The Institution Premium.

The Council have decided that in future the Institution Premium (value £25) shall not be awarded more than once to the same person.

Deputation of American Engineers.

A deputation of American engineers visited England at the end of June to express their appreciation of the work done by British engineers in the war. The deputation, which consisted of

*Mr. Ambrose Swasey ..	Chairman of Deputation.
Mr. Charles T. Main ..	Past President, American Society of Mechanical Engineers.
Mr. Robert A. Cummings	Vice-President, American Society of Civil Engineers.
*Mr. John R. Freeman ..	Past Vice-President, American Society of Civil Engineers.
Dr. Ira N. Hollis ..	Past President, American Society of Mechanical Engineers.
Mr. Jesse M. Smith ..	Past President, American Society of Mechanical Engineers.
Colonel Arthur S. Dwight	Vice-President, American Institute of Mining and Metallurgical Engineers.
Mr. Charles F. Rand ..	Past President, American Institute of Mining and Metallurgical Engineers.
Mr. William Kelly ..	Past Director, American Institute of Mining and Metallurgical Engineers.
Dr. F. B. Jewett ..	Past Vice-President, American Institute of Electrical Engineers,

were entertained by the Council at dinner at the Royal Palace Hotel, Kensington, on the 30th June, and also attended the Institution Conversazione the same evening. At 10 p.m. Mr. Swasey addressed those present, and he was followed by Dr. Jewett, who, in the course of his remarks, read the following letter from the American I.E.E.:

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.
33, West Thirty-ninth Street,
New York.
24 May, 1921.

DEAR DR. JEWETT,

I am advised that on June 30th the deputation of American engineers visiting Great Britain will be the guests of the Council of the British Institution of Electrical Engineers, and it has seemed to me that advantage should be taken of so notable an occasion

* Also representing the American Society of Mechanical Engineers.

to emphasize the relation of mutual trust and understanding which exists and must continue to exist between the great English-speaking nations.

From my knowledge of the membership of the American Institute of Electrical Engineers, I am confident that I am voicing only their conviction when I state that this relation is the foundation upon which the structure of world peace and progress must be built. Departure from it would be a world calamity. There may be differences of opinion, but these are but the surface ripples of a wind blowing against the stream. The main current flows on.

Will you, on the occasion in question, express to our confrères in Great Britain the greetings of the Institute; make clear our recognition of the absolute need of the maintenance and promotion of understanding and co-operation; and convey our firm purpose to do what we may toward their advancement?

Very truly yours,
(Signed) A. W. BERRESFORD,
President.

The speeches were suitably acknowledged by the President, Mr. Ll. B. Atkinson.

Premiums.

At the Annual General Meeting on the 31st May the President announced that the following premiums for papers as indicated have been awarded by the Council:—

The Institution Premium (*value* £25).
E. A. WATSON, O.B.E. "Magnetos for Ignition Purposes in Internal Combustion Engines."

The Ayrton Premium (*value* £10).
Sir PHILIP DAWSON. "Electric Railway Contact Systems."

The Fahie Premium (*value* £10).
Sir WILLIAM NOBLE. "The Long-Distance Telephone System of the United Kingdom."

The John Hopkinson Premium (*value* £10).
G. A. JUHLIN. "Temperature Limits of Large Alternators."

The Kelvin Premium (*value* £10).
Prof. E. WILSON. "Magnetic Susceptibility of Low Order" (3 Lectures).

The Paris Premium (*value* £10).
K. BAUMANN. "Some Recent Developments in Large Steam Turbine Practice."

The Webber Premium (*value* £10).
J. PALEY YORKE. "Some Recent Developments of High-Current Arcs, with special reference to Searchlights.

A Premium (*value* £10).
S. EVERSHED. "Permanent Magnets in Theory and Practice."

A Premium (*value* £10).
Jointly to
W. B. WOODHOUSE. "The Distribution of Electricity."
R. O. KAPP. "Some Economic Aspects of E.H.T. Distribution by Underground Cable."

A Premium (*value* £5).
A. E. MCCOLL. "Automatic Protective Devices for Alternating-current Systems."

A Premium (*value* £5).
G. E. BAIRSTO, D.Sc. "The Effect of Shunted Resistance, or Plug Leakage, on the Sparking Performance of an Electrical Ignition System."

A Premium (*value* £5).
M. B. FIELD, O.B.E. "Multiple-unit Shunts for the Measurement of Very Heavy Currents."

A Premium (*value* £5).
A. MCKINSTRY, B.A., M.Sc. "Notes on the Electrical Calculation of Long-Distance High-Voltage Transmission Lines."

WIRELESS SECTION PREMIUMS.
The Duddell Premium (*value* £20).
B. S. GOSSLING. "The Development of Thermionic Valves for Naval Uses."

A Premium (*value* £10).
Major R. C. TRENCH. "Range of Wireless Stations."

Wireless Section Committee, 1921–22.

At the meeting of the Wireless Section on the 25th May, the Chairman announced that the following members had been nominated to fill the vacancies which will occur on the Committee on the 30th September next:—

Chairman: Professor G. W. O. Howe, D.Sc.
Ordinary Members of Committee: Mr. B. Binyon, O.B.E., Dr. W. H. Eccles, F.R.S., Mr. G. H. Nash, C.B.E., and Mr. C. C. Paterson, O.B.E.

and that the following members would continue to serve:—

Sir Charles Bright, F.R.S.E., Mr. R. C. Clinker, Professor C. L. Fortescue, Mr. Andrew Gray, Admiral of the Fleet, Sir H. B. Jackson, G.C.B., Prof. E. W. Marchant, D.Sc., Captain H. J. Round, M.C., Mr. A. A. C. Swinton, F.R.S., and Mr. L. B. Turner.

No further nominations having been received, the above, together with the representatives of the Admiralty, the War Office, the Air Ministry and the Post Office, will constitute the Committee for the year 1921–22.

Army Reserve of Officers.

The Council of the Institution, having been requested by the Army Council to assist in obtaining members to join the Army Reserve of Officers as Officers for duty with the Royal Engineers and other technical branches of the Army in the event of emergency, desire to draw the attention of the members of the Institution who are ex-officers to this communication, with a view to such officers joining the Reserve. The conditions governing this Reserve are set out in Army Order 550/1920, a copy of which can be seen at the Institution Offices.

At the request of the Army Council, the Council also desire to bring to the notice of members the fact that there is at the present time an extreme shortage of tradesmen of all trades in the Army and that the number who appear to be willing to enlist is very small. Men who have already served are permitted to join Section D of the Army Reserve direct, but comparatively few have so far availed themselves of this opportunity, and it is thought that the possibility of doing so may not be understood.

Electrical Research.

The Secretary of the Institution has received from the British Electrical and Allied Industries Research Association the following:

QUARTERLY PROGRESS REPORT BY THE DIRECTOR OF RESEARCH (JUNE 1921).

Joint Committee on Dielectric Losses.

The work before this Committee has developed in extent and importance, and it has been decided to reorganize it under an independent section, which will deal with fundamental properties of insulating materials.

Attention is being given to thermal resistivity, there being considerable scope for experimental research on this subject.

Section A: Fibrous Insulating Materials.

Fabrics, untreated and treated.—Based on the preliminary tests made, an extensive programme is being developed in co-operation with manufacturers. There are still wide differences in practice in manufacture. These are under investigation.

At the same time the requirements of users of treated fabrics are being classified and studied in co-operation with manufacturers of the materials.

Papers, untreated and treated.—A large number of tests have now been made on untreated papers, and the results are being studied by the Committee with a view to standardizing the methods used and setting limits for purchasing.

The requirements of users of treated papers are being studied and classified.

Fibres and boards.—Specifications have been drafted for the principal materials in use, and tests are being made to prove the proposed clauses and find limits for purchasing. Classification has been carried further by a careful study of processes of manufacture and users' requirements.

Section B: Composite Insulating Materials.

The reports on work carried out during the last 12 months are now to hand, and will be studied by the Committee next month. Some difficulty has been experienced in dealing with materials which are subject to such large variations as are the moulded insulating materials in common use, but methods have been developed of bringing the results into perspective

Section C: Porcelain.

Programmes of research on insulation resistance, improvement of surface, dielectric strength, and uniformity of vitrification are still under consideration.

Section D: Mica.

Mica and micanite for commutators.—The Committee is now drafting a report on the data obtained bearing on the use of mica and micanite. The question of abrasive hardness is still under consideration.

Mica for condensers.—The Committee is preparing a report on this subject.

Section E: Insulating Oils.

Preliminary specification.—The proposed purchasing specification for insulating oils for transformers, oil switches and breakers is under consideration by the British Engineering Standards Association.

Researches in hand.—Good progress is being made on researches on thermal transference coefficients, vapour pressures, thermal conductivity and effect of catalysers.

Samples of oil subjected to arcing, under severe conditions in oil circuit-breakers, have been examined by the ultra-microscope, spectroscope and other methods, and conclusions reached which will be of assistance in developing the programme of research.

Section F: Conductors.

Heating of buried cables.—Reports on tests planned last quarter are now coming to hand and will be considered next month. Attention is being given also to recent work published elsewhere.

Overhead line wires.—Progress is being made with the special apparatus required for testing long lengths of conductors under working conditions, and test results are expected shortly.

Section G: Electric Control Apparatus.

Phenomena of switching and arcing.—In preparing a résumé of the large amount of data now collected, it has been found that most of the published conclusions require careful reconsideration, and new methods of attack are being developed as an outcome of the conclusions now reached.

Power station engineers are co-operating with the Committee and tests are being made on several large plants. The Newcastle Electric Supply Company is arranging to place plant at the disposal of the Association

in the autumn for short-circuit tests on existing designs of oil switches, for comparative performance tests and the study of phenomena.

Mining switchgear.—Tests are being arranged for the study of pressures set up by arcs in switch cases, whether normal or the result of faults, and of methods of relieving the pressure developed by such arcs and by gas explosions.

Heavy duty fusible cut-outs.—The Committee is obtaining samples of British Standard ordinary-duty fusible cut-outs for test and study, preliminary to arranging tests on heavy-duty fuses. Meanwhile a quantity of data on heavy-duty fuses is being placed at the disposal of the Committee for study.

D.C. circuit breakers.—The Committee is collecting data from available sources, bearing on service conditions, and inviting the co-operation of power plant engineers and others, in experimental work.

Resistivity of joints and contacts.—A comprehensive programme of research on the resistivity of joints in busbars, switchgear connections and the like has been prepared, and arrangements are in hand for collecting data from existing installations showing results obtained with present designs under service conditions. Temperature-rise is also receiving consideration.

Section H : Corrosion Researches.

The Committee has under consideration researches on corrosion phenomena, with special reference to condensers.

Section J : Turbine Researches.

This Committee has been established to deal with researches of interest to turbine manufacturers. It will co-operate with the Institution of Mechanical Engineers in research work being carried out at Manchester University on turbine nozzles. Researches on turbine blading are also in hand.

Section K : Synthetic Resins.

This Committee has been established to further the manufacture and use of insulating materials with a synthetic resin base and having the valuable heat-resisting and other properties possessed by certain of these materials.

The co-operation has been secured of manufacturers interested in the production of the raw materials and finished products. A Sub-committee has been established to give special consideration to the question of production in quantity in this country.

Accessions to the Reference Library.

American Institute of Electrical Engineers. Recommended practise for electrical installations on shipboard (Marine rules). Prepared by the Marine Committee of the A.I.E.E. Dec., 1920.
8vo. 97 pp. *New York*, [1921]

—— Standards of the A.I.E.E. 1921 revision.
8vo. 183 pp. *New York*, 1921

Beaumont, W. W. Industrial electric vehicles and trucks. 8vo. 195 pp. *London*, 1920

Broadbent, F. Dynamo & motor attendants and their machines. 10th ed.
sm. 8vo. 228 pp. *London*, [1921]

Denny, C. W. The electro-deposition of copper and its industrial applications.
sm. 8vo. 120 pp. *London*, 1921

Denton, F. M. Elementary principles of continuous-current armature winding.
sm. 8vo. 112 pp. *London*, 1921

Havelock, J. H. Electrical installation work (including "Electric wiremen's work").
8vo. 374 pp. *London*, 1921

Herbert, T. E., and de Wardt, R. G. The arithmetic of telegraphy and telephony.
sm. 8vo. 194 pp. *London*, [1921]

Ibbetson, W. S. Motor and dynamo control : theory and practice. A book for the motor and switchboard attendant, and all engineers who have the care and control of electrical machines under their supervision. 8vo. 495 pp. *London*, 1921

Lawson, A. J. Foreign trade tables : metric weights and measures and British equivalents, engineering data. 3rd (English-Italian) ed.
8vo. 579 pp. *London*, [1920]

Leggett, B. Wireless telegraphy, with special reference to the quenched-spark system.
8vo. 500 pp. *London*, 1921

Meares, J. W. Hydro-electric development. The determination of water-power possibilities, methods of utilization, design and construction of works.
sm. 8vo. 99 pp. *London*, [1920]

Petroleum Year Book, The. 1921. Ed. by S. H. North. 8vo. 252 pp. *London* [1921]

Poole, H. E. High tension switchboards. . . . Enumerating the leading points in design and describing the types of switchboard in most general use.
sm. 8vo. 123 pp. *London*, 1921

—— High tension switchgear. Describing the design, construction, and functions of the leading types of switchgear used in the control of high-tension electrical plant.
sm. 8vo. 127 pp. *London*, 1921

Royds, R. Heat transmission by radiation, conduction and convection.
8vo. 246 pp. *London*, 1921

—— Heat transmission in boilers, condensers, and evaporators. 8vo. 310 pp. *London*, 1921

—— The measurement of steady and fluctuating temperatures. 8vo. 173 pp. *London*, 1921

Starling, S. G. An introduction to technical electricity.
sm. 8vo. 193 pp. *London*, 1921

Trotter, A. P. The elements of illuminating engineering. sm. 8vo. 114 pp. *London*, 1921

Turner, L. B. Wireless telegraphy and telephony. An outline for electrical engineers and others.
8vo. 207 pp. *Cambridge*, 1921

Wall, T. F., *D.Sc.* Electrical engineering.
8vo. 502 pp. *London*, [1921]

White, H. G. Electric bells, alarms and signalling systems. 2nd ed.
sm. 8vo. 113 pp. *London*, [1921]

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 27. JUNE 1921. INSTITUTION NOTES. [ISSUED WITH No. 302 OF THE JOURNAL.]

Royal Charter.

H.M. the King, Patron of the Institution.

At a meeting of the Privy Council held at Buckingham Palace on Wednesday, 10th August, 1921, the petition of the Institution for a Royal Charter of Incorporation was approved, and a Royal Charter has now been granted.

His Majesty the King has also been graciously pleased to intimate his willingness to become Patron of the Institution.

Letter from the President.

TO MY FELLOW MEMBERS OF THE I.E.E.,

Although our newly elected President does not take up his duties until October next, the work of the present session now lies behind us and the present issue of the *Journal* is the last to be published during my year of office, and for this reason I take the opportunity of saying to the members of Council, and all others, a few words of thanks.

The session has not lacked in incident, or in work for the Council. The Annual Report of the Council has told the details to the membership, but the special tasks which have been accomplished are these: we have readjusted our finances to the new conditions of values; we have returned to our own home on the Embankment, just in time, to my own great gratification, to enable the Electricity Commissioners to hold the London Inquiry in the Institution building; we have obtained a Royal Charter of Incorporation; and in addition the King has graciously honoured the Institution by becoming its Patron. Now these four tasks have on the whole been accomplished so smoothly that, to those who have not access to the details, it may be thought that they were easy formalities. But everyone of them has been beset by initial difficulties and pitfalls, and it is right that the general membership should understand that the smooth realization of our tasks has been due to great skill and foresight in anticipating obstacles and preparing to meet them before they confronted us, and in securing wise counsel and help as to the methods and means to be adopted. I desire to thank all those on the Council and those who, both in our membership and outside, have helped so loyally in these tasks, for what they have done. Where so many have helped it would be invidious to single out individuals, but I cannot refrain from especially acknowledging all that I, personally, and the Institution, owe to our Honorary Treasurer, Sir James Devonshire. In the four tasks I have named, there is not one which would not have been more difficult to achieve, and perhaps even doubtful of achievement in the time actually taken, but for his tireless effort and skilful handling. I have already personally thanked our Chairmen and Officers of Territorial Centres; may I also add my thanks to all members I have come in contact with for their help in the great work of our Institution, and finally, but not least, to Mr. Rowell and the Institution staff.

I have found that it is no light task to be your President; I did not think it would be, but it has been a deep pleasure to me as it has been a great honour which you conferred on me. In many ways it has proved a record session, but we must look forward to greater achievements still.

Yours sincerely,

(Signed) LLEWELYN B. ATKINSON,
President I.E.E.

17*th August*, 1921.

Meeting Days and Hours of Meetings.

The Council have decided to hold the Ordinary Meetings of the Institution next Session on alternate Thursdays (except at times of public holiday) beginning with the first Thursday in November.

The Council have at present under consideration the question whether the Meetings should begin at 6 p.m., as has been the case during the last few years, or whether the former hour of 8 p.m. should be reverted to.

Informal Meetings.

The programme for the Informal Meetings to be held during the forthcoming session is in preparation and the Informal Meetings Committee will be glad to hear from members willing to open a discussion. It is not necessary to submit to the Committee a copy of the proposed introductory remarks, but only the member's name and the subject on which he proposes to speak. Communications should be addressed to the Secretary of the Institution.

Benevolent Fund.

The offer of Mr. E. M. Hughman (see *Institution Notes*, No. 22, p. 2, and No. 25, p. 18) to give a donation of £250 to the Benevolent Fund on condition that 19 similar amounts were contributed by 30th June, has been extended by him to 31st December, 1921.

Associate Membership Examination.

It is proposed, if a sufficient number of candidates make application, to hold an examination in London and at other places (at home and abroad) in October 1921. Entry forms and full particulars may be obtained on application to the Secretary.

The following is a list of the successful candidates at the examination held in this country in April last :—

Whole Examination.

G. H. Ayres (London).
C. H. Brazel (Swansea).
W. N. C. Clinch (London).
W. R. Cooper (Middlesbrough).
V. L. F. Davis (Stoke-on-Trent).
F. J. Knight (Chelmsford).
G. E. Moore (Newcastle-on-Tyne).
F. A. Pike (London).
R. H. Rawll (London).
E. Shipton (London).
A. T. Smee (London).
D. T. Smout (London).

Part II Only.

F. J. Pearce (Sale).

The results relating to candidates who sat for the examination abroad will be published later.

Return Visit of Dutch Electrical Engineers.

At the request of the President, Mr. R. Borlase Matthews has written the following account of the return visit of Dutch electrical engineers to this country in May last :—

In the autumn of 1919 the Institution accepted an invitation of the Association of Managers of Dutch Electricity Supply Undertakings, of which Association Mr. J. G. Bellaar Spruyt was (and still is) President, to send a deputation to visit the Netherlands. An account of the visit will be found in *Institution Notes*, No. 15, page 34, August 1919.

Early this year the Council of the Institution, acting as host on behalf of its members and of those institutions and firms which had been associated with the visit in 1919 to Holland, desiring to return the hospitality of the Dutch engineers, decided to invite the Association to send representatives to visit this country. The Royal Dutch Institute of Engineers was also invited to send a representative. The invitation was cordially accepted and the party of Dutch electrical engineers arrived towards the end of May, after the programme had been altered more than once owing to the coal strike in this country, labour troubles on the Continent, and changes in steamer sailings due to the difficulty of obtaining bunker coal. Most of the British engineering works were running on short time, and it was therefore proposed to change somewhat the character of the visit and to give our Dutch guests an opportunity of seeing the beauties of the English countryside in the month of May. The visitors were greatly pleased with this suggestion, and the method adopted was to convey the party by motor from London to Hereford, and thence along the borders of Wales *via* Chester to Manchester, opportunities being afforded at various points to inspect engineering enterprises.

The Dutch visitors were :—

Mr. J. G. Bellaar Spruyt (*President, the Association of Managers of Dutch Electricity Undertakings*), Managing Director, Stroomverkoop Maatschappij, Maastricht.

*Mr. G. J. F. Bakker (*Secretary, the Association of Managers of Dutch Electricity Undertakings*), City Electrical Engineer, The Hague.

Mr. C. Noome (*Second Secretary, the Association of Managers of Dutch Electricity Undertakings*), Managing Director, Provinciale Utrechtsche Electriciteits Maatschappij, Utrecht.

Mr. E. van Dijk, Managing Director, Provincial Electriciteits Bendrijf in Friesland, Leeuwarden.

Mr. E. B. van Doesburgh, City Electrical Engineer, Groningen.

Mr. H. Lohr, Managing Director, Provinciale Electriciteits Maatschappij, Arnhem.

Mr. J. C. van Staveren, Manager of the Office for High-Tension Lines, Maastricht.

Dr. W. Lulofs, City Electrical Engineer, Amsterdam.

Mr. A. B. van Hamel, Consulting Engineer to the Ministry of Commerce, Department of Economics, The Hague.

Upon arrival at Liverpool-street Station on Sunday, 23rd May, the visitors were met by the President (Mr. Ll. B. Atkinson), the Secretary (Mr. P. F. Rowell), and the following members of the Institution and others who had offered to assist the President in acting as hosts during the tour and also in providing motor cars for the transport of the party : Messrs. A. R. Kibblewhite, R. Borlase Matthews, S. Pollock, and P. Rosling. Sir Thomas Callender, who was represented at the station by Mr. T. Petersen, joined the party personally a couple of hours later.

The visitors were first taken to their headquarters—the Hotel Cecil—and then motored to Tagg's Island, Hampton Court, for luncheon, which was taken at the river's edge. An excursion was then made to the residence of Mr. W. J. Crampton at Weybridge, to inspect his wireless telephone and telegraph installation. Mr. Crampton had arranged with the President of the Dutch Radio Institute to telephone by wireless a speech, in Dutch, from The Hague, and the visitors were also able to "listen in" at a concert which was being given at The Hague. After tea a return was made to the Hotel Cecil, where an informal dinner of welcome was arranged. Mr. Ll. B. Atkinson and Sir Thomas Callender expressed the sentiments of the Institution, to which appreciative responses were made by the visitors.

On the next morning (Monday) the visitors inspected the Institution building, and then the Lots Road

* Representing also the Royal Dutch Institute of Engineers.

power house of the Underground Railways of London, where they were received by Lord Ashfield (Chairman of the Board of Directors), Sir James Devonshire, K.B.E., and the chief officials of the Company.

From Chelsea the party proceeded to St. Stephen's Club, where the chief formal function of the visit took place. This consisted of a luncheon at which the Dutch engineers were formally welcomed by representatives of the Institution and of the various electrical Associations. At the luncheon a communication was read from Lord Stamfordham, stating that owing to a previous engagement His Majesty the King would be unable to receive the Dutch engineers, as had been hoped before the programme of the visit had to be changed.

At the conclusion of the luncheon the guests set out on the first stage of the motor tour proper, a run to Henley. Here, after tea on a riverside lawn, the party embarked in a private launch for a trip on the Thames, returning to Henley for the night.

Hereford was the destination of the next day's drive, a stop being made at Oxford, where Mr. F. H. Francis, Chief Engineer of the Oxford Electric Company Ltd., acted as a guide to the principal colleges, and later Mr. R. J. Petersen entertained the party to luncheon at Ettington Park. A detour was made from the direct route to Hereford, passing through Stratford-on-Avon, in order that the visitors might see a little of Shakespeare's country.

At Hereford, which was reached on Tuesday evening, the party was met by the Mayor, the Chairman of the Electricity Committee, and Mr. W. T. Kerr, the City Electrical Engineer. After dinner, the power station was visited, and on the following day Mr. Kerr conducted the party out of Hereford to show them the overhead line construction and overhead transformer platforms.

On Wednesday, stops were made at Shrewsbury for luncheon, and at Chester, where Mr. S. E. Britton, the City Electrical Engineer, acted as guide to the city walls and old buildings.

At Manchester, on the Thursday, a visit was first paid to the Stuart-street generating station, where Mr. S. L. Pearce, the City Electrical Engineer, conducted the party, and subsequently the constructional work on the new Barton power station was inspected. A special demonstration of the opening of the Barton aqueduct, where the barge canal crosses the Manchester Ship Canal, greatly interested the visitors. Lunch was provided by the Metropolitan-Vickers Company; after which the Company's works at Trafford Park were inspected. The facilities of the port of Manchester for handling goods of every description by mechanical means were then shown to the visitors, and the official visit terminated with a dinner given by the Lord Mayor at the Town Hall. The arrangements at Manchester were in the hands of Alderman W. Walker, who accompanied the party during their stay in the city.

On the following day, Friday, the guests reluctantly left behind their motor cars, in which they had journeyed over 450 miles, and travelled by train to London to catch the steamer next day. A special saloon on the train was kindly provided by the Midland Railway Company through the good offices of Mr. J. Sayers, of Derby.

The Dutch engineers were much interested in all that they saw, especially as, though they all spoke English remarkably well, several of them were strangers to this country. The tour was most successful, in spite of the coal strike, and the visit, it was felt, was not merely a return for hospitality previously extended to English engineers, but rather a further bond between nations, a further establishment of mutual understandings, and a practical demonstration of the fact that engineering in England is at least as far advanced as on the Continent. Great interest was taken by the visitors in the manner whereby the coal-strike negotiations were being conducted, for apparently both Continental masters and men were watching and awaiting the turn of events, as they too had suffered so many industrial disputes which were only temporarily patched up and on the verge of breaking out again.

The Dutch engineers were frankly disappointed at the almost complete lack of electricity supply to rural communities, except in a few places such as Hereford. In the Netherlands, they stated, it has become a matter of national policy, as it is felt that it is most important to keep labour on the land, and it can only be kept there if the rural population are able to obtain a considerable share of the amenities and comforts of city life—and of these electricity holds a foremost place.

Hosts and guests were indebted to individuals who kindly lent motor cars and offered private hospitality, also to the Lord Mayor and Corporation of Manchester for their hospitality to the party.

Accessions to the Reference Library.

BARRETT, W. F. "The telephone." A lecture delivered at Norwich [Dec. 29th, 1877]. Reprinted from the "Eastern Daily Press."
8vo. 22 pp. *London*, [1878]

BATES, L. W. Colloidal fuel. [Paper, Institution of Petroleum Technologists, November 16, 1920].
4to. 11 pp. *London*, 1920

BIGGAR, E. B. Hydro-electric development in Ontario. A history of water-power administration under the Hydro-Electric Power Commission of Ontario.
8vo. 202 pp. *Toronto*, [1920]

BISACRE, F. F. P. Overhead track construction for direct-current electric railways. [Excerpt Minutes of Proceedings of the Institution of Civil Engineers, vol. 208, Session 1918–19, pt. ii].
8vo. 46 pp. *London*, 1921

BOARD OF TRADE. WATER POWER RESOURCES COMMITTEE. Interim report—Second interim report—Third interim report: (Tidal power).
[3 pts.] fol. *London*, 1919–20

BREMNER, D. A. The pathology of industrial unrest, with some special reference to the engineering industry.
sm. 8vo. 8 pp. *London*, 1920

BRIGHT, *Sir* C. Inter-imperial communication through cable, wireless and air. [Paper read before Section F of the British Association, Sept. 12, 1919. Reprinted from the "Electrician," Oct. 17 and 24, 1919].
8vo. 15 pp. *London*, [1919]

British Electrical and Allied Manufacturers' Association. Education and training for the electrical and allied engineering industries. Being a report of the Education Committee of the B.E.A.M.A. sm. 8vo. 64 pp. *London*, 1920

British Engineering Standards Association. [Publications]. no. 1, 12, 94, 97, 100, 106, 111–114, 122, 131, 132, 136. 8vo. *London*, 1920

1, Lists of British standard rolled steel sections for structural purposes. (Revised July, 1920).
12, British standard specification for Portland cement. (Revised 1920).
94, British standard specification for watertight glands for electric cables.
97, British standard specification for watertight fittings for incandescent electric lamps.
100, British standard dimensions for body spaces and frame ends for chassis for private automobiles.
106, British standard specification for electrically heated cooking range. (Two sizes).
111, British standard schedule of wrought steels for aircraft.
112, British standard schedule of cold worked steels for aircraft.
113, British standard schedule of sheet steels for aircraft.
114, British standard schedule of valve and valve spring steels for aircraft.
122, British standards for milling cutters and reamers.
131, Report on British standard forms of notched bar test pieces.
132, British standard specification for steam turbines for electrical plant.
136, Interim British standard terms and definitions for use in connection with automatic telephone systems.

Clayton, A. E. Power factor correction. sm. 8vo. 119 pp. *London*, 1921

Clerk, *Sir* D. Distribution of heat, light, and motive power by gas and electricity. Paper read before the Royal Society of Arts on March 19, 1919. 8vo. 31 pp. *London*, [1919]

—— Smithells, A., and Cobb, J. W. Report on the coal-gas and electrical supply industries of the United Kingdom, to the President of the Institution of Gas Engineers. fol. 38 pp. *London*, [1919]

Conjoint Board of Scientific Societies. Metric Committee. Report on compulsory adoption of the metric system in the United Kingdom. la. 8vo. 70 pp. *n.p.*, [1920]

Curchod, A. Installations électriques de force et lumière. Schémas de connexions. Préface de P. Janet. 3e ed. 8vo. 230 pp. *Paris*, 1914

Davis, C. W., and Simmons, D. M. Allowable working stresses in high-voltage electric cables. [Reprinted from "The Electric Journal," July, 1920]. 4to. 7 pp. *Pittsburgh*, 1920

Dellenbaugh, F. S. A direct recording method of measuring magnetic flux distribution. [Massachusetts Institute of Technology, Publications, Electrical Engineering Research Division, No. 23]. 4to. 5 pp. [*Boston, Mass.*], 1920

Dick, J. R. The economics of works costs. [Reprinted from "The Electrician," Nov. 29, 1918–Jan. 3, 1919]. 8vo. 43 pp. *London, n.d.*

Fields, J. C. Universities, research and brain waste. [President's address, Royal Canadian Institute, Nov. 8, 1919]. 8vo. 27 pp. *Toronto*, 1920

Gordon, H. H. Some aspects of metropolitan road and rail transit. Lecture, with a report of the discussion. [The Institution of Civil Engineers]. 8vo. 83 pp. *London*, 1919

Grierson, R. Dust removal equipment as applied to residences, offices, public buildings, etc. [Institution of Heating and Ventilating Engineers, Ann. gen. meeting, Feb. 8th, 1921]. 8vo. 32 pp. [*London*, 1921]

Hainsworth, A. J. Hydro-electric power stations. [Paper, New Zealand Society of Civil Engineers, February, 1920]. 8vo. 23 pp. *Wellington*, 1920

Home Office. Report of proceedings at Industrial Safety Conference. Organised by the Home Office and The British Industrial "Safety First" Association, and held at Olympia, London, Sept. 22, 1920. 8vo. 79 pp. *London*, 1920

—— Safety pamphlet[s], no. 1–3. 8vo. *London*, 1919–1920

1, Fencing and safety precautions for transmission machinery in factories.
2, Protection of hoists.
3, The use of chains and other lifting gear.

Horsburgh, E. M. Calculating machines. [Institution of Engineers and Shipbuilders in Scotland, Session 1919–20]. 8vo. 46 pp. *Glasgow*, 1920

Hudson, R. G. The theory of metallic arc welding. [Massachusetts Institute of Technology, Publications, Electrical Engineering Research Division, No. 21]. 8vo. 12 pp. [*Boston, Mass.*], 1920

Institute of Inventors. Conference on "The relations of the inventor to the State" held at the Royal Society of Arts, May 13, 1920. 8vo. 34 pp. *London*, 1920

Lockyer, *Sir* N. Sir Norman Lockyer, K.C.B., F.R.S., 1836–1920. [*A collection of personal appreciations.* Reprinted from "Nature," of August 19, 26, and Sept. 2, 1920]. sm. 8vo. 59 pp. *n.p.*, [1920]

McLennan, J. C., *F.R.S., and associates.* Report on some sources of helium in the British Empire. [Canada, Department of Mines, Mines Branch, Bulletin, no. 31]. 8vo. 80 pp. *Ottawa*, 1920

New Zealand. Conference of Electric-Power Boards (constituted) and Electric-Power Board Provisional Committees, held in Parliamentary Building, Wellington, 3rd Aug., 1920. fol. 12 pp. [*Wellington*, 1920]

—— Public Works Statement, 1920. Annual report of Chief Electrical Engineer. fol. 22 pp. [*Wellington*, 1920]

Nicolson, A. McL. The piezo-electric effect in the composite Rochelle salt crystal. [Reprinted from the Proceedings, A.I.E.E., November, 1919]. 8vo. 19 pp. [*New York*, 1919]

Örsted, H. C. Correspondance de H. C. Örsted avec divers savants. Publiée par M. C. Harding, au frais de la Fondation Carlsberg. 2 vol. la. 8vo. *Copenhague*, 1920

—— La découverte de l'électromagnétisme faite in 1820 par J. C. Œrsted. Publication aux frais du Trésor pour le Comité J. C. Œrsted par A. Larsen. 4to. 46 pp. *Copenhague*, 1920

Facsimile reproductions of the Latin pamphlet by Orsted entitled "Experimenta circa effectum conflictus electrici in acum magneticum" *and the French, Italian, German, English translations and a reprint of a Danish translation.*

O'Neill, H. The properties and characteristics of colloidal fuel. [Paper, Institution of Petroleum Technologists, November 16, 1920]. 4to. 31 pp. *London*, 1920

Parry, E. Interference of power circuits with telephone circuits. [Reprinted from "The English Electric Journal," Jan., 1920]. 4to. 8 pp. *London*, 1920

THE INSTITUTION OF ELECTRICAL ENGINEERS.

No. 28. JULY 1921. INSTITUTION NOTES. [ISSUED WITH No. 303 OF THE JOURNAL.]

Arrangements for November and December, 1921.

ORDINARY MEETINGS OF THE INSTITUTION.

(*Thursdays.*)

[To be held at the Institution, Savoy Place, Victoria Embankment, London, W.C. 2, at 6 p.m. (light refreshments 5.30 p.m.).]

Date.	*Author and Title of Paper.*
1921.	
3 Nov.	J. S. HIGHFIELD: Inaugural Address as President.
17 Nov.	E. S. BYNG: "Telephone Line Work in the United States."
1 Dec.	L. J. STEELE and H. MARTIN: "The Cyc-Arc Process of Automatic Electric Welding."
15 Dec.	L. H. A. CARR: "Induction-type Synchronous Motors."

MEETINGS OF THE WIRELESS SECTION.

(*Wednesdays.*)

[To be held at the Institution, Savoy Place, Victoria Embankment, London, W.C. 2, at 6 p.m. (light refreshments 5.30 p.m.).]

9 Nov.	Prof. G. W. O. HOWE, D.Sc.: Chairman's Address.
7 Dec.	T. L. ECKERSLEY: "An Investigation of Transmitting Aerial Resistances."

Students' Premiums.

The following premiums have been awarded by the Council for papers read before the Students' Sections during the Session 1920–21.

Value of Premium.	*Author.*	*Title of Paper and where read.*
£15	H. E. DANCE	"Design of Armature Windings for D.C. Dynamo-electric Machinery" (Liverpool).
£15	R. T. FLEMING	"Properties and Characteristics of Insulating Materials" (Manchester).
£15	A. ROSEN	"Telephone Transmission through Submarine Cables" (London).
£10	A. C. WARREN	"Radio-telegraphic Transmitting Apparatus" (London).

Royal Charter.

A facsimile of the Royal Charter granted to the Institution will be found on pages 31 to 36 of these Notes. The Council's petition for a Charter was in the following terms:

TO THE KING'S
MOST EXCELLENT MAJESTY IN COUNCIL.

The Petition of

LLEWELYN BIRCHALL ATKINSON, Electrical Engineer, of Sardinia House, Sardinia Street, Kingsway, in the County of London; WILLIAM ASHCOMBE CHAMEN, Electrical Engineer, of Royal Chambers, Queen Street, Cardiff, in the County of Glamorgan; WILLIAM HENRY ECCLES, D.Sc., F.R.S., Electrical Engineer, of The Finsbury Technical College, Leonard Street, Finsbury, in the said County of London; JOHN SOMERVILLE HIGHFIELD, Electrical Engineer, of 36, Victoria Street, Westminster, in the said County of London; ALAN ARCHIBALD CAMPBELL SWINTON, F.R.S., Electrical Engineer, of 66, Victoria Street, Westminster, in the said County of London; and Sir JAMES LYNE DEVONSHIRE, K.B.E., Electrical Engineer, of Electric Railway House, Broadway, Westminster, in the said County of London.

MOST HUMBLY SHOWETH:—

1. That in the year 1871 a Voluntary Association was formed under the name of The Society of Telegraph Engineers having for its objects the general advancement of Electrical and Telegraphic Science.

2. That in view of the fact that the qualification for membership of the Society was not confined to Telegraph Engineers the name of the Society was in the year 1880 changed to The Society of Telegraph Engineers and Electricians with the object of indicating more clearly its scope and the eligibility of persons engaged in all or any branches of Electrical Science for membership of the Society.

3. That in the year 1883 The Society of Telegraph Engineers and Electricians was incorporated under the Companies Acts 1862 and 1867 with the object

(*inter alia*) of succeeding to and taking over the property rights and obligations of the Society the name of the Society being changed in the year 1888 to The Institution of Electrical Engineers in consequence of the great development which took place about that time in the application of electricity to lighting and heavy engineering the term "Electrical Engineer" being substituted as embracing all classes of members of the Institution.

4. That the Institution is composed of Corporate Members (namely Honorary Members Members and Associate Members) and Non-Corporate Members (namely Associates Graduates and Students) to the total number at the present time of about 9 400.

5. That the property and affairs of the Institution are under the direction and management of a Council consisting of the President and Past-Presidents four Vice-Presidents the Honorary Treasurer eighteen Ordinary Members of Council all of whom are elected by the Corporate Members and Associates of the Institution and the Chairman and a Past Chairman of each Local Section of the Institution which the Council may create in any district.

6. That the name of a candidate for admission to Corporate Membership of the Institution cannot be submitted for ballot unless with the approval of the Council who before giving such approval satisfy themselves as to the professional status and qualifications of the candidate and that Corporate Membership of the Institution is universally recognized as implying the possession of high professional qualifications for the practice of the profession of an Electrical Engineer.

7. That the Institution is not constituted for gain and that its members have not received and cannot under the terms of its constitution receive any remuneration or other financial advantage from their membership.

8. That in pursuance of the objects for which the Institution was established its efforts have been particularly directed to the promotion of the general advancement of Electrical Science and its applications and the facilitating of the exchange of information and ideas on subjects connected with that Science among the members of the Institution and otherwise.

9. That the incorporation in 1883 of the Institution under its then name of the Society of Telegraph Engineers and Electricians was approximately contemporaneous with the commencement of the vast and rapid development of Electrical Science and Discovery and of modes of Application of Electricity which has taken place within the last generation.

10. That the Institution includes and has for many years past included among its members all the leading British Electrical Engineers of the time and has become an increasingly important body representative of the profession of Electrical Engineers.

11. That members of the Institution have either originated or been intimately concerned with all the principal developments of Electrical Science which have taken place since the establishment of the Institution.

12. That the development and improvement through the instrumentality of the Institution and its members of the modes of Application of Electricity to many and varied purposes have proved highly beneficial to human life and health and have played an important part in the growth of the trade and commerce of the world.

13. That your Petitioners believe that the incorporation of the Institution by Your Majesty's Royal Charter and the recognition thereby of its status as the body representative of the profession of Electrical Engineering will be the means of enabling the Institution more fully to achieve the objects which it has in view and will tend towards the further development of Electrical Science and the maintenance of a high standard of professional conduct among its members and thus serve the best interests of the community.

14. That Your Petitioners Llewelyn Birchall Atkinson, who is President of the Institution, William Ashcombe Chamen, William Henry Eccles, John Somerville Highfield, and Alan Archibald Campbell Swinton, who are Vice-Presidents of the Institution, and Sir James Lyne Devonshire, who is Honorary Treasurer of the Institution, have been duly authorized by the Council of the Institution to present this Petition, and that such presentation has been approved by a Resolution passed at a Special General Meeting of the Institution.

FOR THESE AND OTHER REASONS your Petitioners on behalf of themselves and the other members of The Institution of Electrical Engineers most humbly pray that Your Majesty will be graciously pleased to grant a Royal Charter for incorporating under the title of "The Institution of Electrical Engineers" or under such other title as to Your Majesty may seem fit and with all such powers and privileges as may be necessary or proper the Petitioners and the several persons who are now members of the present Institution and other parties who may hereafter become members of the said Institution to be incorporated in pursuance of the regulations thereof and that in terms of the draft Charter herewith submitted or in such other terms as to Your Majesty may seem proper.

AND YOUR PETITIONERS WILL EVER PRAY, ETC.

SIGNED at London this twenty-ninth day of June, One thousand nine hundred and twenty-one

LLEWELYN B. ATKINSON.
W. A. CHAMEN.
W. H. ECCLES.
J. S. HIGHFIELD.
A. A. C. SWINTON.
JAMES LYNE DEVONSHIRE.

[FACSIMILE OF THE ROYAL CHARTER OF INCORPORATION.]

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George the Fifth, by the Grace of God of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith:

To all to whom these Presents shall come, Greeting.

Whereas the Association or Institution incorporated under the Companies Acts, 1862 and 1867, and known as the Institution of Electrical Engineers hath petitioned Us for a Charter of Incorporation such as is in and by these Presents granted.

And whereas we are minded to comply with the prayer of such Petition:

Now, therefore, We, by virtue of Our Royal Prerogative in that behalf, and of all other powers enabling Us so to do, of Our Special Grace certain knowledge and mere motion do hereby, for Us, Our Heirs and Successors, will, grant, direct, appoint, and declare to the said Association or Institution as follows:—

1. The persons now Members of the said Association or Institution known as the Institution of Electrical Engineers, and all such persons as may hereafter become Members of the Body Corporate hereby constituted pursuant to or by virtue of the powers granted by these Presents, and their Successors, shall for ever hereafter (so long as they shall continue to be such Members) be by virtue of these Presents one Body Corporate and Politic by the name of "The Institution of Electrical Engineers" and by the same name shall have perpetual succession and a Common Seal, with power to break, alter, and make anew the said Seal from time to time at their will and pleasure, and by the same name shall and may implead, and be impleaded in all Courts, and in all manner of actions and suits, and shall have power to do all other matters and things incidental or appertaining to a Body Corporate.

2. We do also hereby, for Us, Our Heirs, and Successors, license, authorize and for ever hereafter enable the Institution of Electrical Engineers hereby incorporated, or any person or persons on its behalf, to acquire for the purposes of the Institution, any lands, tenements or hereditaments or any interest in any lands, tenements or hereditaments whatsoever within Our United Kingdom of Great Britain and Ireland, not exceeding in the whole the annual value of ten thousand pounds (to be determined according to the value thereof at the time when the same are respectively acquired) and to hold the same in perpetuity or otherwise and from time to time (subject to all such consents as are by law required) to grant, demise, alienate, or otherwise dispose of the same or any part thereof.

3. And We do also hereby, for Us, Our Heirs, and Successors, give and grant Our License to any person or persons and any Body Politic or Corporate to assure in perpetuity or otherwise, or to demise to or for the benefit of the Institution of Electrical Engineers any lands, tenements or hereditaments whatsoever, or any interest in any lands, tenements or hereditaments within Our United Kingdom of Great Britain and Ireland, so as the same do not exceed at any one time the annual value aforesaid; hereby, nevertheless, declaring that it shall not be incumbent upon any such person or persons or Body to inquire as to the annual value of the property which may have been previously acquired by the Institution.

4. The objects and purposes for which the Institution of Electrical Engineers (hereinafter called "the Institution") is hereby constituted are to promote the general advancement of Electrical Science and engineering and their applications and to facilitate the exchange of information and ideas on those subjects amongst the Members of the Institution and otherwise and for that purpose—

(*a*) To hold meetings of the Institution for reading and discussing communications bearing upon Electrical Science and engineering, or the applications thereof, or upon subjects relating thereto.

(*b*) To hold or promote Exhibitions of instruments, apparatus, machinery or other appliances connected with Electrical Science and engineering, or their applications.

(*c*) To print, publish, sell, lend or distribute the proceedings or reports of the Institution, or any papers, communications, works or treatises on Electrical Science and engineering or their applications, or subjects connected therewith, in the English or any foreign tongue, or any abstracts thereof, or extracts therefrom.

(*d*) To take charge of the books, pamphlets, publications and manuscripts known as the Ronalds Library, which are the subject of a deed of trust dated 24th June, 1875, and to observe and perform the Trusts of the said Deed, and to add to the said Ronalds Library, and form, acquire, receive, hold and take charge of any supplemental or additional library of books, works or manuscripts on Electrical Science and engineering, or the applications thereof, or other subjects allied thereto.

(*e*) To make grants of money, books, apparatus, or otherwise for the purpose of promoting invention and research in Electrical Science and engineering, or their applications, or in subjects connected therewith.

5. The Institution shall not carry on any trade or business or engage in any transaction with a view to the pecuniary gain or profit of the Members thereof. No Member shall have any personal claim on any property of the Institution and no part

of the income or property of the Institution shall be paid or transferred directly or indirectly by way of dividend or bonus or otherwise howsoever by way of profit to the Members of the Institution except in the case of and as a salaried Officer of the Institution.

6. There shall be a Council of the Institution consisting of such number of members, with such qualifications, and to be elected or constituted as such members of Council in such manner, and to hold office for such period, and on such terms as to re-election and otherwise, as the Bye-laws for the time being of the Institution shall direct.

7. Of the Members of the said Council of the Institution one shall be the President, and two or more shall be the Vice-Presidents of the Institution. The said President and Vice-Presidents shall be elected in such manner, and shall hold office for such period, and on such terms as to re-election and otherwise, as the Bye-laws for the time being of the Institution shall direct.

8. The first President of the Institution shall be Llewelyn Birchall Atkinson and the first Vice-Presidents shall be William Ashcombe Chamen, William Henry Eccles, D.Sc., F.R.S., John Somerville Highfield and Alan Archibald Campbell Swinton, F.R.S.

The first Honorary Treasurer of the Institution shall be Sir James Lyne Devonshire, K.B.E.

The first Members of the Council shall be the said President, Vice-Presidents and Honorary Treasurer, the following Past Presidents of the said Voluntary Association or Institution, namely: Alexander Siemens, Colonel Rookes Evelyn Bell Crompton, C.B., Sir Henry Christopher Mance, C.I.E., LL.D., James Swinburne, F.R.S., Sir John Gavey, C.B., Sir Richard Tetley Glazebrook, K.C.B., D.Sc., F.R.S., William Morris Mordey, Gisbert Kapp, M.Sc., Sebastian Ziani de Ferranti, D.Sc., Sir John Francis Cleverton Snell, Charles Pratt Sparks, C.B.E., Charles Henry Wordingham, C.B.E., and Roger Thomas Smith; the following Ordinary Members of Council, namely: Percy Ruskin Allen, O.B.E., Henry James Cash, Henry William Clothier, John Robert Cowie, Daniel Nicol Dunlop, Samuel Edgar Fedden, Ernest Arthur Gatehouse, Frank Gill, O.B.E., Philip Vassar Hunter, C.B.E., Edgar Walford Marchant, D.Sc., Albert Henry Weaver Marshall, Sir William Noble, Clifford Copland Paterson, O.B.E., Alexander Russell, D.Sc., Josiah Sayers, O.B.E., Philip Debell Tuckett, Charles Vernier and Burkewood Welbourn, and the following Chairmen and Past Chairmen of Local Sections of the said existing Association or Institution, namely: James Robert Beard, Albert George Bruty, William Cross, Arthur Ellis, Frank Forrest, Julius Frith, Charles Cornfield Garrard, Frank Graham, William Benison Hird, Charles James Jewell,

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William Cranswick Laidler, John Willoughby Meares, Arthur Joseph Newman, James Archibald Robertson, James Edmund Sayers, William Morrish Selvey and Richard Tanham.

The said President, Vice-Presidents, Honorary Treasurer and Members of the Council shall respectively hold office as such until the due election and coming into office of their successors in accordance with the Bye-laws of the Institution, but shall be respectively eligible, subject to such Bye-laws, for re-election if otherwise qualified.

9. The Institution shall have such Officers, with such functions, tenure and terms of office as the Bye-laws of the Institution may prescribe, and such other Officers and Servants as the Council of the Institution may from time to time appoint. The First Secretary of the Institution shall be Percy Fitz-Patrick Rowell.

10. The government and control of the Institution and its affairs shall be vested in the Council subject to the provisions of these Presents and to the Bye-laws of the Institution. The business of the Council shall be conducted in such manner as the Council may from time to time prescribe.

11. Unless and until the Bye-laws of the Institution shall otherwise provide there shall be six classes of members of the Institution termed respectively Honorary Members, Members, Associate Members, Associates, Graduates, and Students, of whom the Honorary Members, Members and Associate Members, shall be known as Corporate Members and the Associates Graduates and Students shall be known as non-Corporate Members.

The Members of the said existing Association or Institution known as the Institution of Electrical Engineers, who by virtue of these Presents become Members of the Institution shall be deemed to have entered the Institution as Members of the same class as that to which they belonged in the said existing Association or Institution.

12. The qualifications, method and terms of admission, privileges and obligations, including liability to expulsion or suspension of Members of each of the said six classes respectively, shall be such as the Bye-laws for the time being of the Institution shall direct.

13. The Council shall alone have power to decide conclusively respecting each person proposed for or seeking admission to any class of membership of the Institution whether he has or has not fulfilled such conditions as are applicable to his case.

14. An Honorary Member shall be entitled to the exclusive use after his name of the initials "Hon.M.I.E.E."; a Member of the initials "M.I.E.E."; an Associate Member of the initials

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"A.M.I.E.E."; an Associate of the initials "Associate I.E.E.", a Graduate of the initials "Graduate I.E.E." and a Student of the initials "Student I.E.E."

15. Every person being at any time a Corporate Member of the Institution may so long as he shall be a Corporate Member take and use the name or title of or describe himself as a member of the Chartered Institution of Electrical Engineers.

16. The meetings of the Institution shall be of such classes and shall be held for such purposes as may from time to time be prescribed by the Bye-laws of the Institution, and the rights of the several classes of members of the Institution of attending and voting at the said meeting shall be such or subject to such restrictions as may be so prescribed.

17. The Corporate Members or the majority of such Members present in person or by proxy and voting at any duly convened Special General Meeting of Corporate Members with respect to which notice has been given of the matters to be taken into consideration thereat, shall have power from time to time to make such Bye-laws of the Institution as to them shall seem requisite and convenient for the regulation, government and advantage of the Institution, its members and property, and for the furtherance of the objects and purposes of the Institution, and from time to time to revoke, alter or amend any Bye-law or Bye-laws theretofore made so that the same be not repugnant to these Presents or to the Laws and Statutes of this Our Realm; Provided that no such Bye-law, revocation, alteration or amendment shall take effect until the same has been allowed by the Lords of Our Privy Council of which allowance a Certificate under the hand of the Clerk of Our Privy Council shall be conclusive evidence.

18. The first Bye-laws to be made under these Presents shall be made by the Corporate Members of the Institution, within the period of six months from the date of these Presents, unless the Lords of Our Privy Council shall see fit to extend such period, of which extension the Certificate of the Clerk of Our Privy Council shall be conclusive evidence.

19. Pending the making and approval of the Bye-laws to be made under these Presents but no longer, the Articles of Association of the said existing Association or Institution known as the Institution of Electrical Engineers shall be the Bye-laws of the Institution, and shall have effect as though the Institution, its Officers and Members had therein been referred to throughout in lieu of the said existing Association or Institution, its Officers and Members.

20. The property and moneys of the said existing Association or Institution known as the Institution of Electrical Engineers (including any property and moneys held by them as trustees) shall from the date of these Presents become and be deemed to

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be the property and moneys of the Institution, and shall, as soon as may be, be formally transferred to the Institution or such person or persons on its behalf as the Bye-laws may prescribe.

21. And We do hereby, for Us, Our Heirs, and Successors, Grant and Declare that these Our Letters Patent, or the enrolment or exemplification thereof, shall be in all things good, firm, valid, and effectual, according to the true intent and meaning of the same, and shall be taken, construed and adjudged in all Our Courts or elsewhere in the most favourable and beneficial sense and for the best advantage of the said Institution, any mis-recital, non-recital, omission, defect, imperfection, matter or thing whatsoever notwithstanding

In Witness whereof We have caused these Our Letters to be made Patent.

Witness Ourself at Westminster, the fifteenth day of August in the Year of our Lord One thousand nine hundred and twenty-one and in the Twelfth year of Our Reign.

By Warrant under the King's Sign Manual.

Schuster.

Informal Meetings.

The programme for the Informal Meetings to be held during the forthcoming session is in preparation and the Informal Meetings Committee will be glad to hear from members willing to open a discussion. It is not necessary to submit to the Committee a copy of the proposed introductory remarks, but only the member's name and the subject on which he proposes to speak. Communications should be addressed to the Secretary of the Institution.

"Beama" Journal.

The price of single copies of the above Journal to Students of the Institution is 1*s.* 8*d.* *plus* postage (5*d.* in the United Kingdom), *not* 1*s.* 8*d.* post free as stated in the circular to Students dated August, 1921, which was enclosed with No. 302 of the *I.E.E. Journal.*

Students' Visit to Switzerland.

The Secretary has received for publication the following account of the Students' Visit to Switzerland in July, 1921 :—

The 1920 Summer Visit to Glasgow having proved so successful, the Committee of the London Students' Section decided this year to attempt a more ambitious trip. A programme was accordingly drawn up and a special Tour Secretary was authorized to make the necessary arrangements, in conjunction with Messrs. Thos. Cook & Son, for a 10 days' trip to Switzerland. It was decided to organize the affair for the Students' Section as a whole, and, in all, 43 members took part, representative of the whole of the country.

Leaving London on Saturday, 23rd July, at 8.20 a.m., the party eventually arrived at Zurich, via Calais and Basle, at 9 a.m. on Sunday. The rest of the day was allowed for recuperation, but the majority of the members took the opportunity of seeing the town or bathing in the lake.

On the Monday the party was divided into two groups. The first of these visited the Maschinenfabrik Oerlikon in the morning, an interesting feature being the new electric locomotives on order for the Swiss Federal Railways. These locomotives, it is understood, are designed for a single-phase 15 000-volt $16\frac{2}{3}$ period system and will be used at first on the Zurich–Lucerne portion of the route, the electrification of which is already in progress. The party was entertained to luncheon by the firm. In the afternoon, at the works of Messrs. Sulzer Brothers, Diesel and Uni-flow engines were seen in various stages of construction and erection, and a test on a large Diesel engine was also witnessed.

The other group, in visiting Messrs. Escher Wyss & Co.'s works in the morning, had an opportunity of seeing some of the latest turbine practice. The method adopted with steam turbines, of building the blades up in the mould and so making them solid with the casting, was distinctly novel. The afternoon was spent in a trip to Messrs. Brown Boveri's works at Baden, where among other things the mercury-arc rectifiers proved of considerable interest. A demonstration was also given in the high-tension test-shop of the danger of standing too near a body at high potential. A tank A was placed about 18 inches from a second tank B, and the potential of B was gradually raised until a flash-over took place. It was shown that when A was insulated (but in proximity to earth) the flash-over potential was less than when it was definitely earthed, due to the acquisition of a charge by static induction. After the visit to the works the party was entertained to tea.

On the Tuesday morning a visit was paid to the University at Zurich, where the authorities provided English-speaking guides and took considerable trouble to render the visit interesting. The party left Zurich the same afternoon for Lucerne where the rest of the day was spent.

Early next morning the party set out for Meiringen via the Brünig Pass. This being the first rack railway travelled on, considerable interest was aroused by the various manœuvres, such as the reversing of the engines at the summit, while the scenery was the finest so far encountered. At Meiringen a visit was paid to a carbide factory at the invitation of the owner, Herr Frey-Fürst, who subsequently conducted the party to the Reichenbach Falls and power station. The carbide furnace was three-phase, taking a current of 20 000 amperes at 40 volts, the power being supplied from the Reichenbach station. In the afternoon the Gorge of the Aar was visited and the party left for Brienz at 4.55, taking the steamer from there to Interlaken.

A welcome relief from the strenuous effort of the initial stages was provided by two days' clear rest here. Some of the party visited the Trummelbach Falls and the Jungfrau power station (600 ft. head and Pelton wheels) on the Thursday afternoon, while a group of about 30 went up the Jungfrau itself on the Friday. The journey is via Lauterbrunnen to Scheidegg by rack railway (an electrified system using direct current with overhead collection), and thence by the Jungfraubahn (three-phase) for the last 5 miles to the Jungfraujoch. This latter portion, being above the snow line, is in a tunnel all the way, occasional stations being provided where passengers may alight and inspect the glacier. The last station at the Jungfraujoch (shoulder) is 10 600 ft. above sea-level, the summit of the Jungfrau itself being 12 700 ft. The return journey was via Grindelwald.

The final portion of the tour commenced the next morning when the party left for Spietz. At this point Herr Fischer of the Bell Telephone Manufacturing Co. kindly came from Berne to meet the party, and acted as guide and interpreter for the rest of the tour. The morning was spent in a visit to Spietz hydro-electric station, a fair example of high-head plant, the fall being 120 ft. Power is generated at 16 000 volts, 40 periods, three-phase, and is stepped up to 45 000 volts for long-distance lines. There is also a 15 000-volt, $16\frac{2}{3}$ period, single-phase supply for the Swiss Federal Railways. Water

leaks are used for preventing the accumulation of static charges on the line.

The party then proceeded to Berne and visited the locomotive sheds of the Swiss Federal Railways, where the latest type of locomotive for the Lötschberg–Simplon line were inspected. These machines, which are built for the standard voltage (15 000, $16\frac{2}{3}$ periods) are for the Berne–Lötschberg portion of the route, the Simplon portion being three-phase. The horse-power developed is 2 200 on the one-hour rating, the maximum being 2 600, and the locomotives are required to draw a train up a gradient of 2·5 per cent, one of the heaviest known on a pure adhesion system.

The same evening a farewell dinner was held, at which a presentation was made to the Tour Secretary, Mr. J. H. Reyner, as a memento of a very successful tour. In reply, Mr. Reyner thanked the members for their support and hoped that the trip was only the first of many equally enjoyable. The present took the form of a silver model of the Sharpshooter Fountain in the Marktgasse, Berne.

The visit concluded the next morning with a motor-boat trip to Mühleberg hydro-electric station, the most up-to-date installation in Switzerland, having been completed only towards the end of the war. The river Aar has been dammed and a fall of about 60 ft. has thus been obtained. The plant, which is classed as low-head, consists of six 8 000-h.p. machines each generating at 16 000 volts, 40 periods, transmission being at 45 000 volts, as at Spietz, with which the station is interlinked. It is interesting to note, however, that as it is hoped ultimately to adopt a 50-period standard throughout the country, the generators have been so designed that with a very slight alteration they can be made to supply 50-period current. In addition to the six generators there are two 8 000-kVA converters which are used to transform the three-phase 40-period current to single-phase at $16\frac{2}{3}$ periods, 16 000 volts, for the railways. An unusual feature is the provision of six direct-current generators and a battery, as a stand-by for the excitation, lighting, and control mechanism of the station. The switching and control are centred in a small room overlooking the main turbine house, while the feeder switchgear and transformers are in an entirely separate building. After a most interesting morning, the motor boat took the party back to Berne via Neubrucke, an old wooden bridge built some 400 years ago.

The tour came to an end the same night, some members remaining for a longer time in Switzerland, and others going straight home, while a group of about 20 visited Paris. The Committee desire to express their thanks to the various firms for their courtesy during the tour; also to Mr. Harley, H.B.M. Consul, Zurich; to Herr Frey-Fürst of Meiringen; and to Herr Fischer of Berne for their very material contributions to the success of the undertaking. They feel that the tour has done much to increase the activity of the Students' Section as a whole, and to enhance the co-operation between the various Students' Centres. They also wish to acknowledge the help received from the Institution, and particularly from Mr. F. Gill, Member of Council, in making the arrangements for the tour.

Associate Membership Examination Papers.

The following papers were set at the Examinations held in April 1921 and September 1921 :—

ASSOCIATE MEMBERSHIP EXAMINATION.

APRIL 1921.

Friday, 29 April, 1921, 10 a.m.–1 p.m.

ENGLISH ESSAY.

Examiner : C. C. HAWKINS, M.A., M.I.E.E.

(*Time allowed : 3 hours.*)

INSTRUCTIONS.

The questions are of equal value, and not more than TWO *are to be answered. One essay of sufficient merit and length will suffice to obtain a pass.*

Marks will be awarded for grammar, spelling, punctuation, clear and simple style, orderly presentation of facts or arguments, and power of expressing ideas in good English. Candidates are not required to have special or technical knowledge of the subject-matter of their essays, and the views supported are not material to success.

1. Write an essay on the foundation and future prospects of the League of Nations.

2. What is your opinion of the scope and usefulness of the welfare movement in our factories ?

3. Write an account of the applications of photography in the field of science, pure and applied.

4. Discuss the advantages of knowing a foreign language and the considerations that decide the best one for an engineer to learn.

5. Is profit-sharing likely to prove the solution of the problem of industrial unrest ?

Friday, 29 April, 1921, 10 a.m.–1 p.m.

TRANSLATION FROM SPANISH.

Examiner : E. SLATER, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Particular attention must be paid to clearness of expression and good construction as well as to correct translation.*)

A.

Según la teoría de Richardson, el número de electrones emitidos por un filamento a cualquier tempera-

tura es independiente del campo eléctrico que lo rodea. Si éste no existe, los electrones emitidos por el cuerpo vuelven al mismo siendo reabsorbidos por él. En cambio, si se coloca otro cuerpo cargado positivamente en las inmediaciones del filamento en incandescencia, los electrones emitidos por éste son atraídos por aquél y su paso por el espacio que separa ambos cuerpos constituye una corriente eléctrica que va del filamento en incandescencia como *cátodo* al cuerpo cargado positivamente como *ánodo*.

A medida que aumenta el potencial del ánodo mayor es la proporción de electrones atraídos por él, hasta alcanzar un punto en que todos los electrones libres del electrodo caliente son absorbidos por el frío y cualquier aumento de potencial de este último no produce aumento ninguno de la corriente que a él se dirige. En este caso se dice que la corriente está saturada y su intensidad es igual a la carga total de todos los electrones emitidos por el filamento en un segundo.

Si el cuerpo que se coloca cerca del filamento en incandescencia está cargado negativamente, los electrones son repelidos por aquél y vuelven al filamento, como en el caso en que no hay campo eléctrico exterior.

Richardson ha dado el nombre de *corrientes termoiónicas* a las que se producen entre un filamento incandescente y un electrodo frío cargado positivamente.

B.

Los tubos de condensador son de una aleación de dos metales que ocupan los extremos de la escala citada, el cinc es por naturaleza un elemento muy instable, es electro-positivo al cobre, luego la corrosión que en ellos se produce debe ser rápida.

La acción electrolítica se produce en toda la extensión del tubo, quedando todo su interior cubierto de una capa gris de óxido que al ser removida aparece de un color negruzco; de trecho en trecho se notan pequeñísimas prominencias de óxido las cuales removidas por una herramienta puntiaguda, permiten reconocer que cubren la picadura inicial de ataque, que con el tiempo se extiende en longitud. Donde se asocia el óxido con el cobre empieza la acción electro-química, y puede notarse entonces la eliminación completa del cinc y la producción de óxido rojo y cobre electrolítico como resultado de una acción secundaria de reducción. Podría preguntarse el por qué no se utilizan otros metales para las aleaciones de los tubos. Es fácil contestar a ello, recordando que una aleación de cobre con plomo o estaño da un cobre duro y quebradizo impropio para ser estirado en tubos; una aleación de plomo y estaño no resistiría a la temperatura que lleva el vapor en la descarga, y sólo quedaría una aleación bronce-níquel que se ha llevado a cabo para tubos de condensador y conocida bajo el nombre de Benedict-Niquel. Esta aleación de color blanco como el níquel puro, es mucho más cara y sufre también las consecuencias de la electrólisis, aunque no de una manera tan pronunciada como la del bronce, no compensando esa cualidad su diferencia de costo, pero presentando la ventaja de no ser atacada directamente por el aire salino ni por el agua del mar.

C.

Cuando Herschel, dirigiendo su gran reflector á varias manchas nebulosas, comprobó que eran resolubles en grupos de estrellas, dedujo y sostuvo por algún tiempo que todas las manchas nebulosas eran grupos de estrellas situados á inmensa distancia de nosotros, pero después de años de concienzuda investigación, concluyó que "había nebulosidades que no eran de naturaleza estelar," y sobre esta conclusíon se basó su hipótesis de un fluido difuso luminoso que produjo estrellas por sus agregaciones eventuales. Una potencia telescópica mucho más fuerte que la usada por Herschel ha dado medios á lord Rosse para resolver algunas de las nébulas antes irresueltas, y volviendo á la conclusión que había formado primero Herschel sobre semejantes fundamentos, pero que rechazó más tarde, varios astrónomos han supuesto que, bajo potencias visuales suficientemente elevadas, cada nébula sa descompondría en estrellas; que la irresolubilidad no se debe más que á la distancia. La hipótesis sostenida generalmente ahora es que todas las nébulas son vías lácteas más ó menos semejantes en naturaleza á la que inmediatamente nos rodea; pero que están tan inconcebiblemente lejos, que al mirarlas por los telescopios ordinarios parecen pequeñas manchas, y no pocos han sacado como corolario que la hipótesis de la nebulosa ha sido desaprobada por los descubrimientos de lord Rosse.

D.

Las turbinas de vapor van ganando terreno poco á poco á las máquinas de émbolo La turbina se presta mejor que la máquina de émbolo para la producción de electricidad, á partir de una fuerza de 500 caballos. La centralización de la energía en un punto dado, produciendo electricidad para distribuirla desde el mismo, es una de las características de nuestro tiempo. Las turbinas de vapor se han perfeccionado hasta tal punto que es posible producir el equivalente de un caballo indicado con menos de 5 kilos de vapor recalentado á temperaturas hasta de 350° C. y presiones de 14 á 17·5 kilos por centímetro cuadrado, y contando con suficientes elementos de condensación para alcanzar un alto grado de vacío. En las mejores centrales modernas de electricidad, la producción de vapor se efectúa mediante una combinación de calderas provistas de recalentadores, cargadores mecánicos, economizadores, maquinaria para el transporte del carbon, é instalación para la aspiracion y arrastre de cenizas. La economía obtenida merced al rendimiento que produce la adecuada combinación de todos estos elementos se aumenta, por otra parte, con el ahorro de jornales. En la mayor parte de las instalaciones modernas no se toca con las manos el carbón ni las cenizas; las principales funciones de los encargados de las calderas consisten en manejar las válvulas de los depósitos de carbón para llenar las tolvas de los cargadores mecánicos, cuidar del nivel del agua y atender á las válvulas del agua de alimentación.

Friday, 29 April, 1921, 2 30 p.m.–5.30 p.m.

MECHANICS, PHYSICS, AND CHEMISTRY.

Every candidate must take Section A together with either Section B or Section C.

(Time allowed : 3 hours.)

SECTION A. APPLIED MECHANICS.

Examiner : PROFESSOR A. J. MARGETSON, M.Sc., M.I.Mech.E.

(Not more than FOUR *questions to be answered.)*

1. What is meant by the term "coefficient of friction" between two metal surfaces? How is the coefficient altered in value when the surfaces are lubricated?

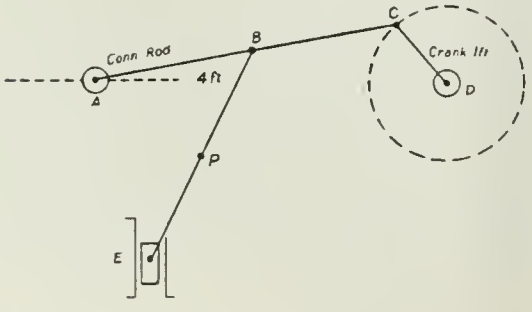
Find the H.P. used in overcoming the resistance of an unlubricated bearing in which the journal is 6 inches diameter running at 200 revs. per minute. The pressure between the journal and the bearing is 3 tons. Take a coefficient of friction of 0·1.

2. Explain the term "radius of gyration." A disc flywheel 4 feet diameter by 9 inches width is made to revolve at 300 revs. per minute. What is the energy stored up in it? Assume the weight of the metal to be 480 lb. per cubic foot and that the radius of gyration is 1·414 feet. Can you prove this last figure to be correct?

3. Explain why a beam which is securely built in supports on both sides of the span can be made of lighter section than one which merely rests on supports. Illustrate your answer by referring to a beam covering a span of 10 feet and carrying a distributed load of 1 ton per foot run. Draw approximately to scale the shearing force and bending moment diagrams in each case.

4. A metal hoop of weight 10 lb. and diameter 5 feet starts from rest and acquires a rolling speed of 10 miles per hour in 10 seconds. Find its angular acceleration if uniform. What is its final state as regards kinetic energy and what torque must have been applied to produce this result? Assume the acceleration to have been uniform.

5. Obtain by the method of instantaneous centres



the velocity and direction of the point P in the mechanism shown in Fig. above. Take AB 2 feet, EB 3 feet, EP 1½ feet. Angle CDA 30°. A moves along a straight line towards D. Crank CD revolves at 100 revs. per minute. The end E is constrained to move vertically. At the moment when the mechanism is as shown the angle ABE is 75°.

6. Explain carefully the difference between the function of a governor and that of a flywheel. In the case of a loaded governor of the Watt type why does the application of a load on the ends of the lower links enable the governor to function at a higher speed?

SECTION B. PHYSICS.

Examiner : J. K. CATTERSON-SMITH, M.I.E.E.

(Not more than FOUR *questions to be answered.)*

1. How is sound produced and transmitted through air? Distinguish between pitch and loudness. Describe how a telephone receiver produces sound.

2. Give a short account of "Thermo-electric" phenomena and describe how you would show the "Peltier" and the "Thomson" effects in a thermo-electric circuit.

3. How may currents be measured by the electro-deposition of metals? How many coulombs would it take to deposit 10 grams of metal from a copper-sulphate solution and what current would be required for one hour? The electro-chemical equivalent of copper may be taken as 0·000329 gram per coulomb.

4. State some of the reasons for thinking that light is an electro-magnetic phenomenon.

5. What are the First and Second Laws of Thermodynamics? How is heat energy transformed into mechanical work in the case of (*a*) a reciprocating steam engine and (*b*) a steam turbine?

6. Given that the energy stored in the magnetic field produced by a coil wound upon a curtain ring is :—

$$B^2 . A . l \div 8\pi \text{ ergs}$$

where B = magnetic flux density in lines per sq. cm,
A = cross-section of the ring in sq. cms,
l = mean length of the magnetic path in cms.

Show that this may be written :—

$$\text{Energy stored} = \tfrac{1}{2} L . I^2 \text{ watt-seconds}$$

where L is the coefficient of self-induction in henries and I the current in amperes.

SECTION C. CHEMISTRY.

Examiner : W. R. COOPER, A.M.I.C.E., M.I.E.E.

(Not more than FOUR *questions to be answered.)*

1. Give the formulæ of the three oxides of iron, stating methods of preparation, properties, and any uses to which these oxides are put.

2. A brass has the composition 70 per cent copper and 30 per cent zinc. From one kilogramme of the brass how much crystallized copper sulphate, $CuSO_4 . 5H_2O$, and crystallized zinc sulphate,

$ZnSO_4 . 7H_2O$, could theoretically be obtained? And what is the weight of pure sulphuric acid equivalent to each of the two salts?

Atomic weights:—Cu = 63·6, Zn = 65·4, S = 32.

3. Describe the manufacture of bleaching powder and discuss its probable formula. To what are its bleaching and disinfecting actions due?

4. What is the "nascent state"? Give examples. What explanation is there of the greater chemical activity observed under this condition?

5. Carbon is burnt so that the products of combustion are CO_2 and CO in the proportion of nine molecules of the former to one molecule of the latter. If a metric ton (1 000 kg) of carbon is so burnt what weight of oxygen is theoretically required for the purpose? Convert this weight into litres at 0° C. and 760 mm pressure and give the equivalent volume of air, assuming the usual approximate composition. Results need be given only to three significant figures.

Atomic weight of carbon = 12.

6. What is meant by allotropy? Give examples and describe the preparation of any two bodies, one of which is an allotropic modification of the other.

Saturday, 30 April, 1921, 10 a.m.–1 p.m.

ELECTRICITY SUPPLY: GENERATION, TRANSMISSION, AND DISTRIBUTION.

(First Paper.)

Examiner: W. M. Selvey, M.I.E.E.

(*Time allowed: 3 hours.*)

(*At least* one *question to be attempted from each section, but not more than* seven *questions in all.*)

Section I.

1. What is meant by the expression "Coal of 12 000 B.Th.U.'s"?

Give a short but definite specification of a suitable coal for chain grate stokers of this heat value.

Would all coals of this heat value from any source be equally suitable?

2. Explain how considerable quantities of ashes can be handled with a minimum of labour.

Illustrate your remarks with simple sketches.

In what way would you estimate whether it was advisable to purchase a more expensive grade of coal if of greater purity? Give actual figures to illustrate your reply.

3. Give an outline sketch showing the section of any water-tube boiler well known to you, indicating the position of—

(*a*) Steam outlet from boiler to superheater with method of collection.

(*b*) Position of check valves and mode of operation.

(*c*) The nature of the water circulation which you would expect to get.

(*d*) The points of access you would desire for cleaning the exterior of the tubes.

4. What are the different types of feed pumps commonly used in power stations? State what preference you have, if any, for any one type, giving your reasons. Give a short account of the construction and operation of one of the types you mention.

Section II.

5. Select for explanation four of the following terms:

(*a*) Emergency governor.
(*b*) Oil relay.
(*c*) Hand-operated by-pass.
(*d*) Labyrinth gland.
(*e*) Dummy piston.
(*f*) Spherical seated bearing.
(*g*) "Michel" thrust.
(*h*) Flexible coupling.

And illustrate your explanation of one of them by a simple sketch.

6. Answer (*a*) or (*b*) but not both.

Give a simple outline sketch of—

(*a*) Blades, discs, and shaft of an impulsive type turbine.

(*b*) The rotor of a "reaction" type turbine.

In each case state briefly what methods are used to provide for the passage through the blades of the large volume of steam at the exhaust end of the turbine.

7. Give a short account of the ventilation system of an E.H.T. high-speed alternator.

What do you understand by the expression "hot spot"? How does the use of dusty air affect the operation of such a machine, and how may its effects be avoided?

8. An alternator when running has been tripped out under short-circuit, believed to be due to an internal fault. State the method of examination you would make. What faults might you expect to find easily? After the fault had been repaired what procedure would you adopt before passing the machine for service again?

9. Describe with sketches the essential features of a modern electrically-operated remote-controlled oil switch. What is a "reactance," and what is its function in this connection?

10. Describe the nature of and give some account of the method of laying a 0·15 square inch E.H.T. feeder in open ground. Refer particularly to jointing. If the voltage is 10 000, what is a reasonable rating in kilowatts for the feeder if the supply is for general power, lighting, and traction?

11. Give a list of the various devices you know of used for automatically cutting off and isolating any electrical conductor which develops a fault to earth. Describe particularly any one of them, stating under what circumstances it is most and least effective.

12. Describe briefly the principle of any well-known type of three-phase integrating wattmeter. What is a more correct description of this instrument ? What is an ampere-hour meter ? What is a practical alternative to the use of such a three-phase meter ?

Saturday, 30 April, 1921, 2.30 p.m.–5.30 p.m.

ELECTRICITY SUPPLY : GENERATION, TRANSMISSION, AND DISTRIBUTION.

(SECOND PAPER.)

Examiner : W. M. SELVEY, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. It is suspected with good reason that coal supplied to a power station is not up to specified or average quality. State clearly what technical procedure you would adopt to obtain confirmatory evidence—

(*a*) To put before the management.
(*b*) For discussion with the supplier or colliery.

2. What do you understand by the expression " percentage of CO_2 " ? How can an analysis of the flue gases reveal inefficient boiler operation ? What are reasonable values for—

Per cent CO_2 in flue gases.
Temperature of gases leaving boiler.
Temperature of gases leaving economizer.
Weekly boiler house efficiency.
Boiler efficiency on test.
Inlet temperature of feed water.
Draught at back of boiler.

3. Explain the expressions—

" Diagram " or " Rankine " efficiency of a turbine.
Available heat drop.
Thermodynamic efficiency.

A turbine operates on an available heat drop of 400 B.Th.U.'s. The heat supplied to the steam used is 1 280 B.Th.U.'s per pound. The boiler house overall efficiency is 70 per cent, the turbine efficiency is 72 per cent. The alternator efficiency is 96 per cent. Five per cent of the alternator output is absorbed in driving electrical auxiliaries. The coal supplied is of 12 000 B.Th.U.'s calorific value. All these figures are for steady full load.

Calculate (1 kWh = 3 412 B.Th.U.'s)—

(*a*) The steam consumption per kilowatt-hour generated.
(*b*) The coal consumption per unit sent out ; for continuous full-load operation.

How nearly would you expect the actual running results on an average power load to approach the value of (*b*), and what would the difference mostly be due to ?

4. What are the principal losses in an E.H.T. high-speed alternator ? Explain how the ventilating air can be used to obtain a fair knowledge of these. If 5 cubic feet per minute be allowed per kW of capacity, what would be a reasonable rise in the temperature of the cooling air—

Air density 13 cubic feet = 1 lb.
Air specific heat 0·24.

5. It is found that in a power system the capital costs per kW of maximum demand on the power station are £60 for the station and high-tension transmission system. The generation costs are 0·6d. per unit at the station. What would be a reasonable price to charge for a bulk supply at high tension—

(*a*) On a " fixed " plus a " running " charge basis ;
(*b*) For a group of similar consumers having a 20 per cent load factor on a " flat rate " basis.

Show clearly what you have allowed for.

6. Explain what measurements you would make, and what instruments you would require to determine the steam consumption of a turbo-generator.

7. Explain what measurements you would desire to make to discover the degree of heating of an alternator after a prolonged run or at any time. What are reasonable temperature rises for iron and copper parts ? What would be a dangerous rise for insulation composed partly of impregnated cotton ? What type of insulation is safest in this direction ? Why is the maximum temperature of greater importance than the rise of temperature ?

8. Explain how the speed of a turbo-generator is governed, and what is meant by inherent regulation of an alternator. What happens to the alternator voltage if full load is tripped off a generator ? How much of the resulting effect is explainable because the governor is not perfectly isochronous ? What apparatus is commonly fitted to control this effect, and how does it act ?

9. Describe all the operations gone through in starting up, running up to speed, and putting on the system of a complete turbo-alternator generating unit.

10. Give a short account of—

(*a*) An ironclad type switchboard ; or
(*b*) A sheet steel cubicle type switchboard.

This to be suitable for an ordinary industrial installation taking bulk supply at 3 000/6 000 volts, giving some direct supply to another board and providing also for transformation. Give in detail all the necessary instruments, trip coils, and all other accessories.

11. Explain some type of maximum demand indicator, and say under what circumstances you would consider its use essential.

12. Give a short account of the requisite duties of a substation engineer having under his direction high-tension switchgear, low-tension switchgear, rotary converters, motor converters. State what records should be kept and what weekly summary figures should be reported.

Saturday, 30 *April*, 1921, 10 *a.m.*–1 *p.m.*

ELECTRIC LIGHTING AND POWER.

(First Paper.)

Examiner : W. M. Selvey, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* seven *questions to be answered.*)

1. A consumer is offered power at £4 per annum per kVA of maximum demand plus 1·05 pence per unit metered. He proposes to install a motor to carry his estimated maximum demand of 300 B.H.P. The motor available has a power factor of 0·85 at full load. How many units will he require at 20 per cent load factor, and what will be his annual bill ? (1 H.P. = 746 watts.)

2. A consumer has an installation which cost £1 000. He finds his bill for power to be £1 095 in one year, for 175 200 units. He writes off £150 per annum for the cost of his installation. His maximum load is 100 kW. Calculate

(*a*) His load factor.
(*b*) The cost charged him per unit for power supplied.
(*c*) The total cost to him per unit, after adding installation charges.

3. Describe three different methods of charging for electricity, and say under what circumstances one is more convenient than another.

4. Explain the term " half-watt " lamp. Give what you consider a better descriptive title.

What is the particular difference between this and an ordinary filament lamp, and explain why a higher efficiency is obtained with the former.

5. Describe some form of shop window lighting, setting out the advantages as compared with other methods.

6. What do you understand by Flood lighting and Indirect lighting, and give instances where they are inapplicable and applicable.

7. Describe the function of a battery for use with an electric tramway supply, and give a method of control with its advantages.

8. Describe a form of " contactor " equipment for the control of a large electrical motor.

9. A pump is required to lift 60 000 gallons of water per hour through a height of 100 feet. Allowing 12 feet of head for pipe friction and pump efficiency 70 per cent, what current would be taken by a direct-current motor running on a 500-volt supply when driving the pump at the above duty ? Give the H.P. of the motor you would install, and assume a reasonable efficiency.

10. Describe a suitable system for wiring a house of six rooms, including for use of radiators in one sitting-room and one bedroom, and for a small electric cooker in the kitchen. Give sketches and approximate cost.

11. Describe one form of electric cooker suitable for an average family, including apparatus for boiling water in small quantities.

12. Describe the apparatus used in some form of electric welding system known to you. State some of the advantages and uses of electric welding.

Saturday, 30 *April*, 1921, 2.30 *p.m.*–5.30 *p.m.*

ELECTRIC LIGHTING AND POWER.

(Second Paper.)

Examiner : W. M. Selvey, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* seven *questions to be answered.*)

1. A consumer agrees to pay for a supply on a basis of one penny for the registration of one division on an ampere-hour meter which is adjusted to read units by assuming a constant voltage. How is his cost for true power affected by the power factor of his installation ?

If his power factor is 0·6 and his units so calculated are 1 200 000 per annum, how much will he save by installing a condenser to raise his power factor to unity ?

Explain clearly how a condenser improves this power factor, and give its necessary capacity in relation to the true power for a power factor of 0·6.

2. A house contains three living rooms, four bedrooms, kitchen, scullery, cellar and usual offices, and requires also one outside light.

The householder desires to use electricity extensively, but wishes for an estimate of the annual cost first.

He finds that the supply is 6d. per unit for lighting, and 1½d. per unit for heating and cooking. He now uses 12 tons of coal per annum at £2 a ton.

Prepare an estimate as to what you would recommend him to install, and calculate his annual electricity account for a reasonable use of the apparatus you recommend, tabulating the details.

3. The overall capital cost of a small undertaking is found to be £100 per kW of maximum load. It is desired that each consumer shall provide at least 20 per cent of this amount per kW demanded as annual revenue.

What would be a suitable tariff for a lighting load having a load factor of 10 per cent ?

Give also a suitable method of charging a small power consumer with a load factor of 25 per cent, so that he may be encouraged to increase his use.

4. Give a description of an electrical installation for a country house remote from any public supply.

5. A drawing office has to be well illuminated. It is 100 feet long by 40 feet wide. It has three rows of drawing tables, several tables, a head draughtsman's office and a passage way along one side. Give sketches of an illumination scheme and some idea of the intensity

and uniformity of the general illumination you would aim at. Show clearly any individual lighting you propose.

6. Give some account of improvements which have been made in modern searchlights. Give a sketch of a searchlight and state how the arc is kept steady.

7. Give some description of a modern electric battery vehicle. State its principal advantages and what hinders its general adoption for local transport.

8. Illustrate with sketches :—

(*a*) a three-phase liquid controller, or
(*b*) a three-phase oil immersed controller, with external resistances for a large induction motor.

State clearly what class of work requires this kind of apparatus.

9. What are reasonable speeds and efficiencies for :—

(*a*) 200 volt—2 H.P. D.C. motor.
(*b*) 440 volt—50 cycle—squirrel cage motor, with three pairs of poles—20 H.P. output.

A line of shafting in an engineering works is required to be belt driven at 80 R.P.M. It requires from 50 to 120 H.P. What size and speed of motor would you install on a 440-volt, 50-cycle, 3-phase supply for this purpose, and what would be the average efficiency of the drive ?

10. Describe some system of house wiring in which all insulation is covered by a completely earthed and conducting envelope.

Explain how this is made off at switches and fittings. Discuss its advantages and relative cost.

11. Describe carefully :—

(*a*) a domestic electric flat iron, or
(*b*) a hot plate, or
(*c*) a pint kettle.

It is desired to have a critical description, referring to the heating element, its method of insulation, its ease of replacement, the nature of the terminals and plug.

What are the common difficulties experienced with this class of apparatus when in careless and unskilled hands ?

12. Describe some modern development in the use of electricity for industrial or commercial purposes.

Saturday, 30 April, 1921, 10 a.m.–1 p.m.

TELEPHONY.

(First Paper.)

Examiner : T. F. Purves, O.B.E., M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* seven *questions to be answered.*)

1. (*a*) Explain the meaning of the following terms as applied to telephone engineering :—

Side tone.
Attenuation constant.
Characteristic impedance.
Sending allowance.
Standard cable.
Effective resistance.
Continuous loading.
Reflection.
Transmission efficiency.

(*b*) How is the transmission efficiency of a telephone circuit measured in practice ?

2. (*a*) Draw a simple diagram of a common battery subscriber's telephone set connected to a direct exchange line, and including the exchange termination of the circuit. Explain briefly the function of each portion of the apparatus.

(*b*) What would be the effect upon signalling, and upon speech, if the condenser in the subscriber's set were short-circuited ?

3. What are the main features of the " magneto," " common battery," and " common battery signalling " exchange systems ; what advantages can be claimed for each ; and what general considerations govern the choice of one of them for installation in a given telephone area ?

4. (*a*) Describe the various devices fitted upon the main distributing frame at a large exchange, and their functions.

(*b*) State how the protective apparatus on the frame would be affected if contact occurred between a subscriber's circuit and a power circuit at a potential of (1) 200 volts, and (2) 600 volts.

(*c*) What is the purpose of an intermediate distributing frame ?

5. (*a*) State the nature of, and the reason for, any special features associated with the charging generator employed in an exchange equipped with a single main battery.

(*b*) Describe the power board and the usual method of power distribution in a large exchange.

6. What arrangements are made in large exchanges to prevent the operation of the signals associated with subscribers' " answering " jacks when out-going connections are made in the " multiple " ? How does an operator determine whether or not a circuit is engaged, and how is the counting of calls effected ?

7. Given a map of a telephone area showing the distribution of present and prospective subscribers, how would you proceed to determine the most economical position for a new telephone exchange, assuming that the purchase prices of all building sites in the area are uniform ?

8. An overhead pole line requires renewal, and you are provided with estimates of the total numbers of circuits which will be required on the route at the ends of five-yearly periods. What factors have to be considered in order to determine whether the existing plant should be replaced by (*a*) another pole line, or (*b*) an underground line ?

9. (*a*) What is the object of "balancing" long telephone cables?

(*b*) Describe briefly the principles of present methods of balancing loaded cables.

(*c*) What precautions are necessary in the maintenance testing of loaded telephone cables in order to preserve the transmission efficiency of the cable?

10. Describe the principles of high frequency "carrier wave" telephony, and discuss its suitability for practical use in this country and elsewhere.

11. Calculate the wire to wire capacity per nautical mile of a loop in a submarine telephone cable, given the following constants:—

Diameter of conductor = 93 mils.
Thickness of dielectric = 93 mils.
Dielectric constant (K) = 2·86.

The formula for the capacity of a single core cable is

$$C = K \frac{L}{2 \log_{\epsilon} \frac{r'}{r}} \times \frac{1}{900\,000}$$

where C = capacity in microfarads,
L = length of cable in centimetres,
r = radius of conductor,
r' = radius of core.

12. A telephone line, 100 miles in length, with uniform leakance, and having a characteristic resistance of 1 000 ohms, has 10 volts (D.C.) applied at one end, the other end being closed through a resistance of 1 000 ohms. Calculate the received current when the attenuation constant per mile is 0·012.

(Base of Napierian logs: $\epsilon = 2{\cdot}7183$.)

Saturday, 30 April, 1921, 2.30 p.m.–5.30 p.m.

TELEPHONY.

(Second Paper.)

Examiner: T. F. Purves, O.B.E., M.I.E.E.

(*Time allowed: 3 hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. Show by sketches the speech transmission circuits usually employed in this country on "local" and "junction" switchboards in common battery exchanges. What are the advantages and disadvantages of the various systems?

2. (*a*) Describe in detail the operation of a keyless order wire junction between two common battery exchanges.

(*b*) Draw a skeleton diagram of a typical cord circuit on an "answering position" at a common battery exchange, showing all the items of apparatus required.

3. (*a*) Describe the general principles of (1) machine driven, and (2) "step by step" automatic telephone exchange systems. (*b*) Which of the foregoing principles is best suited to an exchange system in a very large area, and why?

(*c*) What are the economic advantages or disadvantages of automatic systems, as compared with manual systems, in (1) large area and (2) small towns?

4. (*a*) Describe the manner in which the supervisory signals at the main exchange and at a private branch exchange (common battery working) are controlled, when one of the private extensions is connected to a subscriber on the main exchange.

(*b*) What arrangements are made in order to afford exchange service to a private extension station at night, when there is no operator in attendance at the private exchange?

5. (*a*) Describe a transformer suitable for working a superposed telephone channel on telephone cable circuits.

(*b*) Give a diagram showing how the superposed circuit is formed from two physical circuits.

(*c*) Give a diagram of a method of superposing a duplex Morse sounder set on a trunk telephone circuit.

6. (*a*) Describe briefly the principle of the operation of the three-electrode thermionic valve, and show how such valves can be applied to the amplification of speech, in both directions, in a telephone circuit.

(*b*) State the principle of the "four-wire" telephone repeater; what advantage does it possess over the two-wire repeater circuit?

7. Make a skeleton floor plan of an exchange installation for 5 000 subscribers in a multi-exchange area, showing the relative positions of the apparatus racks, power board, and switchboard, and indicating the cable runs and the usual sizes and types of cable employed. (Assume that the apparatus room and the exchange switchboard are on the same floor.)

8. (*a*) Describe a system of combined underground and aerial distribution, comprising "block wiring" and "distributing poles."

(*b*) Describe the treatment of the main cables entering a large exchange, as far as the "main distributing frame."

9. A pole line carrying 48 copper wires, each weighing 150 lb. per mile and having a breaking weight of 450 lb., forms at one point an angle of 120°. The lengths of the spans on each side of the angle are equal. The inward pull is to be balanced by stay wires, each having a breaking weight of 10 000 lb. The height, from the ground line to the point of attachment of the stay wires on the pole, is 4 times the spread of the stay on the ground line. Calculate the number of stay wires required to make the factor of safety of the stay equal to the factor of safety of the line wires. (Sin 30° = 0·5.)

10. State the factors which you would take into consideration in planning the lay-out of outdoor telephone plant, to meet present and prospective requirements in a given telephone area, assuming that you are provided with estimates of probable development.

In what circumstances would you decide upon the adoption of:—

(*a*) An overhead system,
(*b*) A combined underground and overhead system,
(*c*) A wholly underground system.

11. (*a*) Calculate the real part of the complex attenuation constant

$$\beta + j\alpha = \sqrt{(R + j\omega L)(G + j\omega C)}$$

of a circuit having the following constants per loop mile :—

Resistance ; $R = 44$ ohms.
Inductance ; $L = 0{\cdot}001$ henry.
Capacity ; $C = 0{\cdot}065$ microfarad
Leakance ; $G = 1$ micromho.
$\omega = 2\pi f = 5\,000$.

(*b*) Calculate the characteristic impedance, with its angle, of the same circuit.

(*c*) What is the general effect on the magnitude and angle of the characteristic impedance of such a circuit when it is loaded.

12. (*a*) A cable pair having constants per mile loop :—

Resistance ; $R = 44$ ohms,
Inductance ; $L = 0{\cdot}001$ henry,
Capacity ; $C = 0{\cdot}065$ microfarad,
Leakance ; $G = 1$ micromho,

is loaded at intervals $D = 2{\cdot}5$ miles by coils having an inductance $L_1 = 0{\cdot}132$ henry. The effective resistance of the coils is $R_1 = 6{\cdot}6$ ohms per coil. The natural inductance may be neglected and the formula for the attenuation constant taken as :—

$$\beta = \frac{R + \left(\frac{R_1}{L_1} + \frac{G}{C}\right)\frac{L_1}{D}}{2}\sqrt{\frac{C}{\left(\frac{L_1}{D}\right)}}$$

What is the attenuation constant per mile loop ?

(*b*) What is the total inductance which can be introduced into such a circuit to produce a minimum attenuation constant ?

Saturday, 30 *April*, 1921, 10 *a.m.*–1 *p.m.*

APPLICATION OF ELECTRICITY TO MINES.

(First Paper.)

Examiner : W. H. Patchell, M.Inst.C.E., M.I.Mech.E., M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* seven *questions to be answered.*)

1. What do you understand by the ventilation of a motor and how it is generally effected ? What precautions are necessary in connection therewith ?

2. What different precautions are necessary in running d.c. and a.c. circuits ?

3. What is the difference between the electrical efficiency and the commercial efficiency of a generator ?

4. Draw a diagram of connections for two compound wound dynamos in parallel.

5. How do you account for modern d.c. machines working with fixed position of brushes while earlier machines required the brush position to be changed for different loads ?

6. What is the use of a tail rope in winding ? Draw typical load-diagrams of winding with and without a tail rope.

7. What types of balancers are available for 3-wire d.c. systems ? Illustrate by diagram of connections.

8. Contrast 3-wire d.c. and a.c. for colliery working. Give your reasons for your selection in a definite case.

9. If you had to supply 230 e.h.p. $1\frac{1}{2}$ miles away from a d.c. 1 000-volt generator with $7{\cdot}5$ per cent loss on the lines, state size and weight of copper conductor required.

10. Define " Dry Cell," " Concentric Wiring," " Dielectric," " Capacity of a Cable," " Capacity of Accumulator," " Potentiometer."

11. Explain $A = \dfrac{B \times D}{48 \times E \times V \times F}$

when B is B.H.P. of motor, E is volts, F is power factor, V is volts drop in the line, and D is length of line. A is area of conductor.

12. Discuss type and installation of shaft cables.

Saturday, 30 *April*, 1921, 2.30 *p.m.*–5.30 *p.m.*

APPLICATION OF ELECTRICITY TO MINES.

(Second Paper.)

Examiner : W. H. Patchell, M.Inst.C.E., M.I.Mech.E., M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* seven *questions to be answered.*)

1. What is the electrolysis of conductors ? When is it likely to occur ?

2. Contrast the use of tallow candles, oil safety lamps and electric safety lamps for use at the coal face.

3. What are your ideas as to the ignition of coal-gas by electric arcs ?

4. A fan is exhausting 6 180 000 cubic feet of air per hour at 1 inch W.G. at 150 r.p.m. with 29 e.h.p. motor input. What is the overall efficiency ? What speed would be necessary to set up 4 inch W.G. ?

5. How should the electric energy be delivered to coal-cutting machines ? What type of motor would you preferably employ ? State your reasons.

6. Sketch a motor starting box with double-pole switch, fuses, and starting resistance for d.c., showing connections.

7. What is the use of cable suspenders in underground roads ? What types are commonly employed ?

8. Draw speed, torque, and power diagrams, with explanatory letters, for a winding engine with parallel drum.

9. Contrast cascade control and rheostatic control for varying the volume of air given by a motor-driven fan.

10. How would you arrange the electric drive for 3-throw and centrifugal pumps respectively ?

11. What do you understand by the following :—

$$\frac{V \times A \times \cos\phi \times 100 \times 2}{746 \times E} \quad \text{and} \quad \frac{\text{B.H.P.} \times 746 \times 100}{V \times E \times \cos\phi \times 1{\cdot}73}$$

12. State generally the requirements to be observed in any part of a mine in which inflammable gas is likely to occur in quantity sufficient to be indicative of danger.

Saturday, 30 April, 1921, 10 a.m.–1 p.m.

MANUFACTURE OF ELECTRIC MACHINERY (INCLUDING WORKS MANAGEMENT).

(First Paper.)

Examiner : A. G. Ellis, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. A factory manufacturing generators, motors, transformers and switchgear buys the following classes of material in large quantities—

(*a*) Iron and steel castings.
(*b*) Electrical sheet steel.
(*c*) Copper.

State concisely the conditions you would include in purchasing specifications to which materials in each class are bought, and describe the arrangements you would make for testing their compliance with the specifications.

2. Describe briefly and give the approximate cost of the equipment required for the E.H.T. test room of a factory manufacturing switchgear for working voltages up to 110 kilovolts. How would you regulate and measure the extra-high-tension voltage ?

3. What insulating materials are in common use in rotating electrical machinery ?

State briefly their several properties with special reference to the uses to which they are put.

4. It is proposed to put down a factory for the manufacture of transformers up to 1 000 kVA in size and for a yearly output of 250 000 kVA approximately.

Describe how you would lay out such a factory, giving a sketch of the arrangement of the different departments and approximate dimensions of space required.

State also the height of buildings required, crane capacity, approximate total number of hands, capital involved, and total amount of work on the floor of the shop, in tons and kVA.

5. A large electrical machine factory has available a power supply of 3-phase current at 440 volts, 25 cycles, and alternatively 220 volts, direct current.

State what system you would employ for driving the factory, giving reasons and a description of the general system of driving you would recommend.

6. What special constructional features have to be attended to in electrical machinery for—

(*a*) Collieries.
(*b*) On board ship.
(*c*) Outdoor substations.

Describe as far as possible, with sketches, how these conditions are met.

7. What are the component factors which make up the selling price of electrical machinery ?

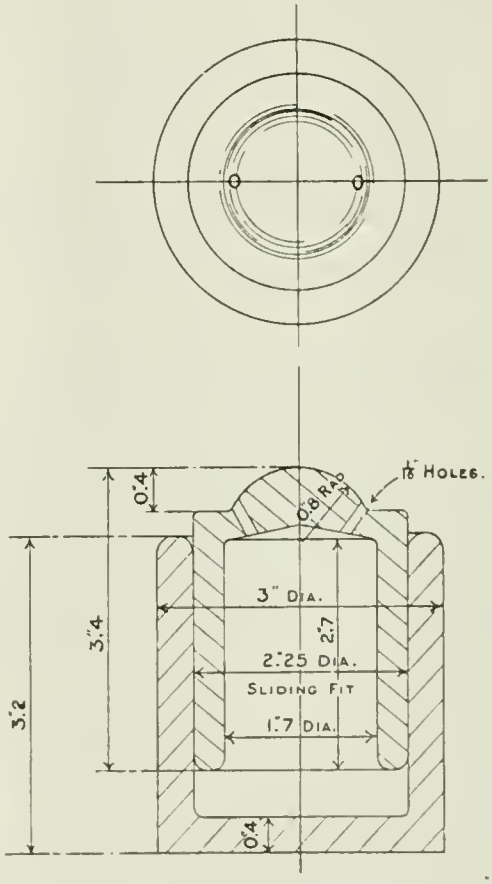
Show how the price of any particular piece of apparatus you name is arrived at, and give approximate figures for the relative value of the different components in the price.

How would you take account of the frequent variation in the market price of materials ?

8. What systems of welding iron and steel are available commercially ?

Describe one of these processes.

In what parts of electrical machinery can welded work be used with advantage ?



9. Describe the working of a limit gauging and inspection system in an electrical works.

10. What are the limiting sizes and weights of electrical machinery that can be transported by—

(*a*) Road.
(*b*) Rail.

How would you handle, transport and erect either—

(*a*) A 25 000-kW 1 500-r.p.m. 3-phase turbo-generator or
(*b*) A 20 000-kVA 22 000-volt 50-period 3-phase transformer ?

11. What difficulties are met in the manufacture of large commutators ?
Describe how these are overcome.

12. The attached sketch illustrates an oil dashpot and plunger. Describe what you consider the cheapest method of manufacturing these in large quantities, in steel or wrought iron, and give an analysed estimate of the cost.

Saturday, 30 *April*, 1921, 2.30 *p.m.*–5.30 *p.m.*

MANUFACTURE OF ELECTRIC MACHINERY (INCLUDING WORKS MANAGEMENT).

(SECOND PAPER.)

Examiner : A. G. ELLIS, M.I.E.E.

(*Time allowed :* 3 *hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. Describe the general organization of a large electrical factory manufacturing any classes of apparatus you name.
What are the functions of the Standards and Research Departments, and their relation to the general organization ?

2. What considerations determine the thickness of electrical sheet steel used in electrical machinery and transformers ?
What are the commercial advantages of annealing finished punchings for the above apparatus ?
What methods would you adopt to minimize the amount of scrap from punchings ?

3. A factory manufacturing one hundred motors per week, of average size 5 h.p. and maximum size 25 h.p., is putting down a test room. Give a general specification of the plant required for either A.C. or D.C. motors, with sketches of the arrangements, also stating the personnel required.
What regular tests would you make ?

4. A factory purchases in large quantities fibrous insulating materials, in sheets and cloths, varnish and insulating oils.
State concisely the conditions you would include in purchasing specifications to which materials in each class are bought, and describe the arrangements you would make for testing their compliance with the specifications.

5. What is the best method of insulating stator windings for turbo-alternators up to 11 000 volts ?
What are the difficulties met with and how are they to be overcome ?

6. Describe the manufacturing arrangements you would make for the mass production of drum controllers, giving short descriptions, with sketches, of the tools involved.

7. Describe, with sketches, a machine for taping armature and field coils and state your views on hand versus automatic taping.

8. Where do you consider enamel-covered copper wire can be advantageously employed in electrical machinery and apparatus ?
Give reasons for your statements.

9. Describe fully a method of manufacturing unjointed metal formers for the moving coils of D'Arsonval instruments, giving rough sketches of the tools employed.

10. Describe with sketches an automatic winding machine for the winding of shunt coils of electricity supply meters, giving special attention to the method of insulating between layers.

11. Describe in detail methods of balancing the movement of an electrical measuring instrument.

12. Describe in detail a method of manufacturing and insulating the commutator of a D.C. ampere-hour meter.

Saturday, 30 *April*, 1921, 10 *a.m.*–1 *p.m.*

DESIGN OF ELECTRICAL MACHINERY AND APPARATUS.

(FIRST PAPER.)

Examiner : J. K. CATTERSON-SMITH, M.I.E.E.

(*Time allowed :* 3 *hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. (*a*) What are the principal conditions which must be satisfied if a shunt-wound dynamo is to be self-exciting when on load ?
(*b*) How would you ensure the satisfactory operation in parallel of a number of shunt-wound dynamos ?
(*c*) Why is an equalizing connection used when a number of compound-wound dynamos are running in parallel ?

2. What percentage rise in voltage at the terminals would you expect when full load is thrown off a 150-kW 250-volt shunt-wound generator, running at constant speed, for which the following particulars are given :

(*a*) No-load magnetization data :—

Field amp.-turns per pole	2 000	4 000	6 000	8 000	10 000
Armature volts ..	118	200	250	275	295

(*b*) Armature winding :—
180 coils, each having 2 turns, connected 4-pole lap. Resistance 0·012 ohm.

3. Plot the efficiency curve of a 500-volt 80-B.H.P. motor which when tested at no-load gave the following results :—

(*a*) Input 6 amperes, running light at normal speed and voltage.
(*b*) Armature resistance (hot) 0·20 ohm.

Criticize this method of deducing the full-load efficiency and suggest methods of increasing its accuracy.

4. Describe with diagrams either a cascade induction motor combination or a cascade motor-converter.

Explain the action and show that the synchronous speed of the set is given by :—

$$\text{R.P.M.}_{\text{syn}} = 120 \times f_1 \div (p_1 + p_2)$$

where f_1 is the line frequency and p_1 and p_2 are the number of poles in the two machines.

5. (*a*) Show that the following relation holds for the conductor in any magnetizing coil :—

resistance per foot
= volts across coil ÷ L.M.T. × amp.-turns

where L.M.T. is the mean length of one turn in feet.

(*b*) Calculate a suitable field winding for a 4-pole, 220-volt, shunt motor to give 4 500 ampere-turns per pole given that the poles are 5 inches in diameter and the length of the winding is not to exceed 4·5 inches.

6. Describe with sketches one type of phase-advancer for improving the characteristics of polyphase induction motors. Use vector diagrams to explain the action.

7. What current would be drawn from a 440-volt d.c. power circuit by a motor coupled to a gear-driven pump which requires a torque of 200 lb.-feet at 150 R.P.M. assuming reasonable efficiencies for the motor and gear ?

8. Give an account of the means employed to minimize the short-circuit current of large turbo-alternators and of the construction adopted to ensure freedom from breakdown of the stator windings. How do the armature and field currents vary during short-circuit ?

9. The output of an armature is given by :—

$$\text{kW} = \text{D}^2 \times \text{l} \times \text{R.P.M.} \times k$$

assuming a suitable value for the output coefficient (k) give the leading dimensions for the armature core of a 500-volt d.c. generator to develop 50 kW at 500 R.P.M.

10. Give a diagram of connections and explain the action of either :—

(*a*) a differential compound connected three-wire balancer, or
(*b*) a static balancer for connection to three slip-rings on a d.c. generator.

11. Describe with sketches and diagrams ONE of the following :—

(*a*) an oil switch for large power H.T. circuits,
(*b*) an a.c. watt-hour meter,
(*c*) an automatic voltage regulator for field circuits,
(*d*) a protective system for a.c. power circuits.

12. Why is it found economical to employ the more expensive steel alloys for the magnetic circuits of electrical apparatus ?

Saturday, 30 April, 1921, 2.30 p.m.–5.30 p.m.

DESIGN OF ELECTRICAL MACHINERY AND APPARATUS.

(SECOND PAPER.)

Examiner : J. K. CATTERSON-SMITH, M.I.E.E.

(*Time allowed : 3 hours.*)

(*Not more than* SEVEN *questions to be answered.*)

1. A paper-insulated cable is to be designed for 100 amperes at 50 000 volts, 50 cycles. The maximum dielectric stress on the insulation ($V \div a \,.\, \log_e b/a$) is not to exceed 40 kilovolts per centimetre. Give a dimensioned sketch of the cross-section of the cable you would propose.

2. Calculate the resistance required for a heater element to raise the temperature of one gallon of water 50 degs. Cent. in five minutes when working on a 220-volt supply. You may assume that all the heat is transferred to the water and no loss by radiation. What will be the cost per gallon of water heated if the energy is obtained at 2d. per unit ?

Note.—1 calorie = 4·20 joules,
1 B.Th.U. = 778 foot-lb.,
1 gallon of water = 10 lb.,
1 lb. = 453·6 grams.

3. In what way would you drive a 2 000-kW direct-current generator from a steam-turbine running at 3 000 R.P.M. ? State what you consider to be a suitable diameter and length of armature core for this dynamo.

4. What are the principal causes of harmonics in the E.M.F. wave-form of three-phase alternators and what steps are taken to minimize these harmonics so that the wave-form may approximate closely to a sine curve ?

5. What is meant by the "synchronizing power" of alternators working in parallel ? Show that the "synchronizing torque" is proportional to the displacement of the rotor and that it exerts an accelerating action upon the lagging alternator. Under what conditions may this action lead to failure in parallel working ?

6. How does sparking at the brushes arise in single-phase a.c. commutator motors ? What are the principal differences in this respect between repulsion motors and series motors ?

7. Describe and criticize the methods of varying the voltage of rotary converters in which the following apparatus is used :—

(*a*) Reactance control.
(*b*) Booster control.
(*c*) Induction regulator control.

8. Give the leading dimensions for the design of an oil-cooled single-phase 50-cycle 2 000/200-volt transformer the full-load output of which is to be 100 kW at a power factor of 0·85.

9. Why are d.c. machines fitted with interpoles? How would you decide upon the iron section and the ampere-turns excitation for an interpole?

10. Give a diagram of connections for either :—

(*a*) A machine for converting single-phase to three-phase power, or

(*b*) A bank of transformers for transforming from two-phase to three-phase power.

11. What number of conductors is required in the stator winding of a 2 200-volt three-phase alternator to run at 250 R.P.M. and having 24 poles, if the magnetic flux is to be about 5×10^6 c.g.s. lines per pole?

12. Briefly describe a method of finding the cost of manufacture of any piece of electrical apparatus such as a transformer or a measuring instrument.

ASSOCIATE MEMBERSHIP EXAMINATION.

(FOR OFFICERS OF THE ROYAL CORPS OF SIGNALS.)

SEPTEMBER, 1921.

Friday, 23 *September*, 1921, 10 *a.m.*–1 *p.m.*

THE THEORY OF ELECTRICAL MILITARY SIGNALLING.

PAPER I. (*Time allowed : 3 hours.*)

NOTE.—*Not more than* EIGHT *of the following questions are to be attempted.*

1. If $y = x^2 - ax + b$, find the value of x which makes y a minimum.

Draw a rough graph of the curve $y(x^2 + 4) = 2x$.

2. The weights of the rubber and copper in an insulated wire are 100 lb. and 200 lb. per mile respectively. The specific gravity of the rubber is 0·975 and the specific gravity of the copper is 8·90; what is the ratio of the diameter of the copper conductor to the diameter of the rubber covering?

3. Describe what is meant by a molecule, an atom, an electron and an ion. Describe also what is meant by ionization.

Illustrate diagrammatically the attraction or repulsion between electrons and positive or negative ions.

4. State Ohm's law for the case of a homogeneous conductor carrying a current.

A cell has an E.M.F. of 1·4 volts and an internal resistance of 0·5 ohm. It is connected up to a circuit made up of two wires in parallel whose resistances are 1 ohm and 2 ohms respectively. Find the current in each wire; also compare the rates of production of heat in the wires.

5. Describe a service pattern cell. What is the electrical difference between a large and a small Leclanché cell?

Give a method of measuring the internal resistance of a cell.

6. The capacities of two condensers, the insulation resistance of each of which is infinite, are 3 and 5 microfarads respectively. They are put in series across the 100 volt D.C. mains. Find the number of coulombs in each.

Find also the difference of potential across their terminals.

7. Draw a diagram of the electrical connections of an induction coil and give the theory of its action.

Give the precautions to be used in practical work.

8. Explain with the assistance of sketches the construction of a telegraphic relay (*a*) in its simplest form, (*b*) in the form known as a polarized relay. Explain its use in telegraphy.

For high-speed working it is usual to put in series with the relay a condenser shunted with a non-inductive resistance. Why is this done?

9. What are the advantages of double-current working in land-line telegraphy? Give diagrams of connections for an end station worked (*a*) simplex, (*b*) differential duplex.

10. A partial earth-fault is known to exist on an overhead wire between two stations A and B. The resistance of the wire from A to B is x, the resistance from A to earth when B is insulated is a, and when B is put to earth the resistance is b. Find the resistance from A to the fault.

11. Describe a magneto-generator used in telephony.

Give a diagram of a line suitable for simultaneous telegraphy and telephony.

12. Explain what is meant by, and the theory of the action of, a "phantom" circuit in telephony. Sketch the arrangement of the circuits in a telephone cable, by which four wires are used for three independent circuits, two physical and one phantom.

Saturday, 24 *September*, 1921, 10 *a.m.*–1 *p.m.*

THE THEORY OF ELECTRICAL MILITARY SIGNALLING.

PAPER II. (*Time allowed : 3 hours.*)

NOTE.—*Not more than* EIGHT *of the following questions are to be attempted.*

1. Draw a graph to indicate the variation of the velocity with the time when a particle moves in a straight line with uniform acceleration, and show how to obtain from the graph the space described in any time.

If a runner can start with a velocity of 20 feet per

second and run with uniform acceleration, find the greatest speed attained in running the 100 yards in 10 seconds, and compare the necessary acceleration with that of gravity.

2. Calculate the relation between the kilowatt-hour, the horse-power-hour and the foot-pound.

Find the cost of hoisting 50 tons 40 feet high by means of an electric motor, if the price of energy is $2\frac{1}{2}$d. per unit. The combined efficiency of the motor and gearing may be taken as 65 per cent.

3. Describe armature reaction (1) in a direct-current generator, (2) in a D.C. motor, and explain to what it is due.

What causes sparking in a D.C. machine? Describe methods of remedying this defect.

4. What are the essential parts of a dynamo to be driven by a small petrol engine to charge accumulators?

Draw roughly the characteristic of such a machine.

5. Describe practical methods of measuring (1) wave length, (2) inductance, and (3) coupling.

6. A condenser and an inductive coil are put (1) in series and (2) in parallel with one another in a given circuit. Find the conditions for resonance.

If a coil whose resistance is R and inductance L is shunted by a condenser of capacity C and placed in an A.C. circuit the frequency of the current in which is f, draw a diagram giving the magnitudes and the phases of the currents.

For what value of the capacity is the ratio of the current in the inductive coil to the current in the main a maximum?

7. Describe the production of loops and nodes of potential in a conductor by high-frequency electromotive force.

8. Explain briefly the principles of radio telephony. Describe the receiving circuit. By what different general methods can the voice modulations be applied to a radio telephone transmitter?

9. Describe some form of crystal detector and explain its action.

Why are crystal detectors being replaced by valve detectors?

10. Describe a plate current grid volt characteristic of a valve.

What is the effect on the characteristic of increasing (1) the applied H.T. voltage, (2) the filament temperature?

11. What do you understand by auto-heterodyne and separate heterodyne? What are the special advantages and disadvantages of each?

12. It is found that a bent antenna emits a less intense radiation at any given distance in the direction in which the free end points than in the opposite direction. Give a rough explanation of the reason of this.

Draw a diagram to illustrate the magnitude of the effect produced.

Associate Membership Examination Results.

April, 1921 (Supplementary List; see "Institution Notes," No. 27, Page 26, June, 1921).

Passed in whole Examination.

Ker, D. W. (Buenos Aires).
Waters, W. A. (New Zealand).

September, 1921 (For Officers of Royal Corps of Signals).

Passed in the Theory of Electrical Military Signalling.

Bell, A. B., Captain (Yorkshire Regt.).
Collings, J. L., Lieut. (Gordon Highlanders).
Daws, H. A., Lieut. (North Staffs. Regt.).
Duvivier, J. J., Lieut. (R.G.A.).
Giblin, R. G. L., Lieut. (R.F.A.).
Goddard, L. D., Lieut. (R.F.A.).
Kennett, B. B., Lieut. (R.G.A.).
King, Q. E. M. A., Lieut. (R.F.A.).
Levett, E., Lieut. (Royal Sussex Regt.).
Morgan, C. L., Lieut. (11th Hussars).
Mould, R. H., Lieut. (R.F.A.).
Ogden, G. W., 2nd Lieut. (Queen's Royal Regt.).
Pope, F. T., Lieut. (Queen's Royal Regt.).
Sharp, C. A., 2nd Lieut. (Worcester Regt.).
Spencer, H. A., Lieut. (R.F.A.).
Thornes, J. N. B., Lieut. (R.F.A.).
Woodbridge, R. C., Lieut. (R.F.A.).
Wynn, J., Lieut. (Hampshire Regt.).

Accessions to the Reference Library.

Aitken, W. Automatic telephone systems. vol. 1, Circuits and apparatus as used in the public services. 4to. 297 pp. *London*, 1921

British Engineering Standards Association. [Publications]. no. 4, 32, 101, 102, 109, 110, 115, 133, 144. 8vo. *London*, 1921

4, Dimensions and properties of British standard channels and beams for structural purposes.
32, British standard specification for steel bars for the production of machined parts.
101, British standard specification for tramway tyres.
102, British standard specification for tramway axles.
109, British standard specification for air-break knife switches and laminated brush switches for pressures not exceeding 660 volts (excluding totally enclosed and flame-proof types).
110, British standard specification for air-break circuit breakers for pressures not exceeding 660 volts (excluding totally enclosed and flame-proof types).
115, British standard specification for metallic resistance materials for electrical purposes.
133, British standard specification for normal type vacuum tungsten filament electric lamps.
144, British standard specification for creosote for the preservation of timber.

Federal Electric Railways Commission. Proceedings, held in Washington, D.C., July-October, 1919, together with Final Report of the Commission to the President. 3 vol. 8vo. *Washington*, 1920

Gibson, A. H., *D.Sc., editor.* Hydro-electric engineering. vol. 1, Civil and mechanical. Contributors: H. D. Cook and the editor. 1a. 8vo. 242 pp. *London*, 1921

GRANT, L. C. The steam turbo-alternator.
8vo. 276 pp. *London*, 1921

GRARD, C. Aluminium and its alloys. Their properties, thermal treatment and industrial application. Translated by C. M. and H. W. L. Phillips.
8vo. 207 pp. *London*, 1921

HALER, P. J., and STUART, A. H. An introduction to physics for technical students.
sm. 8vo. 240 pp. *London*, 1921

HOME OFFICE. Welfare pamphlet no. 7, Lighting in factories and workshops.
8vo. 18 pp. *London*, 1921

IBBETSON, W. S. The kinema operator's handbook. Theory and practice.
sm. 8vo. 158 pp. *London*, 1921

—— Practical electrical engineering for elementary students. sm. 8vo. 167 pp. *London*, 1910

INTERNATIONAL ELECTROTECHNICAL COMMISSION. Publications. nos. 8, 31, 32. 4to. 1911–20
8, Report of the Unofficial Conference held at Brussels, August, 1910.
31, Special Committee on the Rating of Electrical Machinery. Résumé of meeting of delegates. Paris, May, 1919.
32, Advisory Committee on the Rating of Electrical Machinery. Résumé of meeting of delegates. London, October, 1919.

KENNELLY, A. E., HUNTER, R. N., and PRIOR, A. A. Oscillographs and their tests. [Massachusetts Institute of Technology, Publications, Electrical Engineering Research Division, No. 22].
8vo. 45 pp. [*Boston, Mass.*], 1920

LANCASHIRE & CHESHIRE COAL RESEARCH ASSOCIATION, THE. Bulletin 10, The determination of the calorific value of coal. By F. S. Sinnatt and M. B. Craven. sm. 8vo. 23 pp. [*Manchester*], 1921

LEE, J. The economics of telegraphs and telephones.
8vo. 93 pp. *London*, [1913]

LEWES, V. B. Liquid and gaseous fuels, and the part they play in modern power production. 2nd ed., revised and edited by J. B. C. Kershaw.
8vo. 367 pp. *London*, 1921

LOEB, J. Electrification of water and osmotic flow. [Reprinted from Science, n.s., vol. 53, no. 1361, Jan. 28, 1921]. 4to. 7 pp. *n.p.*, 1921

—— Ionic radius and ionic efficiency. [Reprinted from The Journal of General Physiology, July 20, 1920, vol. 2]. la. 8vo. 13 pp. *n.p.*, 1920

—— On the cause of the influence of ions on the rate of diffusion of water through collodion membranes. I. [Reprinted from The Journal of General Physiology, March 20, 1920, vol. 2].
la. 8vo. 22 pp. *n.p.*, 1920

—— The reversal of the sign of the charge of membranes by hydrogen ions. [Reprinted from The Journal of General Physiology, May 20, 1920, vol. 2]. la. 8vo. 18 pp. *n.p.*, 1920

MCKINNON, E. C. The A.B.C. of storage battery management. 8vo. 95 pp. *London*, 1920

MINISTRY OF TRANSPORT. Tramways and light railways laid on public roads. Memorandum regarding details of construction of new lines and equipment.
sm. fol. 4 pp. [*London*], 1921

MONTORIOL, E. Appareils et installations télégraphiques. Préface de [A.] Blondel.
8vo. 631 pp. *Paris*, 1921

PARRY, E. On a theory of fluid friction and its application to hydraulics. [Reprinted from "The English Electric Journal," Oct., 1920].
4to. 19 pp. *London*, 1920

—— The electrical properties of three-phase transmission lines. [Reprinted from "The English Electric Journal," April, 1920].
4to. 16 pp. *London*, 1920

RAILWAY ASSOCIATION OF CANADA, THE. Report of Sub-Committee—Inductive interference.
8vo. 29 pp. [*Montreal*, 1920]

REIS, PAUL. Das Telephon und sein Anrufapparat nach seiner historischen Entwickelung und seiner praktischen Anwendung. Vortrag in Mainzer Vereinen im Nov. und Dez. 1877.
8vo. 52 pp. *Mainz*, 1878

REIS, PHILIPP. Philipp Reis, der Erfinder des Telephons. Sonderabdruck aus dem Jahresbericht des Physikalischen Vereins zu Frankfurt a.M. 1897/98. 8vo. 30 pp. *Frankfurt a.M.*, 1899

SEXTON, A. H. Fuel and refractory materials. New edition completely revised and enlarged by W. B. Davidson. 8vo. 382 pp. *London*, 1921

SINNATT, F. S. The present position of smokeless fuels. [Excerpt from vol. 42, no. 1, 1921, of The Journal of the Royal Sanitary Institute].
8vo. 5 pp. *n.p.*, [1921]

SMITH, C. F., *D.Sc.* The testing of continuous current machines. The main principles and practice of efficiency and output tests on c.c. generators, motors, and motor-generators. With examples from practice. sm. 8vo. 112 pp. *London*, 1921

SNELL, *Sir* J. F. C. Power house design. 2nd ed.
8vo. 546 pp. *London*, 1921

SQUIER, *Maj.-Gen.* G. O. Tree telephony and telegraphy. [Reprinted from the Journal of the Franklin Institute, June, 1919].
la. 8vo. 31 pp. *n.p.*, 1919

SULLIVAN, H. W. Working diagrams of valve amplifying receiver circuits.
obl. 8vo. 24 pp. *London*, 1920

THORROWGOOD, W. J. Lecture on automatic train control. November 30, 1920. [The Permanent Way Institution].
8vo. 13 pp. *Nottingham*, 1920

UNITED STATES. DEPARTMENT OF COMMERCE: BUREAU OF STANDARDS. Miscellaneous publications, no. 41, Weights and measures. 12th Annual conference of representatives from various states held at the Bureau of Standards, Washington, D.C., May 21–24, 1919.
la. 8vo. 226 pp. *Washington*, 1920

VONWILLER, O. U. Notes on the elastic properties of selenium. [Reprinted from the Journal and Proceedings of the Royal Society of New South Wales, vol. 53]. 8vo. 8 pp. *n.p.*, [1919]

WAR OFFICE. Signals Experimental Establishment. Technical report. December, 1920. [*In Ms.*]
fol. [*Woolwich*, 1920]

WOOD, L. E. A practical treatise on three-phase induction motors.
sm. 8vo. 134 pp. *London*, 1921

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(D) indicates a reference to a discussion upon a paper or address of which the general title or subject is quoted.
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